The American Political Science Review

Articles and Essays by Richard K. Dagger, Peter J. Steinberger, John H. Aldrich and Richard D. McKelvey, M. Kent Jennings and Gregory B. Markus, Bryan D. Jones, Saadia Greenberg, Clifford Kaufman, and Joseph Drew, John A. Ferejohn, Morris P. Fiorina, James H. Qualls, James David Barber, John V. Gillespie, Dina A. Zinnes, Philip A. Schrodt, G.S. Tahim, and R. Michael Rubison, Charles D. Cary, John M. Bacheller, David Adamany, Edward R. Tufte

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Thought Reform and Cultural Revolution

James MacGregor Burns
APSA Presidential Address: Wellsprings of Political Leadership

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Published Quarterly by

The American Political Science Association

Vol. LXXI March 1977 No. 1

up-to-the-minute

By Kenneth Prewitt
& Sidney Verba

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The American Political Science Review

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Office of publication: Curtis Reed Plaza, Menasha, Wisconsin.
Foreign Agent: P. S. King and Staples, Ltd., Great Smith Street, Westminster, London.
Second class postage paid at Washington, D.C., and at additional mailing offices.
Printed in the United States of America by George Banta Company, Inc., Menasha, Wisconsin.
Composition by TypoGraphics, Columbia, Maryland.
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ARTICLES

11 **The Selfish Voter Paradox and the Thrown-Away Vote Argument.** The probability that an individual's voting in a presidential election will determine the outcome being negligible, it is argued that participation is irrational if predicated on principles that are either egocentric or act-prospective. Voter participation, if rational, must rely on some over-arching principle that is (a) Sociotropic, (b) Axionomic, (c) Collective-distributive, and (d) Neutrofactual. A distinctively *ethical* component must be involved, such that all purely "economic," "cost-benefit" models postulating selfish voter rationality are incoherent. The notion of "helping" to elect one's candidate is criticized and rejected unless formulated in a special way. An important pragmatic consequence of the analysis is that the idea (relied on by the two major parties) of "wasting one's vote" on a third party candidate is shown to be invalid or of more limited application than generally assumed. If a sizeable minority (e.g., college students) were educated to reject that argument, politics might be profoundly affected.

By PAUL E. MEEHL, Regents' Professor of Psychology, University of Minnesota Medical School.

31 **From Confusion to Confusion: Issues and the American Voter (1956–1972).** The authors of the *American Voter* concluded that the distribution of opinions on current issues was not very important for explaining the vote of the large bulk of the American electorate. Recent studies purporting to demonstrate the increasing prevalence of issue voting in the 1960s and early 1970s fail to present evidence to satisfy the criteria for issue voting upon which the conclusions of the *American Voter* were based. Worse yet, the evidence of these newer studies fails to satisfy even the studies' own alternative criteria for issue voting. The apparent "increases" in issue voting prove to be largely artifacts of the measures employed or misinterpretations of the evidence adduced. When it comes to translating his issue preferences into voting choices, the average voter remains as confused as ever.

By MICHAEL MARGOLIS, Associate Professor of Political Science, University of Pittsburgh.

44 **Presidential Popularity and Negative Voting: An Alternative Explanation of the Midterm Congressional Decline of the President's Party.** Midterm congressional elections have been generally viewed as relatively sterile affairs marked by reduced turnout, party voting, and the play of politically idiosyncratic forces such as friends-and-neighbors voting. The usual reduction in the number of seats controlled by the President's party, according to the "surge and decline" thesis, simply reflects the departure of short-term forces which presumably benefited the president's party two years earlier. In this study an alternative thesis is proposed which considers midterm election outcomes within the context of the current political environment. Evaluations of the President's performance are found to be directly associated with congressional preferences over a series of midterm elections from 1946 through 1966. Moreover, controlling for party identification, persons who disapprove of the President's performance were generally more likely to vote and to cast their ballot against the President's party than were his admirers to support it. This "negative voting" bias helps to explain why the Democratic and Republican parties have performed more poorly in those midterm elections during which they occupy the White House.

By SAMUEL KERNELL, Assistant Professor of Political Science, University of Minnesota.

67 **Thought Reform and Cultural Revolution: An Analysis of the Symbolism of Chinese Polemics.** Although the major purpose of the Cultural Revolution was to transform Chinese political culture, the way in which this transformation took place has remained unclear. This paper attempts to understand cultural transformation as a process of interaction within a semiological system, consisting of a network of communicators and a lexicon of political symbols. The pragmatic aspect of this process is the outcome of an interplay among the intentions of the elites, the masses, and the target of criticism: political circumstances during the Cultural Revolution were more benign to the cathartic and hortatory intentions of the masses and elites than to the expiatory needs of the target. The syntactic aspect of the system concerns the relationship among symbols: These were found to form a dichotomous structure divided by a taboo barrier, which elicited strong but ambivalent desires to achieve a revolutionary breakthrough. The semantic aspect of the symbolism refers to problematic dimensions of experience in Chinese political culture—the psychological repression imposed by a system of rigid social censorship, the political discrimination practiced against certain social categories, the persistence of differences in income or educational achievement in a socialist system—and suggests that these "contradictions" may be resolved by bold frontal assault.

The symbolism of Cultural Revolution polemics has now become part of Chinese political culture. Its impact seems to have been to inhibit social differentiation (particularly hierarchical), to encourage greater mass participation, and to foster more frequent and irreconcilable conflict among elites.

By LOWELL DITTMER, Assistant Professor of Political Science, State University of New York at Buffalo.

86 **What Is Political Obligation?** Political philosophers have long been exercised by the problem of political obligation. Many have tried to solve it, and others, more recently, have dismissed it as a pseudo-problem

which cannot be solved and need not be posed. This essay is an attempt to clarify the problem of political obligation and to see why it is a problem. My argument, briefly, is that the traditional understanding of the problem is misleading because it fails to distinguish questions of obedience from questions of obligation. When it is properly stated the problem can be solved–in principle, at least–and I try to show what form a solution will take.

By RICHARD K. DAGGER, Assistant Professor of Political Science, Arizona State University.

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95 **Hegel as a Social Scientist.** The purposes of this essay are (1) to identify Hegel's role in the propagation of certain philosophic principles essential to the development of an interpretive social science, (2) to demonstrate in what ways Hegel himself can be understood as an early sociologist, and (3) to indicate those aspects of Hegel's thought that might be of greatest use to contemporary philosophers of social and political inquiry.

The first part of the exposition relies on the Preface to *The Phenomenology of Mind* in order to outline Hegel's epistemological and methodological recommendations. The second part demonstrates the practical meaning of these recommendations by looking at the analysis of ancient Greece found in the *Philosophy of History*. It is concluded that this analysis is indeed a social scientific analysis and, moreover, is suggestive of subsequent work by such interpretivists as Weber, Schutz, and G. H. Mead.

By PETER J. STEINBERGER, Assistant Professor of Political Science, University of Denver.

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111 **A Method of Scaling with Applications to the 1968 and 1972 Presidential Elections.** A method of scaling is proposed to estimate the positions of candidates and voters on a common issue dimension. The scaling model assumes that candidates occupy true positions in an issue space and that individual level perceptual data arise from this in a two step process. The first step consists of a stochastic component, satisfying the standard Gauss Markov assumptions, which reflects true misperception. The second step consists of a linear distortion which is introduced in the survey situation. Estimates of the parameters of the model are developed by applying the least squares criterion, and distributions of the estimates are investigated by Monte Carlo methods.

The scaling technique is applied to the seven-point issue scales asked in the 1968 and 1972 SRC survey. The resulting ideal point estimates are related to candidate positions in 1968 to test a simple Downsian voting model.

By JOHN H. ALDRICH, Assistant Professor of Political Science, Michigan State University, and RICHARD D. MCKELVEY, Assistant Professor of Political Science, Carnegie-Mellon University.

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131 **The Effect of Military Service on Political Attitudes: A Panel Study.** Institutional experiences at the young adult stage may act to alter or reinforce pre-existing political attitudes. This paper focuses on military service during the Vietnam War as one such institutional experience. Data are drawn from a two-wave, 1965–1973 national panel study of 674 males. Approximately half of the panel saw active duty. Comparisons are made between civilian and military respondents and, among military respondents, according to the duration, recency, intensity, affective qualities, and institutional salience of the military experience. Bivariate and regression techniques are used in the analyses.

Veterans are somewhat less cynical and have broader attention frames than civilians. Civic tolerance and feelings toward minority groups are also affected by aspects of military service. War-related opinions vary between civilians and veterans and across categories of veterans. These effects remain when other relevant factors, including prior attitudes, are taken into account. The results underscore the potential significance of adult experiences in the socialization process.

By M. KENT JENNINGS, Professor of Political Science and Program Director, Center for Political Studies, University of Michigan, and GREGORY B. MARKUS, Assistant Professor of Political Science and Assistant Research Scientist, Center for Political Studies, University of Michigan.

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148 **Bureaucratic Response to Citizen-Initiated Contacts: Environmental Enforcement in Detroit.** When citizens contact local government agencies, they generally attempt to influence service delivery decisions made by these bureaucracies. This paper examines the nature of citizen contacts, and the results of such contacts, with respect to the enforcement of environmental ordinances in Detroit, Michigan. We first examine the mechanisms responsible for the generation of citizen contacts. Assuming relations among citizen awareness, service need, and social well-being, we derive a downward-opening parabola as appropriate for describing the relationship between social well-being and propensity to contact a service agency. Using data on citizen contacts from City of Detroit agencies merged with census data, we find the expected relationship in evidence. We find that the Environmental Enforcement Division generally responds to citizen contacts, but the quality of the response varies with social characteristics of neighborhoods.

By BRYAN D. JONES, Associate Professor of Political Science, Wayne State University; SAADIA R. GREENBERG, Social Science Analyst, Administration on Aging, Washington, D.C.; CLIFFORD KAUF-

MAN, Associate Professor of Political Science, Wayne State University; and JOSEPH DREW, Ph.D. Candidate, Department of Political Science, Wayne State University.

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166 **On the Decline of Competition in Congressional Elections.** Several authors have observed a decline in the number of competitive congressional districts during the past two decades. Various explanations have been proposed for this change. Among these are theories attributing major causal significance to changing methods of drawing district boundaries, and increasing control of campaign resources by incumbents. These theories are examined critically and arguments are advanced for their rejection. The principal cause of the decline of competition for congressional seats appears to rest on a change in individual voting behavior.

By JOHN A. FEREJOHN, Associate Professor of Political Science, California Institute of Technology.

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177 **The Case of the Vanishing Marginals: The Bureaucracy Did It.** Several authors have addressed the postwar decline of electoral competition on the congressional level. Some have attributed the decline to institutional change such as the redistrictings of the 1960s. Others have remarked on the growing use of the growing resources of incumbency. Still others, like Ferejohn, have focused on behavioral change in the larger electoral system, such as the erosion of party identification. In this comment I suggest that while electoral behavior has changed, the change is at least in part a response to changing congressional behavior, which in turn is a reaction to institutional change for which Congress is partly responsible. Specifically, over time congressmen have placed increasing emphasis on district services: more and more they operate as and are perceived as ombudsmen rather than as national policymakers. This behavioral change is an understandable response to an expanding federal role and an increasing involvement of the federal bureaucracy in the lives of ordinary citizens, an institutional change Congress has helped to bring about.

By MORRIS P. FIORINA, Associate Professor of Political Science, California Institute of Technology.

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182 **Barber's Typological Analysis of Political Leaders.** James Barber's predictive theory of presidential behavior has evoked varied reactions, which have ranged from praise for its sensitization of readers to the fact that personality affects presidential performance, to criticisms for the emphasis that the theory places on personality, to questions about the validity of the theory. This article addresses itself to the criticisms and the questions.

Concerning the questions, it shows, first, that in analyzing presidents, Barber assumes the validity of "character"–the core construct of the theory. It shows, second, that Barber's earlier research on Connecticut legislators, from which "character" derives, does not empirically establish the construct.

Concerning the criticisms, the article isolates a possible origin of the psychological reductionism evident in Barber's explanations of presidential performance. The article identifies a similar reductionism in Barber's legislative research and attributes this reductionism to a fallacious extra-empirical argument.

By JAMES H. QUALLS, Ph.D. Candidate in Political Science, The Johns Hopkins University.

Comment. By JAMES DAVID BARBER, Professor of Political Science, Duke University.

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226 **An Optimal Control Model of Arms Races.** Lewis Frye Richardson's simple differential equations model of armaments races has been long criticized for its lack of incorporation of the goals of nations. Using the mathematics of optimal control theory, the authors formulate a model which incorporates national goals into an "arms balance" objective function. The goals used are based on the traditional concerns in the balance-of-power literature. From an objective function together with the Richardson model an optimal armaments policy is derived. The United States-Soviet, NATO-WTO, and Arab-Israeli arms races are used as empirical examples, and the parameters in the model are estimated by means of functional minimization techniques. The optimal control model is further examined for its equilibrium and stability properties. The equilibrium and stability conditions are assessed with respect to the empirical examples. The findings are that while the United States and the Soviet Union in direct confrontation pursue strategies that lead to a lack of equilibrium and stability, when taken as part of NATO and WTO, the major powers and their alliance partners do pursue stable and equilibrium strategies. The Israeli policy is found to lead to equilibrium and stability while the Arab policy does not.

By JOHN V. GILLESPIE, Associate Professor of Political Science, Indiana University; DINA A. ZINNES, Professor of Political Science, Indiana University; PHILIP A. SCHRODT, Assistant Professor of Political Science, Northwestern University; G. S. TAHIM, Research Associate in International Policy Studies, Indiana University; and R. MICHAEL RUBISON, Assistant Professor of Statistics, Kansas State University.

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245 **A Technique of Computer Content Analysis of Transliterated Russian Language Textual Materials: A Research Note.** The purpose of this note is to describe a technique of computer content analysis of transliterated Russian language textual materials and to illustrate the use of the technique with two examples of my research on the political socialization of Soviet schoolchildren.

By CHARLES D. CARY, Stockton, California.

252 **Lobbyists and the Legislative Process: The Impact of Environmental Constraints.** In this study of lobbyists' techniques and perceptions of the legislative process, the impact of several aspects of the legislative environment is examined, including group competition and the use of issues by candidates in presidential campaigns. Data from a sample of lobbyists indicates that nonconflictual issues are treated differently by lobbyists and the Congress from issues involving group conflict. Similarly, issues used in presidential campaigns are treated differently from those developed outside them. Finally, group size is shown to be a further influence on lobbyists' techniques in dealing with Congress.

By JOHN M. BACHELLER, Assistant Professor of Political Science, Kirkland College.

APSA Presidential Address

265 **Introduction**

By HAROLD D. LASSWELL, Ford Foundation Professor Emeritus of Law and the Social Sciences, Yale University Law School.

266 **Wellsprings of Political Leadership.** To develop a general theory of leadership we must locate the function of political leadership in a theory of historical causation. One may begin by identifying sources of leadership in the wants and needs, aspirations and expectations, of humankind. In helping to gratify these "motive-bases," leaders move followers "up" the hierarchy of needs and other motivations and thus create new social configurations in which leaders act. As persons—especially children—move "up" through stages of morality, they also create structures of values that both empower and constrain would-be leaders. Leadership over human beings is exercised when would-be leaders, possessing certain motives of their own, mobilize their own psychological, institutional, political, and other resources relevant to potential followers' motive bases in such a way as to satisfy the motives of both leaders and followers. The test of leadership is the achievement of goals of both leaders and followers in a context of open conflict over ends and means, with leaders and followers mutually and freely defining their values and purposes.

By JAMES MACGREGOR BURNS, Woodrow Wilson Professor of Government, Williams College.

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By DAVID ADAMANY, Associate Professor of Political Science, University of Wisconsin-Madison.

305 **Political Statistics for the United States: Observations on Some Major Data Sources.** Thirteen major data sources for the study of American politics are examined with regard to their conceptual orientation, error structure, and inferential utility. A great deal of ephemera and measurement without theory is discovered. Few of the documents contain any serious discussion of error structure, although some do report "standard errors" based on naive sampling models. In addition to suggestions for improving the compilation of political statistics, recommendations for a basic minimum library of data sources for American politics are made: *The Almanac of American Politics* and the *Statistical Abstract of the United States,* followed by the *Guide to U.S. Elections.*

By EDWARD R. TUFTE, Professor of Public Affairs, Princeton University.

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The Selfish Voter Paradox and the Thrown-Away Vote Argument*

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During the last quarter century political scientists have become increasingly preoccupied with rational reconstructions of voter behavior, the leading ideas being taken from economics. Some theorists explicitly postulate voter selfishness[1]; others avoid this postulate in favor of whatever utilities (societal goods included) the voter wishes to increase,[2] although it is notable that the examples provided tend overwhelmingly to be egocentric, and usually monetary. Recently there has been growing an uncomfortable awareness that such "economic" theories have major trouble dealing with a big brute fact, to wit, that the empirical probability of an individual voter's behavior determining the outcome of a large scale (e.g., U.S. presidential) election is negligible. Riker and Ordeshook[3] estimate it as $p = 10^{-8}$, and my rough calculations indicate my chances of determining who becomes President are of about same order of magnitude as my chances of being killed driving to the polls—hardly a profitable venture. Precise estimates are neither possible nor necessary, since any fairly computed value surely lies below the $p \leqslant .0001$ whose complement $q = .9999$ Bernoulli[4] and Buffon[5], and all reasonable men, regularly treat as a "moral certainty."

I shall argue from these rather simple and noncontroversial truths that all economic theories of voter participation are radically incoherent, because such participation is irrational as an instrumental action toward an egocentric end. A voter's participation in large scale elections is inherently a case of the "Tragedy of the Commons,"[6] and no manipulation or complexification of the economist's cost-benefit formalism can make it otherwise. As a corollary, I shall try further to show that the *kinds* of justificatory premises required to render voter participation rational are distinctively moral and such that they will also justify voting for a "sure loser" candidate, thus rebutting the standard argument, made by supporters of the two major U.S. political parties, that voting for a third "minor" party is "throwing away your vote."

I think the argument can best be set forth in the form of a dialogue. One protagonist, "S.O.P." (= Standard Old Party), believes it to be irrational to vote for any presidential candidate who is neither a Republican or Democrat, since voting for anybody else is "merely wasting your ballot." The other protagonist, "F.E.V." (= Flat Earth Vegetarian), proposes to vote for the candidate of a fusion party composed of Flat Earthers and Vegetarians, recently united. Voter F.E.V. has become convinced that the troubles of the world are largely attributable to human beings engaging in the unnatural and wicked practice of consuming flesh foods, along with their adherence to the Magellanite heresy concerning the spheric-

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*I wish to thank Dean Carl A. Auerbach (Law), Professor David Cooperman (Sociology) and Professor Rolf Sartorius (Philosophy) and three anonymous reviewers for their critical comments. A nondialogue presentation of this paper's argument, substantially equivalent but with some additional material included, is available from the author as "Difficulties with economic models of voter behavior," *Reports from the Research Laboratories of the Department of Psychiatry, University of Minnesota,* Report no. PR-76-1 (Minneapolis: University of Minnesota, 1976).

[1] Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957).

[2] James M. Buchanan and Gordon Tullock, *The Calculus of Consent* (Ann Arbor: University of Michigan Press, 1971). The present paper was drafted more than three years ago, and I am aware of the spate of articles meanwhile (1972–1976) devoted to aspects of the "voter calculus" problem. I have not discussed or even cited these recent contributions, because despite their merits and general relevance they do not bear appreciably on the distinctively *ethical* core of my argument. If I am right that no egocentric act-prospective rationale for voter participation will do, then elaborations or amendments of Buchanan and Tullock or Downs will not fix things up. Nor will further manipulations of the formalism. Thus to take one example, Ferejohn and Fiorina's paper on "maximin regret" (*American Political Science Review, 68* [September, 1974], 525–536). is an interesting and ingenious variation on the received model, but I think the reader will agree that it leaves my philosophical criticism quite untouched.

[3] William H. Riker and Peter C. Ordeshook, "A Theory of the Calculus of Voting," *American Political Science Review,* 62 (March, 1968), 25–42 at p. 25.

[4] Ian Hacking, "Jacques Bernoulli's *Art of Conjecturing," British Journal for the Philosophy of Science,* 22 (August, 1971), 209–229 at p. 219.

[5] D. A. Gillies, *An Objective Theory of Probability* (London: Methuen, 1973) at p. 165.

[6] Garrett Hardin, "The Tragedy of the Commons," *Science,* 162 (1968), 1243–1248.

ity of the earth. The two disputants have agreed to disagree on the merits of these substantive views, and the dialogue is concerned solely with the question whether it is irrational of F.E.V. to cast his vote for the fusion party candidate, one Pastor Sedlitz, instead of taking the Democratic or Republican candidate as the best available according to his lights.

SOP: Of course I consider your views about the wickedness of eating meat and the earth's flatness as erroneous and, to put it bluntly, preposterous, but it seems clear from our discussions that you are set in your Flat Earth-Vegetarian ways, so I am not going to debate the merits further. However, our discussions have also shown that there are some features of your position that happen to be the same as those of the platform of the Republican party, and of individual statements by Senator Claghorn who is running as the Republican presidential candidate; and there are a few features of your views, although somewhat fewer, to be found in the position taken by the Democratic party and its presidential candidate, Governor Fisbee. I have not myself decided whether to cast my vote for the Republican or the Democrat, but I am certainly going to vote for one of the two; and I want to try to persuade you that it is pointless, even assuming the substantial correctness of your Flat Earth-Vegetarian views, for you to vote for the FEV candidate, Pastor Sedlitz. My argument is short and easy and does not require any big philosophical or political theory as underpinning. It consists simply in pointing out that if you vote for Pastor Sedlitz, even assuming that his FEV views are mainly correct, you will be throwing away your vote.

FEV: I don't understand what you mean by "throwing away my vote." I am voting for the candidate of my choice. Isn't that what I'm supposed to do in a democracy?

SOP: Well, yes, within reasonable limits I suppose one could say that. But the point is that you know right now that Pastor Sedlitz is going to lose. It's not absolutely *certain* that he'll lose, but it's quasi-certain—as certain as we ever can be about human affairs; as certain as we are normally required to be in our daily practical decision making. So it's like dropping your penny into the bottomless pit: you are going through a meaningless gesture of democracy, so to speak, because it is foreseeable with quasi certainty that your candidate will lose, whether you vote for him or not.

FEV: Yes, I concede it is foreseeable with quasi certainty that Pastor Sedlitz and the Flat Earth-Vegetarian party will lose. That doesn't bother me a bit. But I am curious to hear why you propose to vote for Senator Claghorn, or Governor Fisbee, as the case may be.

SOP: Well, you see, I don't know which one of them is going to win, but one of them surely will. So my voting has some point to it; it's not a pointless effort or an empty gesture.

FEV: I don't see how that follows. If I understand you, your argument against my proposed vote for Flat Earth-Vegetarian candidate Pastor Sedlitz is that my voting for him cannot lead to his being elected. But your voting for Senator Claghorn cannot lead to his being elected either, can it?

SOP: It's not that my individual ballot determines Senator Claghorn's election, which of course we know it will not, with a probability of .99999 or better. The thing is, I am helping to bring about an event which, at least from our present condition of information, may possibly be brought about, if I and others like me get together. If I and others like me all vote for Claghorn, he will be elected, otherwise not.

FEV: I can't buy that last statement if I take it literally. You formulate it as a conditional that Senator Claghorn will win the presidency if ". . . you and others like you get together." But that's not literally correct. It is not necessary that *you and others like you* should vote for him. All that is necessary is that enough people, which group may or may not include you, vote for him. From which it surely doesn't follow that *you* must vote for him to bring about the intended result.

SOP: I mean that we have to vote for Senator Claghorn collectively, of course.

FEV: Why does it follow, from the statement that "we have to vote for him collectively" that "I, who desire his election, must vote for him individually"? I had thought that arguing from something that is true collectively to something true for the individuals in the collection was a fallacy, called the fallacy of division. They taught me that in Logic I when I was a freshman. Besides, you persist in saying "we," and you aren't entitled to say "we," if the word *we* is taken to include *you,* the particular individual with whom I am disputing. It is simply not correct to assert that anything hinges upon a specified subset of the electorate. That a subset which happens to include you as one member must vote for Claghorn in order for him to win, is literally just plain false. All that can be said to be literally correct is that a sufficiently large subset of the electorate (to be precise, a larger subset than those who vote for his opponent) must vote for him. It is surely obvious that this hypothetical subset does not need to include you, or me, or any other specific individual. An assertion about a *speci-*

fied subset (e.g., one that includes you) is not even true if stated probabilistically—let alone as a solid-gold conditional.

SOP: Let me make an analogy to economics. Nobody's individual purchase, in classical economic theory, is considered to have an appreciable influence upon the price of hamburger. Nevertheless classical economics also says that these collective choices by all the purchasers who would prefer eating hamburger to keeping their 50c (or buying something else with their 50c) are what determine the supply and the demand and the price of hamburgers. As a rational economic man, I help determine what happens to the price of hamburgers, the supply of money, the number of hamburgers produced, and things of that sort by my individual action as a member of the collective; the same is true of the political process, is it not?

FEV: No, it is not. The rational man in economics buys a hamburger because he wants a hamburger more than he wants his 50c or something else that he could get with his 50c instead of a hamburger. He doesn't buy the hamburger *in order to* "influence" the macroeconomic process or state. He buys it because he wants something that he gets if he buys and lacks if he doesn't buy. The point is that if he as an individual didn't buy the hamburger, then he wouldn't have a hamburger; and he wants one. Whereas if you as an individual don't vote for Senator Claghorn, that does not determine whether you get Senator Claghorn or whether I get him. Almost all analogies between political and economic behavior are fallacious because, for politics in the large, where it can be said on empirical and mathematical grounds that a rational person knows with a "moral certainty" that his individual action does not substantially influence the probabilities of what befalls him (or, for that matter, anybody else, or the collective), he is *not at all* in the position of the vendor or purchaser postulated in economic theory. Incidentally, while the paradoxes of political participation as interest groups or individuals have become the focus of attention in recent years among political theorists,[7] you are surely aware that similar paradoxes were raised many years earlier (long before the advent of modern decision theory or the classic formal incoherence proofs of Arrow and Co.), notably in the well-known essay on the "tragedy of the commons" by William Forster Lloyd, a paper published almost a century and a half ago.[8] So the question also arises in economics; it's just that in economics it is not always critically present but depends on the circumstances; whereas in large scale political democracy, I claim that it is always present.

SOP: Consider what kind of consequences your mode of reasoning would result in if everybody followed it. How would *you* get anybody to the polls in a political democracy?

FEV: That's the point, precisely. *You can't get anybody to go "rationally" to the polls,* unless you introduce some sort of quasi-Kantian principle with a distinctly *ethical* content. Political participation theories that omit "ethical" premises are all radically defective for this reason.

SOP: I'm going to get plenty nervous if we have to go back to old Immanuel Kant to explain political rationality! I doubt very much that even you want to buy Kant, or do you?

FEV: No, I don't buy Kant. I don't even think Kant's own examples, when he gets to illustrating the Categorical Imperative, are either rationally compelling or intuitively punchy. At most, his example about paying debts has some plausibility, although I think I could even take care of that one. The business about suicide, or only looking out for my own interests as long as I don't harm others, or developing my talents versus loafing—all seem to me singularly feeble both in their argumentative structure and their intuitive appeal. (I think they testify mainly to the fact that Kant's background was pietist in its ethos, not that I wish to commit the genetic fallacy or argue *ad hominem.)* That's why I said "*quasi*-Kantian." What I mean is that you can't get me or yourself or anybody else to the polls in a national presidential election if you appeal to their selfish interests. And, worse still, you can't even get us there by an appeal to altruism if you have an act-prospective formal principle of conduct.

SOP: I'm not sure just what you mean by the term *act-prospective.* You better spell that out if you intend to rely upon it.

FEV: I am not prepared to give an absolutely rigorous definition of it. Roughly, I mean by an "act-prospective" formal principle of conduct one that, however stated, essentially makes the rationality of a proposed (dated, particular) action a_i dependent upon foreseeable differential consequences of individual *i*'s taking that action. One must show that there is a substantial difference in the probability distribution over consequences if I, the agent contemplating the action, take the action from what the probabilities would be if I failed to do so. I don't want to speak of an "act-utilitarian" criterion because it seems so obvious to me that one might follow a formally act-prospective rule of conduct without a wholly utilitarian content. Thus, for instance, I might vote in a small faculty meeting in a certain way, not with the aim of maximizing the utilities, but in order to "do justice" as defined in some other than a purely utilitarian way. Such a vote would be rationally act-prospective for me as a means to the "Justice" end because the number of people voting in a faculty meeting is small enough so that my single vote might possibly (even probably, if the faculty is small enough and seems divided closely) make a difference. In fact, for a faculty meeting, even a "lost" vote wouldn't necessarily make it pointless of me to have cast my single vote, inasmuch as how badly somebody loses in a group of a dozen persons may make a significant psychological or political difference now or in the long run.

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[7] See, e.g., Mancur Olson, Jr., *The Logic of Collective Action* (New York: Schocken Books, rev. ed. 1971).

[8] Reprinted (in part) in *Population, Evolution, and Birth Control,* ed. Garrett Hardin (San Francisco: Freeman, 1964), p. 37.

SOP: All right, that's not a very precise explanation, but perhaps it's good enough for our present purposes. Now, how do you propose to use this notion?

FEV: What I argue is that in order for me to go to the polls, or for you to go to the polls—it doesn't matter whether we are Republicans or Democrats or Flat Earth-Vegetarians in our convictions politically—in a presidential election, assuming the premise (which you don't dispute) that there is a moral certainty that neither your vote nor mine determines the outcome, it is necessary to invoke some kind of ethical principle of political participation that is not act-prospective and not egocentric. A principle which is act-prospective won't get me there whether I'm egocentric or not, because there is a moral certainty that whether I vote or do not vote will make no difference in the outcome—even in the "appearance" of a landslide victory or a narrow squeak. Nobody is going to pay any attention to the last digit in a six place number for the state of Minnesota's popular vote for the President, especially as we all know that the voting machines and tabulations will contribute error larger than that. So there must be invoked a principle which is not purely act-prospective, a principle stating some kind of rule that bars direct appeal to probable differential prospects hinging upon my acting versus not acting. No direct appeal to utility can get me to the polls because of the quasi certainty, by any ordinary reliance on probability reasoning in human life, that my vote is without effect, *either* in the sense of determining the outcome or of making the outcome "look stronger." Since you don't like the term "Kantian," even when modified by "quasi", let me characterize the kind of principle required as *axionomic*. I mean by "axionomic" a rule or principle—whatever else its formal properties or content—which has a form that does not confine its binding character to cases where act-prospective calculations make it reasonable to say that a certain outcome hinges, or its probability depends non-negligibly, upon what I do. And now I add a further point that even an axionomic principle will not suffice unless it is to some degree *altruistic*.

SOP: Oho, that will get you in bad trouble. If each voter is supposedly voting so as to benefit others at his own expense, we get into an awful mess, both philosophically and econometrically. Altruism will never work as a basis for voting. Surely you can't mean that Citizens A and B should, for example, each be voting high taxes for himself so as to provide transfer payments to the other? That's absurd, and it just won't work.

FEV: Ah, yes, you're quite right. I should not say altruistic, but something weaker, say *sociotropic*.

SOP: You seem to need quite a few fancy terms to defend your position. What does this neologism mean?

FEV: By *sociotropic* I mean taking some account—we needn't say exactly how much—of other persons' interests or, if you like, of the collective's interest. An axionomic egocentric principle would be a maxim of prudence, which tells me to behave rule-conformingly in a way that *tends* to pay off and bars direct appeal to act-prospective consequences for my own welfare. It bars such direct act-prospective calculations on the usual grounds of uncertainty in human affairs, bias in one's reasoning about complex matters, self-deception and wishful thinking, and the fact that there are some risks which I do not dare allow myself to take even though the probabilities associated with them may be relatively small. Thus we often temper a straight "maximize-expectations" decision rule by a rule which first excludes a class of too-large risks, either by disjunctive characterization (intensional) or, failing that, by actually listing them (extensional). This is not a maximin decision policy but a step-function rule superimposed upon an old-fashioned expectation-maximizing rule. It resembles what in assessment psychometrics we call a "mixed regression and successive hurdles" model. Now the point is that it cannot conceivably be a maxim of prudence for me to vote in any presidential election because the act-prospective probabilities are always negligible; but we know with quasi certainty that I will *lose* certain utilities (such as having a pleasant evening watching the baseball game or making love or drinking beer or playing pinochle). Adding the fact that there is a small but not zero probability that I will get killed driving to and from the polling place—one that has almost the same order of magnitude as the probability that I determine the outcome of the election—clearly a maxim of prudence would lead me to stay at home. Therefore we need something more than an axionomic principle, we need an axionomic principle which is also sociotropic. We need a principle which speaks of my obligations to the rest of the collective, which pays attention to somebody's interest besides my own, and which formulates a *rule* about my conduct that has that sort of reference. If there's not a rule, I should not bother even on altruistic premises; if there is a rule but it's a prudential (egocentric) one, I should not bother either. What I need is both.

SOP: It disturbs me that you are speaking in such general terms rather than actually formulating the rule. If you have the rule up your sleeve, why don't you state it for me? It also concerns me that you keep formulating the matter so negatively, in terms of principles that "*won't* suffice to get me to the polls."

FEV: I assure you I am not engaging in any shenanigans in that respect. I am avoiding stating a rule because I'm not at all sure what particular rule you would accept or, for that matter, what rule I would accept. But I do want to maintain that a characterizable disjunctive class of rules is excludable as insufficient. So far as affirmative statements go, an adequate rule could be Kantian, or neo-Kantian, or Marxist, or production-maximizing, or "biological survival," or nationalistic, or Christian Natural Law, or fascist, or rule-utilitarian, or all sorts of other things. (Act-utilitarian it could not be, for the same reason that it cannot be, more generally, purely act-prospective.) I don't want to commit myself to any one of these specific possibilities. And even if I had made up my mind among them, I wouldn't want to have my argument with you hinge upon that choice. Because I claim we can make some strong meta-arguments concerning what *kind* of principle or reasoning is necessary to get an individual citizen to the polls, and that meta-reasoning suffices for the present purpose. It would be undesirable, in this sort of argument, to have the whole outcome hinge upon our mutual acceptance of a particular ethical principle, such as rule-utilitarianism, furthering the proletarian class struggle, conducing to our nation's survival and power, evolving a better human species, or enabling each human personality to fulfill his potential. I want to say that my position is more general than any of those. As for my position being negative, that's for the same reason. I really don't know how to characterize the class of adequate rules except that each of them would need to be something stronger than a maxim of prudence or an appeal to probable differential consequences, altruistic or egocentric. What affirmative properties an adequate rule must have I do not know, although it seems to me that saying it must be axionomic rather than act-prospective, and saying further that it must have some social content rather than being purely selfish, says a good deal. I am inclined to think that any altruistic rule of that type will do the job, although it probably would not suffice to determine for which candidate I voted once I got there.

SOP: My understanding of what have been called "economic theories of democracy" is that they do not postulate voter altruism, and I don't see why you think that is necessary for them to work. If anything, these models typically assume voter selfishness.

FEV: When I said altruism, I misspoke. All I require is that the rule or principle should be sociotropic, and that doesn't mean altruistic. "Altruistic" is the *contrary* of "egocentric," not its contradictory. By "sociotropic" I mean that the content of the rule or principle (or its derivation chain) must involve at least some reference to the interests of others than the voter, or let us say more realistically, the voter and his immediate family taken as a political, economic, and social unit.

SOP: Well, the economic theories of democracy don't require even that weakened form of concern for others, in my reading.

FEV: No, I agree that they don't as usually formulated. My point is that they *must* do so in order to be adequate to get me into the polling booth as a rational agent. The crucial difference is that between my buying a hamburger because I want the hamburger (regardless of what my purchase might do to the price or supply of hamburgers) and my going to the polls. We simply can't postulate an egocentric means-end situation when it comes to voting.

SOP: But in economic theory your individual purchase of a hamburger doesn't do anything to the price either, does it?

FEV: No, but the point is that while collectively all these hamburger purchases do affect the supply and the price, I don't have that collective impact in mind, nor need I make any implicit reference whatever to it in defending a claim that the single hamburger purchase is rational on my part.

SOP: I remind you that public appeals are often made by politicians and businessmen that people ought to pay attention to that "collective economic impact" such as in war time or to help control inflation, or in urging people to "buy something, anything" during a period of economic recession.

FEV: Yes, but my position is that such "collective economic" appeals also involve a sociotropic component that is *not* present when I behave rationally as an egocentric economic man and purchase my individual hamburger not because of such collective impact considerations, but because *I want that hamburger more than I want my 50c.* And, as is all too well known, such appeals do not tend to work—hence we have wartime price controls, black markets and the like.

SOP: All right, on this I guess we have no disagreement. But I still don't think there is anything necessarily sociotropic in your sense about the economic theories of democracy. It seems clear to me that they only require something weaker than either altruism or sociotropism, namely, a kind of quasi-contractual principle which says that while I do not need to have somebody else's *interests* than my immediate family's at heart, I do have to carry out my part of an implicit social contract or bargain if I live in a democracy, one of the responsibilities of a citizen in a democracy being to cast his vote. I don't see why being sensible of this obligation has to involve even the faintest whiff of concern for your interests, any more than a businessman who accepts, in carrying on trade, the obligation of contracts must be presumed to have the interest of other businessmen with whom he deals at heart, even the least bit.

FEV: H'mmm—I hadn't thought of it that way. Yes, you have an important point there. All right, I will modify the sociotropic requirement. Since I think this kind of nonegocentric consideration might often play a significant role (because that's how people talk to me at times about their own voting behavior), I would suggest an amended version of the statement about content which would be to say that the content must be either sociotropic or social-contractual. Alternatively, I might revise my definition of *sociotropic* to mean "taking some account of the subjective desires, objective interests, or entitlements of others in my social group." It now occurs to me that I have a problem distinguishing between the form and the content of the kind of rule or principle that would be necessary to get me to the polls in a national election, and the additional elements of the reasoning needed either to *get to the rule* or to *go from the rule* to the implication that one is obliged to vote.

SOP: I don't doubt that you have trouble with that, since you seem reluctant to state the rule, preferring only to talk about it or characterize it vaguely, if I may say so without offense. I mean it *does* make it difficult to argue with you, when you operate so much in the *meta*-meta-ethical clouds.

FEV: Well, you've objected to that generality or vagueness before, and I sympathize with your unhappiness, but the point about it is I would not want to state the actual rule, even if I could do so with precision, because I want to deal with a *class* or *family* of possible rules which are disjunctively necessary to make my voting a rational act, and I honestly don't want to defend one of these rules rather than another. I admit that this causes both of us trouble in the argument. You as critic are disadvantaged by not knowing exactly what rule you should be examining, and I am disadvantaged in providing grounds by not knowing exactly what rule or principle is to be inferred. But it can't be helped if my position is to be presented in the generality that I intend. I repeat that I do not want to be stuck defending rule utilitarianism or Marxist ethics or nationalist values or biological survivalism or Rawlsian lexical justice or any other particular set of political and moral principles. So I have to do the best I can with a general formulation. Of course this has the kind of weakness that any position does which talks about "what sort of things would be necessary in order to derive such and such a consequence." After all, we must remember that even in formal logic and mathematics, and *a fortiori* in any empirical domain or ordinary language discourse, statements about what kind of thing would be needed to get somewhere from somewhere else are necessarily only persuasive and cannot claim deductive rigor. But I take it we would agree that deductive rigor cannot be had in discussing political science anyhow, right?

SOP: Right, no quarrel on that score. I don't mean to fault you for this generality, even if I sound as though I'm doing that; I just think you ought to face up to the fact that by not stating the rule, you are bound to lose a certain amount of "punch" when it comes to persuading a reluctant old party supporter like myself. But back to the debate, what did you have in mind by your remark that it's difficult to separate the rule or principle involved from the antecedent reasoning and the reasoning from the rule to the consequence "I should vote"?

FEV: Something like this. Whether we make the rule "strongly" sociotropic by postulating an altruistic interest in somebody else's welfare besides my own, or "weakly" sociotropic, covering altruism, benevolence, and a non-benevolent social-contractual concept which merely requires that by sharing in the benefits of a democratic polity I am prima facie obliged to participate in it as a voter (some scholars would of course require more than that, but I think that's hard to show), in either case we need some additional reasoning first to show this and then to apply it. We have in the back of our minds some reference to what we want to happen (e.g., more prosperity and freedom and health and education and security and all of that) and, equally importantly, certain things that we want to avoid, such as the collapse of the political order, or a ruinous inflation or depression, or the rise of a tyranny or the stunting of human growth potentialities, not to mention a thermonuclear holocaust. Here again I don't know how to state it except in general terms but, speaking meta-talk about what (a) the rule itself, or (b) the reasoning to the rule, or (c) applying the rule, would have to include, I would take it as fairly obvious that it would require some kind of reference to an empirical means-end relation, and also some kind of conditional (allowably but not necessarily counterfactual) about what everybody has to do in order to make the thing work.

SOP: Aha–I thought you rejected the line of argument about what everybody has to do, as for instance the difference between the economic situation and the tug of war situation. In the tug of war situation, everyone on one side must help, otherwise that side is quasi-certain to lose. In the hamburger situation, and I thought you wanted to argue in the presidential election situation, it is literally false that everybody must do anything in order for such and such a result to accrue.

FEV: Correction sustained–I should have said, that in (a) reasoning to the rule and (b) applying the rule in order to get me to the polls and, I would also assume, (c) to determine my voting choices once I'm in the polling booth, we will have to say something about *collective action*. What we say will have to be a statement true collectively (i.e., "statistically", about the collective) and not, as I said before and as you now remind me, distributively. It is literally false to say that "everybody has to participate" in order to make democracy work. That's empirically absurd on the face of it. We have another one of those open concepts[9] where we have to talk about some hard-to-specify minimum amount of participation in order for it to work. It is not necessary if democracy is going to work, for example, that 98 per cent of all eligible voters should participate, but democracy couldn't function on only 2 per cent—although I really don't know how one would go about proving that, do you?

SOP: Well, of course, if participation were that low, we would probably infer that something else of a fundamental character had already taken place so that in some sense democracy must already have ceased to function in a significant way, right?

FEV: That's right, so we needn't dispute about that one. Anyway, what we need is some kind of counterfactual or conditional statement concerning collective action. In order to get to the rule from some kind of sociotropic or social contractual notion, and in order to apply the rule to the question whether I should bother to vote (and, if so, *for whom*), we are going to need some kind of reference to what would happen if nobody voted or what would be desirable about nearly everybody voting. I submit further that in deciding for whom to vote (once I get there), I would also take the same kind of considerations into account, such as deciding how good or bad it would be if a majority voted for Hitler or Gus Hall or Gerald L. K. Smith or Senator Claghorn or Governor Fisbee or the Flat Earth-Vegetarian candidate.

SOP: I guess I understand why you think you need this, but can't you say more about the steps involved?

FEV: The meta-steps are by negation. First we ask, "What sort of overarching ethico-political reasoning would get me to the polls, assuming I am a rational man?". Then we attempt to characterize such a principle meta-ethically, by saying something general about its form and content. Next we show that whatever we proposed provisionally won't do, because acting on that kind of basis would be irrational. From which we infer that the contradictory meta-predicate must be involved instead, if the desired derivation is to go through.

SOP: Let's hear it in the concrete, or at least as concretely as you can do it operating *meta*, as I guess we are stuck with doing.

FEV: Having agreed that the principle or its derivation has to be axionomic and sociotropic, what else would you advance by way of making my individual participation a reasonable act?

SOP: I suppose I would say something to the effect that if people don't vote, then the system won't work; and, if persons who hold particular views, such as your Flat Earth Vegetarians, don't vote, their candidate won't win; he won't even make a showing that might possibly influence future voters, or the future positions of the two major parties, or his successful opponent's in-office behavior. As you say, it's going to be some kind of a conditional argument about the differential consequences of people voting or not voting.

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[9] Arthur Pap, *Semantics and Necessary Truth* (New Haven: Yale University Press, 1958), chapter 11, "Reduction and Open Concepts," pp. 302–360.

FEV: I'll buy that, but only if you put the conditional carefully, since otherwise it's literally false. How do you put it?

SOP: How's this for a starter? "If people don't vote, democracy will collapse; and, in particular, if we Republicans don't vote, Republican candidate Claghorn will lose."

FEV: Well, maybe the Republican candidate is going to lose anyway.

SOP: He may, but we don't *know* that, before the election. Whereas we *know* before the election that the Flat Earth-Vegetarian candidate is going to lose. That seems to me a very important difference.

FEV: I confess it has a certain intuitive punch, even to me. But I must insist that from my position it is nevertheless irrational, when properly analyzed. Remember, we are talking about you, the individual Republican, and about me, the individual Flat Earth Vegetarian. We are trying to make clear how it could be rational for *you,* an individual, to vote for the Republican candidate, whereupon I am going to try to show that whatever reasoning will suffice to make it rational for you to vote for Senator Claghorn will suffice to make it rational for me to vote for the Flat Earth-Vegetarian candidate, Pastor Sedlitz.

SOP: Maybe there is a hang up here on what it means to be politically rational. Do you suppose that could be possible?

FEV: I would hate to be stuck with having to provide a generic definition of political rationality, but if you think that's a lurking problem in our discussion, I suppose I must make a stab at it. For purposes of the voting situation, remember that we are talking about an individual political agent, in particular, an individual who is a prospective voter or campaign fund contributor or automobile bumper sticker displayer. I would say that individual act a_i is politically rational for citizen i if it meets either of two conditions: (1) it is foreseeably probably efficacious, by which I mean that thinking about the empirical facts (e.g., the social order, the rules of the game, the Gallup Polls, the whole ball of wax) shows it reasonable to suppose that if I perform a_i the probability of an outcome O which I intend or desire more than the alternative empirical outcome possibilities is non-negligibly increased over the probability of O occurring if I refrain from performing contemplated action a_i. Secondly, if—

SOP: Now, wait a minute, that first one would automatically exclude almost any political action, according to your conceptions, so why do you bother mentioning it?

FEV: I mention it for completeness's sake, because there are a few situations, for instance, when the voting group is sufficiently small (as in a small college faculty meeting or maybe even in voting for alderman), in which the probability of my vote making a definitive difference, either in the outcome or in the appearance of strength, is non-negligible. Or suppose I had been a candidate for nomination and was defeated at the convention; then my decision publicly to support the nominee might make a significant difference. But you are of course correct that in speaking of an individual anonymous voter whose name carries no particular clout on the social scene, and whose contemplated action is not voting for dog catcher in his precinct but for the President of the United States, this first condition would never be satisfied. That's why we need a second (alternative) condition of political rationality if the ordinary voter is ever going to get into the polling booth rationally at the national level.

SOP: I cannot resist pointing out that in the matter of non-negligible probabilities of having differential effect, there is a slippery slope. It would be hard to draw an arbitrary line indicating when your condition one is unfulfilled.

FEV: It is hard to draw an arbitrary line, but that doesn't stop us from categorizing the vast majority of the instances. Don't forget what Edmund Burke said about twilight: its existence does not prevent our drawing a tolerably clear distinction between night and day. I surely don't want to get us into the "when is a man bald" argument, which fortunately we needn't settle in order to decide that a man with four hairs on his entire scalp *is* bald. Similarly, we don't have to decide precisely when the statistical probability p of my affecting the outcome of the election, or even the "showing of my candidate," is to be labeled "non-negligibly small." For instance, one might have a hassle about the issue like that of the statistician in medical research having to decide whether a significant result should be (for this curative purpose) set at $p < .01$ or $p < .05$. Statisticians tell us, "That's either arbitrary or, more often, decided on the basis of the relevant utilities (serious consequences of Type I versus Type II error, cost of enlarging the experiment)." I propose we accept the admittedly arbitrary cutting point of Buffon and agree that $p < .0001$ counts, for any practical purpose of human decision making and individual action, as a "substantially zero" or "negligible" probability. Thus, if we get into a local election with a small number of voters, we might have a problem deciding about whether that cutting score was exceeded. I treat $p = .0001$ as a sort of lower bound, not claiming it's a greatest lower bound, on the set of what I call "non-negligible probabilities." But I am talking throughout about presidential elections, and there the facts show that a direct, act-prospective means-end condition for rationality is clearly not satisfied when considering the participation of the individual "non-famous name" voter. As soon as we get into cases where some highly visible person's support for a candidate can make a difference, then we are in a different ballpark.

SOP: All right, I agree it will get hopelessly messy unless we stick to the anonymous noninfluential single voter in a presidential election. What was your second alternative for individual action a to have political rationality?

FEV: It wouldn't be incompatible with the first one, but it might be substituted for it; and of course my position is that it *must* be considered an adequate substitute, if we're going to get me *or you* to the polls. The second possible principle of political rationality is that action a is in accordance with a rule or principle, axionomic in character, which has itself been given some kind of ethical justification. I suppose this amounts to saying that *the basis of political rationality can include ethical reasoning that need not be act-prospective.* I emphasize again, however, that it may *also* be act-prospective in some instances, as in voting for dog catcher at the township level, or publicizing my vote when I'm a "big name."

SOP: I don't see why you can't simply refer to its being ethical, since universalizability is one of the generally admitted requirements of an ethical rule or principle, shared by philosophers and political scientists of various ethical persuasions.

FEV: I would be willing to put it that way, except that I must be sure to exclude any justification that is act-prospective, and there are sophisticated forms of act-utilitarianism[10] that would be "ethical" philosophies by any ordinary usage. Besides, the term "ethics" is sometimes taken to include such axiological positions as egoistic hedonism.[11] The point is that I don't want to get into that argument, and even a form of act-utilitarianism like that of Sartorius does not get me to the polls. It would be impossible to use Sartorius's reasons to justify my voting, since the general principles needed to bar a direct appeal to utility, as applied to the national presidential participation by an individual citizen, could not be thus derived. For instance, one of the Sartorius considerations that can bar direct appeal to utility is the fallibility of human judgment about matters empirical—the fact that I cannot always carry out adequate computations about the act-prospective considerations when I contemplate a line of conduct. But in the case of my bothering to vote in a presidential election, it is obvious that I *can* carry out these computations, and that the differential probabilities are essentially zero. Furthermore, we mustn't identify act-prospective justifications with act-utilitarian justifications, because there can be act-prospective considerations that are not act-utilitarian (such as an individual magistrate's deciding something because he is convinced that if he decided the other way, individual injustice would be done, quite apart from his estimate of the total net utilities). It seems to me that the issue between a "pure utilitarian" and somebody who gives justice a separate weight, or postulates a Rawlsian lexical prepotency over utilities, is an issue we can't afford to get into here, when we don't really need to.

SOP: I guess I have no complaint against that, so go ahead with your reasoning.

FEV: I reason thus: In order for your action a_i in voting for Republican candidate Claghorn, or my action b_j in voting for the Flat Earth-Vegetarian candidate, Pastor Sedlitz, to be a rational act, it must be an act which is in accord with a nonact-prospective ethical principle, since this is our second condition and we have agreed that the first condition cannot, at the presidential election level, be satisfied.

SOP: I still can't quite get over the feeling that you have somehow sold me a bill of goods, because my intuitions persist that there is some reasonable sense in which one can say that an action a is a rational means to a political end when it belongs to a class of actions A performed by a class of individuals C_A who share my desire for that political end, and who must, as a group, work together in bringing that end about.

FEV: I know how you feel about that intuition and I share it myself. But I must insist that it is irrational unless stated in a certain careful way, and my point is that people do not ordinarily state it that way. If one says that the individual action a is rational as a means to a political end *whenever it is an exemplar of a class of actions A that a group of persons C_A must collectively* perform in order for that end to be attained, that is an ambiguous formulation. It is not clearly false, but it is not clearly correct either. It just has to be unpacked.

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[10] See, e.g., Rolf Sartorius, *Individual Conduct and Social Norms* (Encino, California: Dickinson Publishing Company, 1975).

[11] Henry Sidgwick, *The Methods of Ethics* (London: Macmillan, 1907; Dover Paperback Reprinting, New York: Dover Publications, 1966).

SOP: It sounds pretty clear to me as you have just stated it. What's the matter with it as it stands?

FEV: I know it sounds clear, but it is not. There is nothing *wrong* with it as it stands, but it is objectionable as a formulation because it lends itself to an easy corruption, to wit, arguing distributively rather than collectively and then, via that error, arguing on a purportedly means-end, act-prospective, empirical-conditional basis, *without the ethical element* and *without the counterfactual element.*

SOP: And you want both the ethical and counterfactual elements for your nefarious purposes, is that it?

FEV: Exactly. We have a harmless sounding but, I think, ambiguous statement: "It is rational for a voter i to perform individual action a_i if a has the property A which actions as a class C_A performed by a group to which he belongs (such as Republicans or Flat Earth Vegetarians) would, if performed by the members of the group, be politically efficacious." By "efficacious" here we mean that actions C_A would have a non-negligible probability of bringing about intended political outcome O rather than disfavored outcome $\bar{O}$. But this kind of talk can be rendered as "Everybody belonging to C, or a sufficiently large number of persons belonging to political interest group C *including me,* must perform action a in order to achieve O." This, as I have insisted and I assume you now agree, is literally false to fact. On the other hand, the loose formulation can be rendered in a cleaned up form as, "It is rational for me to perform individual action a because it has the property A which a sufficiently large number of individuals in group C (who share my views) must perform, *whether that collective includes me or not.*" This latter formulation is not analytic or intuitively obvious without a pragmatic principle or justificatory rule, that is not act-prospective but is axionomic and sociotropically ethical. What I find objectionable in the ambiguous formulation is that it can be taken to mean that since the group statistically must do such and such, therefore I must do such and such, *as a means to an end,* i.e., as an act which is politically efficacious in the sense that my individual failure to do it would bring about the failure of the desired outcome, or will materially reduce the probability of the desired outcome. And while I am doubtless boring you by saying it yet once more, I must repeat that this is literally false as an act-prospective, empirical, means-to-end, conditional assertion. The second translation is all right, but then it becomes a candidate for some kind of proof. Because we cannot infer directly, from the statement that collective "statistical" action A_c is necessary and sufficient to bring about the political outcome O, that therefore individual action a_i by individual agent FEV_i is necessary and sufficient for outcome O. And one can easily show by a simple empirical-mathematical argument that in the domain we are discussing, the latter assertion is not only not a valid consequence, but is in fact false.

SOP: So where does the rationality come from then? I suggest that you have proved too much. I am impelled to put a Konigsbergian query, "How is rational voter action a priori possible?" You have somehow to relate the means to the end, and it sounds to me as though you have effectively cut off the possibility of any such reasonable means-end transition, by the terms of your own argument, have you not?

FEV: As long as the rationale is formulated simply as an act-prospective empirically conditional means-end connection, that is what I think. So, if we are to get me into the polls, or, I repeat, if we are to get you into the polls to support your Republican candidate Claghorn, we have to have something stronger than this, something that goes beyond an empirical means-to-end argument of the form, "Since you individually desire X, you individually must do Y, because your doing of Y is the necessary and sufficient condition for X". Individual voting in a presidential election simply can't be justified that way.

SOP: I don't think you can afford to be stuck with *any* requirement, however you put it, that involves a reference to necessary and sufficient conditions, when we are operating in the realm of politics. You're going to be stuck with probabilities in any case, are you not?

FEV: Correction sustained. What I should say is that the question hinges upon whether my or your individually performing actions a (voting for Pastor Sedlitz) or b (voting for Senator Claghorn) is a condition for bringing about a significant increase in probability of an outcome intended by me or by you respectively. But one can include a funny kind of reference to its being necessary and sufficient, if the "outcome" is stated not as the result of the election, or even the good showing of a candidate, but as a necessary and sufficient condition for bringing about a non-negligible increment in the probabilities of those outcomes. I think I could do best by both of us if I throw the ball back to you, since after all the structure of my argument is that a principle adequate to get you into the polling booth to vote for Republican candidate Claghorn will, if carefully stated, also be adequate to get me into the polling booth voting for FEV candidate

Pastor Sedlitz. How would you defend voting for Senator Claghorn, since your vote will not determine the outcome, even in the weak sense of "improving his showing" in the event that he loses?

SOP: I suppose I would proceed along the lines that you are suggesting. I would invoke a conditional which says that, roughly, "If a sufficiently large number of persons vote for Claghorn, he will be elected. More generally, if a sufficiently large number of persons vote, democracy will continue, however clumsily, to function. Perhaps this last is more forcibly stated in the negative, that is, if enough people don't bother to vote—for whichever candidate—democracy will not function." Then I postulate, or derive (I'm not sure which, but maybe it doesn't matter for our purposes) some kind of ethical rule or principle which says that an individual member of a democratic polity has an obligation to make his political will known via the ballot box.

FEV: Why?

SOP: *Why?* It seems to me rather obvious why.

FEV: It's intuitively rather obvious to me too, but since I want to watch you like a hawk in this phase of the argument, I'd like you to say why. I don't require a derivation, and I don't know that I could give one myself, except perhaps in some general terms involving an implicit social contract or something of that sort. That is, if I receive the benefits of the society (including the very great benefits that come by virtue of its being a political democracy), I have some obligation to be a political participant in its decision-making processes. Will you buy that?

SOP: Yes, I think that's good enough for both our purposes, although I don't quite like this stated as an obligation, since there might sometimes be good reasons for abstaining from participation in the process.

FEV: Agreed. We could either say that it's one of W. D. Ross's prima facie obligations, or we could make it even weaker and say something to the effect that there is a kind of "obligatory tendency" involved. Like you, I wouldn't want to say that there was an absolute hard and fast moral-political rule that everybody has to vote in every election.

SOP: Of course one way you make your views known if you have a strong distaste for the available candidates is by abstaining. Modern political science has available some souped up statistical methods, based upon careful breakdowns of various geographical areas in terms of their make up and their previous voting pattern, of coming to empirical conclusions such as "part of the trouble with Governor Fisbee is that he lost the Polish Catholic vote this time around."

FEV: Yes, I would be quite willing to include periodic abstention from the ballot box as one form of meaningful political participation. Although I must emphasize that abstaining doesn't require any output of energy or opportunity cost or risk of getting killed on the way to the polls, so that the cases are not quite on all fours.

SOP: Anyway, we agree that an absolute requirement to cast one's vote is excessively strong, and is not needed for either of our purposes. So where are we now?

FEV: I was egging you on to spell out the principle in a little more detail. If you can derive it from some kind of ethical principle more general than that of political life, that's fine with me. But I repeat that I don't require this, as I'm not at all sure that I could do it myself on demand.

SOP: Well, let's not try it then. I gather we agree that if one did try it, it would involve some kind of reference to keeping the books straight between myself and the other members of my society, or some kind of reference to the benefits that I get requiring my participation in return, sort of analogous to the lawyers' notions of "unjust enrichment" or quasi-contract or some such way of thinking.

FEV: Okay, let's see how you put the principle for your purposes; never mind mine for the moment. And please explain the "obligatory tendency," if you need to rely on it. It was my expression, but it makes me uncomfortable, as it smacks of psychologism. We both know that voters *feel* obligated to participate, and the issue between us depends on why they *are* rationally obligated.

SOP: I want to avoid psychologism as much as you do. How about the term "obligation vector," to get the quantitative and directional flavor?

FEV: All right.

SOP: I would say that some sort of prima facie obligation or obligation vector exists for me as a voter to participate in the electoral process, relying on the general principle that unless people do, the system won't work; and when it comes to the *content* of my vote and the more specific application, I suppose further relying on the principle that if enough people who feel as I do vote as I contemplate voting (for Senator Claghorn), he will win or, even if he loses, he will make a respectable showing; and this will affect the subsequent course of events. It might, for example, affect the behavior of his opponent Governor Fisbee when he becomes President.

FEV: Sounds all right to me. I suppose then you would be willing to make explicit that there is here involved some kind of principle that says I have a quasi obligation or an obligation vector to do whatever it is I sincerely believe would be a "good thing" in terms of collective interests for some unspecifiably large number of persons to do, or something like that.

SOP: Yes, that is surely the underlying idea, and I presume that's why you made your earlier reference to the principle of justification being "quasi-Kantian."

FEV: Right. I certainly didn't mean to commit either of us to any orthodox form of the Categorical Imperative, except insofar as there is something rather like the Categorical Imperative involved in almost any nonegocentric axiological premise or meta-principle, to wit, the idea of universalizability. And I think we're both stuck with that much of Kant, are we not?

SOP: I think that's almost certainly true, and at any rate I am willing to include it among our shared political postulates for purposes of the present discussion.

FEV: Good. Go on then with your development—you were explaining why you intend to vote for Senator Claghorn.

SOP: I am going to vote for Senator Claghorn because I favor him and his views, or at least I favor more about him and his views than I do about the competition. I don't suppose we want to get into the knotty problem of deciding what to do when one likes some things about one candidate and dislikes others, or prefers one candidate but prefers the platform of the opposing party. It seems to me that distressing as those complexities are, and capable as they are of generating terrible problems of weighting and even sometimes out-and-out paradoxes or Arrow-type pragmatic contradictions, it would be a tangential issue for our purposes here.

FEV: Agree entirely. But please continue with your derivation.

SOP: So to simplify matters I will just say that I think that Senator Claghorn and his position are clearly the best candidate and position to solve the problems that currently confront the country. And I intend to vote for him because if everybody did—no, strike that, I'm falling into the same careless language you did earlier!—*if enough people did,* then he would be elected, and that would be a desirable social outcome.

FEV: Don't you want to mention "showing strength" here?

SOP: Yes, quite right, add ". . . and if he is elected I want him to be elected by a large enough majority so that he can appropriately talk of having 'received a mandate' and so forth."

FEV: But what if he isn't elected?

SOP: Even if he isn't elected, I would like him to make a "good showing" because I believe this will have some influence, at least on the average and however slight in some respects, upon the successful opposition candidate during his tenure of office.

FEV: So what's the principle involved?

SOP: I guess the principle is that I feel an obligation to vote for the person whose qualifications and views I believe would be a good thing, in some overall sense.

FEV: But why do that, since your vote doesn't determine the result?

SOP: I thought we went over that already. I feel an obligation to vote because I believe it would be a good thing if enough people (don't know how to say "enough," but it either means enough to get him elected or enough to make a good showing whether he gets elected or not) will do so.

FEV: But what if not enough people will do so?

SOP: The point is that I *don't know* in advance how many of them will. And that's one of the big differences between my voting for Claghorn and your voting for Pastor Sedlitz and the Flat Earth Vegetarians. You *know* right now that Pastor Sedlitz is going to lose, but I don't know that Senator Claghorn is going to lose.

FEV: I don't see what difference that makes to either of us.

SOP: It seems to me the difference is obvious and important. When one is contemplating an action and doesn't know what its effect is going to be, isn't his situation rather dramatically different from that in which he does know what its effect is going to be, to wit, nil?

FEV: I hate to keep dragging us back to previous points, but I thought I had successfully argued that *your effect* (in performing your individual action a_i) is going to be nil in either case. That is, Senator Claghorn is either going to win or he is going to lose, and if he loses he is either going to make a good showing or a poor showing and hence influence the subsequent behavior of Governor Fisbee as President; but whether he loses or wins, and whether if he loses he loses making a good or a poor showing, is not a function of whether you vote for him or stay home reading the funnies. Look at it this way: You do not determine that Senator Claghorn wins by voting for him, nor do you determine that he makes a good showing by voting for him. The difference between his winning Minnesota by 274,631 votes and 274,630 votes is a socially, politically and psychologically null difference; ditto for his losing by one of these two amounts. Furthermore, you don't determine the base *N* to which that unit ballot is added or subtracted. That is, not only don't you determine the outcome by whether you vote, and not only do you not determine the showing by whether you vote, but you don't determine what the collective has done by whether you vote.

SOP: All right, all right—of course the whole business depends upon how many people think as I do about Claghorn and the platform he stands for. It seems to me we have to get that in somewhere.

FEV: I don't mind getting it in, because I would need it myself in justifying my intended ballot for Pastor Sedlitz and the Flat Earth-Vegetarian Party. But it seems to me the only way you can get it in is the same way you get in the reference to "enough people voting." That is, just as your vote doesn't determine who wins, and your vote doesn't determine how good a showing the winner or loser makes, so your vote doesn't determine how many people think and feel as you do (or I should maybe say how you think and feel in deciding how to vote doesn't determine how many people think and feel as you do).

SOP: Well then we have to move it back one step in our statement about universalizability, do we not?

FEV: That's all I would need.

SOP: I'm not sure just how we should be putting it at this point.

FEV: I don't know that I could put it precisely but wouldn't it be something like this: I am obliged (or, better, there is an obligation vector on me) to cast my vote so that it instantiates a universalizable statement to the effect that a voter ought to vote for the candidate that he believes to be the best one, all things considered. And this ethico-political principle is in turn derived from some more general statement about acting politically in a way that one can say, so far as he sizes up the situation, it would be sociotropically desirable for a sufficiently large number of persons to act, assuming of course that they cannot in good conscience act against their political or other strong convictions. So I guess this amounts to saying that you think it would be desirable if as many people as possible, or at least enough people to elect or make a good showing, shared your views about Senator Claghorn and his party's platform. Will that do it?

SOP: So far as I can see that would suffice to get me to the polls. It mischievously occurs to me, however, that I might have an overarching principle that suffices to get me to the polls, that was sociotropic in content, and nevertheless once there, I might vote egocentrically. What would be the matter with that?

FEV: I'm not sure it's an out and out contradiction, although I suspect it is. Put it this way: To vote for X implies to vote. If the ethical principles involved in selecting X are not sociotropic, then it seems to me they would be axiologically incompatible with the ethical principles required to vote at all. That is, I can't vote for X without voting for somebody. And if the rationale in voting for X is a completely egocentric rationale, then I have cut the ground out from under my feet so far as justifying voting for anybody. Because we have seen that voting for anybody is egocentrically irrational, since maxims of selfish prudence would leave me at home.

SOP: Yes, yes, I know all that, although it's remarkable how hard it is to remember it.

FEV: That's right. That's why politicians in the two major parties, no offense to you, can easily get by with what is basically a fallacious argument when they keep telling disaffected independents that they would be throwing away their vote by voting for a minor party candidate.

SOP: But, you know, your argument went a bit fast a moment ago. You say that I can't vote for X without voting for somebody; so that if my rationale in voting for X is completely egocentric, I have cut the ground out from under my feet in justifying voting for anybody. That line of reasoning has some plausibility, but I don't find it persuasive. Why shouldn't I have a principle of political participation that justifies my voting, but still proceed, having decided to go to the polls on that principle, to cast my ballot, once there, on the basis of rational self interest? You really ought to spell that out a bit more.[12]

FEV: I'll try, although it seems to me pretty obvious. There might be, I suppose, some rational reconstruction of an act-sequence such that an earlier member of the sequence is justified by a different ethical principle from that used to justify a later one. Even there, if the meaning (purpose, "sense") of the first member of the response chain depends upon the later one, so that the first sub-action, being instrumental or preparatory, would be, so to speak, "meaningless" or "pointless" without the second (or at least the anticipation of it), then I should think that while the principles might be *different*, they could hardly be *contradictory* without getting us into logical trouble.

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[12] I am indebted to my colleague Rolf Sartorius for challenging this argument and forcing me to expand the dialogue hereat; as he is out of the country I do not know whether he will find the expanded form persuasive.

But actually the voting case is far tighter than such an act-sequence would be. We don't deal here with two actions in a chain. (Of course you could make it seem that way by detailing the instrumental sequence involved in getting to the polls, such as putting on your topcoat, then getting the ignition keys for your car, opening the garage doors and the like. But these instrumental precursors of the molar "act of voting" are themselves justified by the *telos* of the final voting act, and I don't think we need have any quarrel about them.) The important thing about the act of voting is that the relation between "voting (at all)" and "voting for Fisbee" is *not* merely a contingent empirical connection, as between two acts one of which is a precursor or instrumentally preparatory for another. It is not even adequately represented by a stronger relationship than the causal arrow, such as the logician's "hook" (deducibility). No, the proper way to analyze it is that there is *one act*, namely voting (for somebody—you can't vote without voting for somebody, can you?).

So, if we do divide it up somehow for analytic purposes, we can't divide it into two actions, or even two components or aspects of an action. We have to look at it in terms of an action a of which some property or attribute P is predicated. That is, "voting" is the action, and we inquire about its various *properties*, such as whether you pull the lever with your right or left hand, whether you do it quickly or with much deliberation, and more importantly, *for whom* you vote. So that any notion of somehow splitting the behavior up in such a way that you can give an ethical ground E_1 for voting and then give a different *and incompatible* ground E_2 for your choice of candidate, is based upon an incorrect analysis. There is simply one act; the candidate-choice involved is one of its properties; and furthermore, that property of candidate choice is a determinant under a determinable whose presence is analytically part of the very concept of voting.

After all, nobody has ever proposed to define the act of voting as being physically at the polls on a certain night, without making a mark on a ballot or pulling the lever of the voting machine! So I argue that one cannot justify the act of voting (which, I repeat, always means the act of voting *for somebody*) by a principle that is act-prospective egocentric; rather he has to justify the act by a principle which is axionomic and sociotropic. And if that principle is rationally active at the moment of his casting his ballot, it cannot become mysteriously irrelevant or be replaced by an act prospective, or a maxim of prudence-egocentric, kind of principle as the basis of rational choice between the candidates.

SOP: As I see the state of the discussion, we had arrived at a point where I was defending my vote for Claghorn on grounds of unpredictability, and you objected to that line of argument.

FEV: Yes, I do object most strenuously. In order to rely upon the argument from ignorance, namely, that I do not know what is going to happen and that I therefore ought to decide how to behave on the basis of that "informed ignorance"—knowing the alternatives and the risks and maybe something about the probabilities—one still needs some kind of strong ethical principle that involves universalization contrary to fact. Compare voting with buying life insurance. When I contemplate buying life insurance to protect my family on the grounds that I do not know the date of my death and that it is possible I may get hit by a truck tomorrow, the core of such reasoning lies in the if-then statements that I can make about consequences of the various unforeseeable contingencies. That is, I can say that if I fail to buy life insurance, and if having thus failed to protect my family, I die tomorrow of a myocardial infarction or an automobile accident, then my decision will accrue specifically, individually, and with near-certainty to their disadvantage. Given my state of ignorance, I am still able to attach differential consequences to various actions I may choose, considering each of the various unknown future states of nature.

But that is simply not the case in the voting situation because of the same old "large numbers" or "collective" argument. That is, you do not know whether Senator Claghorn will get a majority and become President of the United States; and you do not even know whether, if he should lose, he will at least make a good showing; but the point is that there aren't any statements you could make that have any appreciable probability attached to them which are of the form, "Assuming that he loses, then *if* I were to add one more tally to his vote count, *then* . . . [consequences]", because all such conditional statements are literally false. There isn't any way to get around it: *In a presidential election whether you vote for a given candidate or don't bother to vote at all has a negligible probability of having a non-negligible effect on either outcome or goodness of showing or policies of winner, regardless of what other people do so long as enough of them vote at all, which enough of them are quasi-certain to do for the purposes of this argument.* That is, you do not know in advance how many people will vote for Claghorn or

Fisbee, but you do know that enough people will vote in the presidential election so that the differential impact of your vote for Claghorn or Fisbee (or my vote for Pastor Sedlitz) *has a quasi-certainty of exerting a negligible influence upon anything.* That's the basic empirical-statistical fact upon which the entire discussion must remain based, and the strong point I reiterate is that given that basic premise, which no sane man will dispute, there isn't any way to get either of us into the polls to vote for anybody unless we invoke some strong ethical argument about universalizability despite its counterfactual character and the collective nature of the outcome. I daresay you are getting as tired of hearing this reiterated as I am getting tired of saying it, but for some reason it seems unusually difficult for us to accept.

SOP: I suggest that the difficulty is that it gives anyone who takes it seriously a feeling of political impotence.

FEV: That may well be. All I would say is that the impotence I have as a Sedlitz supporter is actually no different in kind or amount from the impotence you have as a Claghorn supporter.

SOP: But you know, despite the apparent irrefutability of your main thesis, I still can't quite liquidate the view, or perhaps I should say (as a concession to rationality) the "sentiment," that in casting my vote for Senator Claghorn I am in some significant sense helping him to win or helping him to make a decent showing, whereas you admit that you know in advance that Pastor Sedlitz and the Flat Earth Vegetarians not only cannot win but cannot conceivably make a decent showing, and therefore you are not, as a tiny electoral atom so to speak, "helping" to do anything. I really don't want to say, when the chips are down, that this sentiment of mine is utterly foolish—even though I agree that in the sense of an appropriate means to an end, my individual vote doesn't have the usual means-to-end empirical properties with which we are familiar in our ordinary decision making in nonpolitical areas of life. There is something in me that insists on a hard core of reasonableness in that sentiment, and I find it difficult to think that you don't share it, reluctant though you may be because it's bad for your position.

FEV: We are being honest with each other, and I can see that I have some such feeling. That is, I do have a feeling of impotence when I cast my vote for Pastor Sedlitz; and I don't suppose that feeling would be as strong if I were casting it for Senator Claghorn or his opponent Governor Fisbee, since we agree that empirically it is quasi-certain that one of those two major party candidates is foreseeably the winner. But when I try to explicate the nonemotional content of that sentiment, which I share with you, I don't seem to get very far, *unless I put in a distinctively moral element.* That is, if I unpack the cognitive as opposed to the emotive content of this attitude—which, I repeat, I do tend to share with you even though I don't like it for my position—the best I can get sounds pretty silly to me, and I think it will sound pretty silly to you upon leisurely contemplation. It goes sort of like this: "If the collective of a very large number of voter actions a_1, a_2, $a_3 \ldots a_N$, where N runs into millions, achieves a political outcome O which I think it good to attain; then I think of myself as helping to attain it in some significant sense even if the counterfactual 'Were I to stay home from the polls, the end O would not be attained' is false. But if the end is foreseeably unattainable because there will be a very much smaller N_B of acts B, then my individual contribution to the collective of actions b_1, b_2, $b_3 \ldots b_N$ is not in any significant sense helping." What that would mean is that you can only be said to help achieve an end if the end is achieved, even though whether it would have been achieved without your individual helping is irrelevant. I think such a definition of helping is irrational despite the intuitive appeal to the individual participant.

SOP: You can make it look pretty irrational, depending on how you choose to say it. You are emphasizing that something doesn't make a difference, whereas in my formulation I would emphasize that collectively we all do it.

FEV: Just how would you express it so it would sound more reasonable? And please don't use the word *help* if you can avoid it. Or at any rate I would like you to spell it out if you do.

SOP: I would put it roughly as follows. If a collective action A_c results in dichotomous outcome O, or if the outcome I desire failed but my candidate makes a good showing (even if he wins I can, as we have agreed, have an interest in how good a showing he makes) then my individual ballot, my individual action a in going to the polls and voting for him, can be said in some significant sense to have "helped" to bring about an outcome which was in fact successfully attempted by all of us in taking these actions. Whereas if my action a belonged to a class of actions performed by the collective C of persons who agree with my views, but their candidate fails at election and, further, so few of them agreed with me (or so few who agreed with me bothered to take this action) that our candidate also made a poor showing, then I would say I have not helped to do anything that is significant. I remind you that a sufficiently poor showing may actually work *against* my views, because it begins to take on a kind of ludicrous and fanatic quality. And it doesn't seem to me that this formulation is as silly as you have suggested it is.

FEV: All right, I will confess that my intuitions go a little bit with you on that formulation. But I still want to unpack the rationale. We have agreed that nothing hinges upon words, and I am not going to quarrel with the word "help", although since you get a certain amount of motivational mileage out of being able to use that word in this rather special collective action sense, I have a sneaky desire to tack on a negative-sounding modifier such as "negligibly help" or "nonsignificantly help" or "ineffectively help." But that would just be a semantic device to counteract the semantic loading of the word "help," so let me do it this way: If the term "help" is taken to mean what I would prefer to have it mean as in the tug of war example, namely, that I make a significant difference that is foreseeably different, and by that I mean that I non-negligibly increase the probability of the dichotomous outcome O (my candidate wins whereas he would not have won had I not voted for him); or that there is a non-negligible probability, assuming that he wins without my vote, that my vote constitutes a significant increment in his mandate as a winner; or if he loses, that my vote has a non-negligible probability of his making a good showing and therefore, despite losing, influencing the course of political events in a variety of possible ways; then the argument runs easily from the basic premise that "He who wills the end must will the means," or some similar kind of act-prospective, means-end empirical contingency statement. What I want to maintain against your present offer to explain "help" in more broad terms (and so do justice to your intuitions) is that when the word "help" is used as broadly as you propose to use it, where it *does not* imply that there is a significant increase in the probability of an outcome or of a good showing by my action, that the overall generalization "You ought to help . . . [consequences]" can no longer be taken simply as stated. It can now be challenged because it is no longer axiomatic or intuitively obvious or analytic or whatever kind of meta-principle we are relying upon in the domain of pragmatic inference. When "help" no longer has a hypothetical means-to-end, act-prospective conditional significance, which it doesn't as you are now defining it, I will cheerfully accept your revised and extended definition of "political helping." But now I will deny the overarching premise that one is *obliged to help* elect his candidate or to *help his candidate make a good showing,* because with this broadened meaning of "helping" *which no longer implies any differential consequence with non-negligible probability,* I would not accept, without further argument, a premise that says that one is obliged to help his candidate to win or to make a good showing.

SOP: I just don't understand what you're saying now. You are accepting my definition of "help," but you are saying you now deny one ought to help, is that it?

FEV: No, I am not denying one ought to help. I am saying that with this extended meaning of "help," the proposition that I am obligated to help support my candidate is no longer "obvious" or "self-evident" or "acceptable as a primitive ethical obligation of political life," which it might plausibly be said to be on a narrower use of the word "help." I guess what I should say at this juncture is that the four conditions for rational justification of my individual ballot have returned in full force.

SOP: How do you mean the four required assumptions return in full force? I didn't really intend to renege on what we had agreed on in discussing them. Are you suggesting that I am?

FEV: Maybe you didn't mean to, but my point is that the admittedly intuitive notion of "helping" as we have now spelled it out came up because you felt that somehow I was wasting my vote for the Flat Earth-Vegetarian candidate Pastor Sedlitz but you were not wasting yours by voting for Republican Claghorn or Democrat Fisbee. You felt that way because one of those two had a chance to win, and even the one that lost will have an impact derived from the strength of his showing. (Let's assume we don't know which one of them will lose, although modern public opinion polling makes it almost a sure thing to say which one will win—I'll return to that in a moment because it's very important in our dispute about foreseeability.) The intuition gets its force from the semantic overtones of the word "helping" or similar words like "participating in collective action" or "contributing, however slightly, to the total result." And I am arguing that when we spell out the meaning of "help" that is needed for your purposes at this point, it is not self-evident or an analytic truth of practical reason, so to say, that one must vote because that is the appropriate means to his own desired end.

The point is that straightforward application of an axiom about ends and means is unproblematic only if there is a single end and only if the means are known empirically to be efficacious or at least probably (or at the very least conceivably) efficacious. Whereas in the individual voting situation there are other ends (such as the risks and the clearly foreseeable costs, however slight, of bothering to vote) as we have discussed above; and, more to the point, even if the means were completely devoid of disutility or risk, the means-to-end conditional relationship is not an empirical one. It is mediated first by making reference to the collective, and then by applying that statement about the collective to oneself as an individual, hoping to grind out an obligation vector from that application.

My point is that once we have spelled it out to that degree, then one goes back to the question, "Why should I bother to do something that is quasi-certainly inefficacious, in the precise sense that what happens does not *depend upon* my individual action a_i contemplated?" When you spell out "help" in the way we have just done, this important question recurs.

The reason I speak of it as "recurring" is that when we have stated that I do in some sense "help" to bring about an end which is successfully brought about by the collective action of numerous people helping as I am helping, the word "help" does make it seem, *unless we scrutinize the details carefully,* as though we had an instance of a straightforward application of the general axiom of practical reason which says that if you will the end you must will the means, *ceteris paribus,* assuming the means are licit. So it might appear that one has, by spelling out the intuition of helping, somehow bypassed the reasoning we went through earlier. And my point is that we can't bypass it because once we spell it out this way, it is simply no longer evident or obvious or a truism of practical reason that I ought to take the means to the end, since the means, considered here as my individual action a in going to the polls, is not literally a condition for the end. We only get to it by reference to the collective and, as before, adding some sort of counterfactual or neutrofactual remark about what *would* happen if a lot of people stayed home, or what *would* happen if *enough* people voted for Claghorn instead of Fisbee, and the like.

So my point is not to dispute about the word "help" as we have agreed to use it in your broad sense. My point is that when we have agreed to use it in that broad sense, it ceases to be an analytic truth of practical reason that "Anyone should help when he can . . . [consequence]" or however else one might choose to say it. In order to make it stick as a moral statement, since it cannot be a simple act-prospective means-end conditional statement in the political situation, one has to have the four conditions we laid out previously. One has to rely upon some sort of overarching principle that is axionomic, sociotropic, collective, and neutrofactual. So I continue to insist that your intuitions about "helping," which I admit I share, do not avoid the thrust of my argument.

Furthermore, even if I were to buy what you say about "help" applying only when success is possible, which means almost all presidential elections, we would be on a slippery slope because of those elections in which the Gallup Poll made it highly probable that one of the major party candidates was going to lose. As you know, the first-class pollsters haven't missed one since 1948, and even there the best outfits, using the most scientific methods (e.g., the Michigan Survey Research Center) called it right for Truman over Dewey. I just don't see any way to get to the desired conclusion that individual voter John Q. Public should bother at the presidential election level without relying on a distinctively moral proposition that somehow says something about one having an obligation (or a prima facie obligation or an obligation vector) to act in accordance with a property of action A which one believes it would be desirable, *for whatever reasons* (I repeat I am not committing myself to utilitarianism or distributive justice or any other particular social content as to the ends) to be collectively achieved by some unspecifiably large number of persons doing it. How many will do what you intend to do is not something that you determine, and the same goes for me and for everybody else. If the principle requires that I determine the outcome, or the goodness of the showing, or that I determine by my vote how other people will vote and hence the collective result of which mine is a part, none of these forms of the principle will get me there because they're all empirically false with quasi-certainty. It has to be an ethical thing; and it has to be a positing, in a distributive sense, of something that is politically effective only in the collective sense; and it has not to depend on the actual outcome or the predictable outcome, but upon what *would be* the outcome if enough people did what I am contemplating doing. That is why I say that it must be both collective and neutrofactual ("possibly counterfactual"). And when I put all those requirements together, that it must be axionomic rather than act prospective; that it must be sociotropic rather than an egocentric maxim of prudence; that it must be collective rather than distributive; and that it must be possibly counterfactual rather than an indicative conditional—then, while you have all you need to justify a vote for Senator Claghorn, by those four principles I have all I need to justify my "useless, thrown away" vote for Pastor Sedlitz. If any one of these four principles is removed or weakened in such a way as to render irrational my voting for Sedlitz, I am confident you will find that the rationale of your voting for Claghorn will have also been destroyed in the process.

SOP: It's a plenty tough argument to answer, and I am surprised how tough you have made it. At least for the time being, I don't believe I can rebut it. Just a couple of final questions, though. First off, don't you think you might get into some difficulty, not at this philosophical level but at an intermediate level of some of the derived consequences, within the framework of current empirical political science theory—namely, that countries which have a sizable list of different parties don't seem to function as well as those with a couple of big parties and maybe a third party at most constituting a swing party or a possible basis for a coalition government?

FEV: Yes, I think I may have a problem there, but that takes me into details and complexities beyond my competence in political science. I don't know that I would be willing to subscribe to any simple empirical generalization about the merits of a two big-party system, particularly since, as I read the record, the United States' two-party, both "moderate," political system, despite its impressive historical performance, is now creaking badly in the joints—if not coming apart at the seams. Personally, I have the impression that not one single major problem of our society is currently being solved by our political system, but that view probably reflects my substantive beliefs in the Flat Earth-Vegetarian picture of the world. So I guess I would rather just say that you have a point there that would be worth exploring on another occasion. What's your other question?

SOP: Let's stay on this one for a moment. On the social facts, I don't agree at all about our two-party system coming apart at the seams. Creaking at the joints, yes—of course I hold that is the human condition, it's the nature of *homo politicus*. But I agree we can't go into all that now. There's an aspect of the third party "sure-loser" business, however, that really needs clarification on your side. I think that persons politically identified with one of the two major parties sometimes use the locution "throw away your vote" not as a general designation of all circumstances where one votes for a low-probability candidate, but instead confine this "throwing-away" language to the special situation where one can be highly confident that the great majority of minor-party voters in a particular election will be drawn from (or, putting it affirmatively, would alternatively be drawn into) one of the two major parties rather than the other one[13]. If it can be further assumed that the race between the two major-party candidates is going to be very close, so close that a movement of potential third-party voters into one of the two major-party supporters would probably make the determining difference, then the notion of "wasting one's vote" on the third party candidate takes on a stronger meaning. Suppose, for example, that we are waging an unpopular war against Communist Eskimos in Greenland, and your Flat Earth-Vegetarian candidate Pastor Sedlitz proposes immediate unilateral withdrawal, which you as a pacifist voter would strongly prefer. But suppose that the Republican candidate is inclined to drop the Big Bomb on Greenland, whereas the Democrat wants us to get out, say under some carefully negotiated and secured conditions. The pollsters indicate that the vote between Republican Claghorn and Democrat Fisbee is very close, so close that a preponderance of pacifist votes for Fisbee would probably lead to his winning; whereas if all the pacifists were to defect to Sedlitz—who we *know* cannot win—then Claghorn would win and turn Greenland into a radioactive desert. Hence a pacifist "throws away his vote" if he votes for Sedlitz instead of Fisbee. One who employs this more restrictive meaning of "throwing away your vote" does not rely solely upon the negligible probability of the Flat Earth-Vegetarian candidate's winning. He goes on to point out that if most of those who, on the merits, would be inclined to support Sedlitz in the particular forthcoming election, were to do so instead of supporting the lesser of the two evils between Claghorn and Fisbee, then the worse of the two evils, to wit, Claghorn, would win.

FEV: Well, I confess that seems to generate a possible exception to my general line of reasoning, but I am not sure about it at present. Since the individual voter's contribution even in that instance is still infinitesimal, and my arguments supra still hold, as qualified by the assumed special circumstances of the close race, I believe that we still have to postulate the acceptance of some overall ethico-political premise which has the four required properties axionomic, sociotropic, collective, and neutrofactual. The complication introduced by specifying Auerbach's condition is, as I see it, mainly one of how we characterize the collective reference class *indicatively* (as contrasted with counterfactual-subjunctively) in formulating our axionomic premise. Suppose one says, "If everybody voted as I am contemplating voting, it would be a good thing (so far as the Greenland war is concerned)." Or, what amounts to the same thing but spells it out a bit more, one may say, "If everyone were to think as I do, and (relying upon an axionomic justification) were to vote accordingly, it would be a good thing (so far as the Greenland war is concerned)." It would seem that either of these two ways of stating the counterfactual legitimates my going ahead with the intention of voting for F.E.V. candidate Pastor Sedlitz, following the general line of ethical reasoning I defend. That is, my expectation that most people won't proceed to do what I am individually about to do need not deter me.

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[13] I am indebted to Dean Carl A. Auerbach for calling this usage to my attention and pressing the argument for that case.

SOP: But in the light of the special political circumstance of a very close race between the atomic-incinerator candidate and the negotiated-slow-withdrawal candidate, I may prefer to formulate the neutrofactual (note, here we dare not say *counterfactual*) statement differently. Although it is known with near certainty that the strong pacifist candidate Sedlitz cannot win, it is not known with near certainty or even high probability which of the two major candidates will win. It is also known with near certainty that none of those who, like you, are currently contemplating voting for Pastor Sedlitz, would vote for Republican Claghorn if they went over to one of the two major parties; all such persons would, if they moved from the minor-party candidate to one of the two major candidates, vote for the Democrat Fisbee. On these empirical premises I therefore put to you the neutrofactual: "If, among those who feel as you do about the Greenland war, all (or most, or some sufficiently large number) were to vote for the Democrat Fisbee rather than for the Flat Earth-Vegetarian Sedlitz, Fisbee would win over Claghorn. And given the terrible importance of not incinerating the Eskimos, that is what persons who feel as you do should do under these special circumstances."

FEV: The difference between the formulations lies in the reference class taken as a basis. If we talk about what would happen *if* most people *were to* agree with my views, we get a different result ethically from what we get talking about what would happen if, *among all who already do agree with my views,* many (or most, or some sufficiently large number) were to vote as I am contemplating voting rather than voting as Auerbach would advocate voting under such circumstances.

SOP: I find his reasoning rather persuasive, don't you?

FEV: Yes, but not hammer-blow coercive. A detailed analysis of the ground upon which one chooses the reference class for the condition contrary to fact in formulating an axionomic ethical principle (especially in the political domain) would take us into another long dialogue. I shall content myself with saying that an Auerbach Case *may* yield an exception to the general position I take.

SOP: Assuming we can predict a sufficiently close race between the two major contenders, plus a negligible probability for the third party candidate's winning, combined with a high probability statement about which of the two major party contenders would receive votes by defectors from the third party support, what ethico-political neutrofactual statement is appropriate? How does one slice up the subpopulation of would-be third-party supporters? How does one decide with whom they can be appropriately conjoined for purposes of formulating an ethically applicable neutrofactual? How does one assign priorities to the forthcoming election's outcome versus longer term "teaching a lesson to the major parties?"

FEV: These seem to me extraordinarily difficult issues which, while they are probably more acute in the domain of political participation ethics, are not confined thereto.[14] Now what's your other question?

SOP: One of the nice things about economic theories of democracy like Downs's or Buchanan-Tullock's, which you have shown simply won't wash if we are to postulate egocentric rationality for the individual voter as economics postulates it for the economic man, is their liquidation of distinctively moral predicates from the conversation. Some scholars don't like this, but others believe that it has a conceptual advantage. Aren't you bothered by the regressive notion that political science cannot be handled without returning to what some would view as outmoded prescientific moral metaphysics?

FEV: I would answer that at two levels. First off, one does not himself need to subscribe to an axiological rule or principle in order to show how it must play a role in purported rational activity by those bipeds that are the subject of his investigation. Thus, for instance, I might as an anthropologist investigate the moral behavior of the Kwakiutl without holding their value system. I don't think that one can liquidate the subject of axiological principles or rules as part of political behavior and do empirical justice to the facts of the subject matter, any more than one can liquidate the concept of a grammatical rule in dealing with verbal behavior. Stones cannot think or value; human beings can. In other words, the super-behavioralist in political science seems to me to be in somewhat the same danger that Skinner is vis-à-vis the criticisms of linguists like Chomsky.

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[14] See, e.g., David Lyons, *Forms and Limits of Utilitarianism* (Oxford: Oxford University Press, 1965), especially chapter II, "Describing an action."

More importantly, however, I would say that this whole argument can be made hypothetical, and I would prefer to make it that way. I am not prepared to deliver the goods on a demand for a general philosophy of ethics, or even to provide a refutation of the positivist view that ethical statements are neither true nor false. In fact, I am very much open to the unpleasant possibility that among the various components of "classical Vienna Circle positivism,"—most of which have been abandoned, refuted, or modified almost beyond recognition, largely by the logical positivists themselves—the only one that still seems to have merit is their destructive criticism of the cognitive content of ethical statements. It may be that ethics is nonsense, except as an expression of taste. I don't like to contemplate that very much, as an ethical being. But it wouldn't bother me for the restricted purposes of the present discussion, since a showing that there is no such thing as a rational ethical obligation generally, while it would mean that my behavior in voting for Pastor Sedlitz and the Flat Earth-Vegetarian party was irrational, would also mean that your voting for Claghorn was irrational. So again we would both be in the same boat if ethics is garbage. The point is that the "thrown-away-vote" argument, as generally employed in American politics, presupposes that there is such a thing as *not* throwing away a vote, and I believe I have shown that you always "throw away your vote" unless you are behaving as an ethical animal. If ethics is baloney, and if there is no harm in failing to behave as an ethical animal, then it doesn't make sense to say that you "throw away your vote" on a minor third party candidate, since the only meaning to such an accusation of foolishness or irrationality would reside in a background presupposition that there is some way of voting in a presidential election that is *not* irrational, and the liquidation of ethics would liquidate such a possibility for all of us.

From Confusion to Confusion: Issues and the American Voter (1956–1972)*

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A Paradigm of Issue Voting

Back in those dark days of primitive data processing, before computers made light work of the heavy calculation of statistical formulae, those sturdy pioneers of behavioral research—Campbell, Converse, Miller, and Stokes—laid down some basic prerequisites for issue voting. First of all, they suggested, a citizen must have an opinion about a given issue. Second, he must also have some idea of government policy concerning that issue. Third, he must perceive some differences in the stands of the parties concerning the issue. Finally, he must care enough about how the issue is resolved to make the first three prerequisites relevant to his electoral considerations.[1]

Examining the electorate of 1956 on 16 selected issues, Campbell et al. found that on the average fewer than 30 per cent of those interviewed satisfied the first three prerequisites. (The range ran from 18 to 36 per cent.)[2] And since those who cared tended to be those most firmly committed to one party or the other, the proportion of the population basing its voting decision on any given issue was likely to be considerably lower than these modest figures. The distribution of opinions on current issues, they concluded, was just not very important for explaining the vote of the great bulk of the American electorate.

At the outset, it is important to note just what the authors of the *American Voter* did and did not say with regard to issue voting. Although voters' positions on the issues did not provide a very powerful explanation of actual votes cast, the relationship between individuals' positions on the issues and their votes was by no means random. On New Deal-Fair Deal concerns, such as underwriting of medical costs, aid to education or guaranteed employment, there was substantial agreement that the Democratic party was more favorable. On questions of desegregation, civil rights, and anti-communism, however, there was considerable disagreement among voters about the particular position each party favored. Voters tended to take an issue into account when the issue position related to intensely held personal values, i.e., when they cared about how the issue was resolved. It followed that on New Deal-Fair Deal issues, voters' concerns were easily translated into issue voting, for a majority of voters who strongly preferred the same alternative would vote for the same party. On other types of issues, however, voters' concerns would not translate into issue voting, for those sharing an identical issue position would not share identical perceptions of which party was in agreement with that position.[3]

To complicate the matter of issue voting even further, the authors pointed out that individual voters were concerned with only a subset of the major issues of the campaign. Only one-third of the voters, for instance, claimed to have positions on 14 of 16 issues, while approximately one-quarter admitted having no opinion on 9 or more issues. In addition, different groups of voters were by no means concerned with the same subsets of issues. For example, if voter A and voter B were each concerned with 10 issues, there was no guarantee that these would be the *same* ten issues. In fact, more likely than not, the overlap would correspond to the expectations of chance.[4] Finally, even on those issues for which voters' shared perceptions of party positions were accurate, voters' party identifications usually preceded, and thereby conditioned, the formation of the voters' opinions. As a result, voters' positions on the issues tended to agree with those of their party.[5]

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*My thanks to Paul A. Beck, Raymond Corrado, Charles O. Jones, David Nexon, Richard Niemi, Raymond Owen, Bert Rockman and two anonymous reviewers for comments on this paper. Special thanks to Raymond Hinnebusch for help with the data processing. The data were made available through the Inter-University Consortium for Political Research.

[1] Angus Campbell, Philip Converse, Warren Miller, and Donald Stokes, *The American Voter* (New York: John Wiley and Sons, 1960) pp. 170–174.

[2] *Ibid.*, p. 182.

[3] *Ibid.*, pp. 183–87.

[4] *Ibid.*, pp. 173, 178–79.

[5] *Ibid.* However, see Donald O. Searing, Joel Schwartz and Alden E. Lind, "The Structuring Principle: Political Socialization and Belief System," *American Political Science Review*, 67 (June, 1973), 415–32, for evidence that party identification did not serve very well to structure opinions of voters on issues in the 1968 election. In fact, Searing et al. did not find

But time marches on, and a new generation of political scientists has reexamined some of the propositions of the *American Voter* using data from decades other than the 1950s. Their work has resulted in a number of new insights. They tell us, for instance, that the 1950s were a somewhat unusual period. President Eisenhower's conscious projection of the role of President as being above the common political battles—as virtually nonpartisan—served to confound the electorate. Its failure to identify parties with issues in 1956 can be attributed to a significant degree to Eisenhower's incumbency. In the 1960s the new research uncovers increased correspondence of vote with issue position and increased understanding of the differences between the parties on the issues. Both can be attributed, at least in part, to the return of more partisan presidential politics.

It would seem that evidence for such developments could be presented appropriately in the form of simple tabulations of voters satisfying the *American Voter*'s prerequisites for issue voting over time. In place of such straightforward presentations, however, we find the results of sophisticated statistical routines or elaborate cross tabulations, in which the data analyzed may no longer represent the direct answers of the respondents but some more sophisticated measures derived from conceptually superior scales. Indeed, one oft-cited analysis fails to include so much as a single raw figure—not even an N upon which the interpretations presented are based![6]

Now no one can seriously gainsay the advantages of new and powerful statistical techniques. We all know they offer subtle insights which cruder, old-fashioned analyses fail to uncover. Yet, in applying them to issue voting, political scientists must proceed with caution, taking care that a fascination with discovering relationships between issues and voting among subgroups of the population does not overwhelm an appreciation of the relative importance or unimportance of these subgroups in comparison with others in the general population. Care must also be taken to assure that findings are a true reflection of actual relationships underlying the data, not merely a function of the techniques of measurement or modes of analysis employed.

These caveats come to mind when we note the growing body of literature presenting contradictory evidence of ongoing party "decomposition," i.e., diminishing loyalties to the major political parties and a growing sense of the parties' irrelevance to important societal problems.[7] To say the least, it seems prudent to remain somewhat suspicious of findings which suggest bigger and better associations of issues with parties (and with popular votes for parties) right at this time.

In light of these cautions, this paper reexamines the evidence contained in several leading monographs on issue voting published since 1966. This reexamination will suggest that voting in presidential elections in the late 1960s did not become as issue oriented as has been claimed. Furthermore, the evidence suggests that as we move into the 1970s, issues have continued to play only a relatively minor role as determinants of voting choice in presidential elections.

The Responsible Electorate?

The prodigious effort to detect issue voting derives major impetus from the publication in 1966 of V. O. Key's *The Responsible Electorate.*[8] In this book, subtitled *Rationality in Presidential Voting, 1936–60,* Key employed Gallup and NORC data to demonstrate that far from voting randomly with regard to issues, Americans tended to vote for those presidential candidates whose parties supported the voters' issue preferences. (The anti-Catholic voters in 1960 were the only major exceptions.)

Unfortunately, *The Responsible Electorate*'s demonstration of issue voting simply ignored the criteria for issue voting described in the *American Voter.* All the book really demonstrated was a simple bivariate relationship be-

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any set of political orientations that explained more than 15 per cent of the variance in voters' opinions on issues. (See Tables 5 and 6, p. 429.)

[6] Gerald M. Pomper, "From Confusion to Clarity: Issues and American Voters, 1956–68," *American Political Science Review,* 66 (June, 1972), 415–28.

[7] Cf. Walter Dean Burnham, *Critical Elections and the Mainsprings of American Politics* (New York: W. W. Norton, 1970), chapters 4–7; Walter Dean Burnham, "Theory and Voting Research: Some Reflections on Converse's 'Change in the American Electorate,'" *American Political Science Review,* 68 (September, 1974), 1002–1023; Arthur Miller and Warren Miller, *Time Series on Selected Variables,* mimeo (Ann Arbor: Center for Political Studies, March 1974) Tables 1-4; Herbert F. Weisberg and Jerrold G. Rusk, "Dimensions of Candidate Evaluation," *American Political Science Review,* 64 (December, 1970), 1167–85; Paul A. Beck, "A Socialization Theory of Partisan Realignment," in *The Politics of Future Citizens: New Dimensions in Socialization,* ed. Richard Niemi (San Francisco: Jossey-Bass, 1974); Norman H. Nie, Sidney Verba, and John R. Petrocik, *The Changing American Voter* (Cambridge, Mass.: Harvard University Press, 1976), chapters 4, 10.

[8] V. O. Key, Jr., *The Responsible Electorate* (Cambridge, Mass.: Harvard University [Belknap] Press, 1966).

tween issue position and vote. Key never asked if voters perceived the party they voted for to be in agreement with them on any given issue. Nor did he examine whether those who voted for a party with a particular issue position in fact identified with that party to begin with—let alone examine the effects of any more subtle control variables. Moreover, Key failed even to check whether or not voters cared about the issues for which he claimed they voted.

That Key's empirical analysis should contain such obvious inadequacies should not surprise us. After all, Key had died in October 1963, having completed only the first two chapters of the book. It was Milton C. Cummings, Key's former student, who, working from the unfinished drafts, managed to complete the remaining chapters. To make certain the book reflected Key's thought rather than his own, Cummings wisely refrained from embellishing Key's original interpretations. While this decision preserved the integrity of Key's unfinished thought, much of the subtlety that could have been brought to the final interpretation of the data was undoubtedly lost.

Published as a posthumous tribute to the great man, the book received overwhelming advance acclaim—not only a eulogistic foreword by Arthur Maass, but dust-jacket salutes from both David Truman and Robert Dahl, then president and president-elect of the American Political Science Association. Publication of a less than flattering review at this juncture would have been imprudent, if not unseemly.[9] *The Responsible Electorate* had been declared an unqualified success, and V. O. Key had been proclaimed a champion of democracy.

Forgotten was the V. O. Key who, only two years before his death, had characterized elections as less than rational exercises:

> In truth, the more I study elections the more disposed I am to believe that they have within themselves more than a trace of the lottery. That of course, is not necessarily undesirable so long as all concerned abide by the toss of the coin. Be that as it may, the moral for our purposes is plain. Even when the public in manifest anger and disillusionment throws an Administration from office, it does not express its policy preferences with precision. The voice of the people may be loud, but the enunciation is indistinct.[10]

Forgotten was the V. O. Key who believed "the health of a democratic order consists in the beliefs, standards, and competence of those who constitute the influentials, the opinion-leaders, the political activists in the order,"[11] the V. O. Key who thought democracies decayed "not because of the cupidity of the masses but because of the stupidity and self-seeking of the leadership echelons."[12] Whereas Key had previously been attacked as an elitist,[13] he was henceforth to be praised for having rediscovered the rational good sense of the common man.[14] In death he became transfigured from elitist to democrat.

Yet in spite of its failure to measure up to the hyperbole of its reviewers, *The Responsible Electorate* remains an important book because of what Key *tried* to demonstrate, even if that demonstration was not entirely successful. Key's publication came at a time when several theories of pluralistic democracy—each denigrating the importance of the supportive role of the average citizen—had received wide currency.[15] These theories were largely based upon interpretations of survey research data on the electoral behavior and the opinions of mass publics. *The Responsible Electorate* was the first important work to use a mass data base to emphasize the essential rationality of the individual's voting choice.

In the years following Key's publication a host of books and articles have appeared concerning issue voting.[16] With few exceptions, these works have shown the average citizen in a more favorable light than had those which preceded Key. *The Responsible Electorate* played no small part in this resurrection of the rational voter.

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[9] See Angus Campbell, "The Responsible Electorate" (review), *American Political Science Review*, 60 (December, 1966), 1007–08, for an example of a careful, gentle critique.

[10] V. O. Key, "Public Opinion and the Decay of Democracy," in *Political Opinion and Electoral Behavior: Essays and Studies*, ed. E. C. Dreyer and W. A. Rosenbaum (Belmont, Cal.: Wadsworth Publishing Company, 1967), p. 417. The essay originally appeared in the *Virginia Quarterly Review*, 37 (1961), 481–94 with quotation at page 487.

[11] V. O. Key, *Public Opinion and American Democracy* (New York: A. A. Knopf, 1961), p. 558.

[12] *Ibid.*, p. 557.

[13] Cf. Peter Bachrach, *The Theory of Democratic Elitism: A Critique* (Boston: Little, Brown and Company, 1967), pp. 47-48; Jack L. Walker, "A Critique of the Elitist Theory of Democracy," *American Political Science Review*, 60 (June, 1966), 286–287.

[14] Cf. Peter B. Natchez and Irvin C. Bupp, "Candidates, Issues, and Voters," in Dreyer and Rosenbaum (2nd ed. 1970), pp. 433–34. The article is from *Public Policy* (1968), 409–437; Dennis F. Thompson, "The Use of Social Science in Democratic Theory," in *Frontiers of Democratic Theory*, ed. Henry S. Kariel (New York: Random House, 1970), p. 408.

[15] Cf. Bernard Berelson et al., *Voting* (Chicago: University of Chicago Press, 1954), chapter 14; Gabriel Almond and Sidney Verba, *The Civic Culture* (Boston: Little, Brown, 1965), chapter 13; Herbert McClosky, "Consensus and Ideology in American Politics," *American Political Science Review*, 58 (June, 1964), 361–82.

[16] See John H. Kessell, "Comment: The Issues of Issue Voting," *American Political Science Review*, 66 (June, 1972), 459 for a listing of these publications.

Three Demonstrations of Issue Voting: A Critique

As previously indicated, however, to show an association between voters' opinions and their votes—Key's demonstration of rationality—does not demonstrate issue voting. As described in *The American Voter,* issue voting requires that voters be aware of the differences between the parties and that they care about these differences. In this section, therefore, I shall examine three articles which claim to present more satisfactory evidence of issue voting. These are Gerald Pomper's "From Confusion to Clarity: Issues and the American Voter 1965–68,"[17] David RePass's "Issue Salience and Party Choice,"[18] and Arthur Miller et al. "A Majority Party in Disarray: Policy Polarization in the 1972 Election."[19]

Let us begin with Pomper's paper. Basically Pomper argues that between 1956 and 1968 not only was there a considerable increase in the correlation between party identification and opinion on five of six issues asked about in each presidential election but also there was an increase in the voters' perceptions of party differences on these issues, the Democrats becoming increasingly more identified with the liberal position. To demonstrate these points Pomper presents three tables. The first shows the percentages of various types of party supporters holding the liberal position on each issue over the period covered. It employs gammas to demonstrate the extent to which the association of "Democraticness" with liberalness increases over the years. The second presents the percentages of these same types of party supporters who hold opinions and perceive party differences on the issues. The third stipulates the percentages of those in the second table who identify the Democrats with the liberal position.[20]

Conceptually, the first table is the weakest. Resembling Key's analysis, it contains nothing but a series of simple bivariate correlations between party identification and issue position. It says nothing about whether or not the respondents care about the issues, have any idea of what the government is doing about them, or indeed recognize any differences between the parties on the issues. In short, it demonstrates little about actual issue voting. In fact, as John Kessel has shown, the correlations discovered are to a significant extent explained by the respondents' attitudes toward the increasing power of the federal government.[21]

The other two tables are more relevant to issue voting. They contain those voters who satisfy the prerequisites of having an opinion and perceiving differences in the party positions on each given issue. The problem involved with these tables, however, is that Pomper never reveals the N's upon which they are based. As a result, it is impossible to tell the extent to which those voters included in the tables were typical of the general population.

Table 1 below contains a recalculation of the "total sample" rows of Pomper's Tables 2 and 3 basing the percentages on the N's for the entire sample rather than upon the N's for those having opinions and perceiving differences among the parties. The points involved are fairly obvious. Far from showing that in the 1960s the great bulk of the population came to perceive party differences, or for that matter that they came to agree upon these preferences, the table shows more modest increases in awareness of and polarization on the issues. Instead of reaching the proportions in the 70s and 80s displayed by Pomper, the decade in question yields proportions in 40s and 50s, the largest proportion of citizens having an opinion and perceiving party differences on any issue being 58 per cent on medical care in 1964. Moreover, the increases in awareness of party differences between 1956 and 1960 can be seen largely as an artifact of the survey schedule arising from the elimination of a filter question concerning each respondent's awareness of government policy in the issue area.

It can be argued, however, that the above criticisms are rather superficial. After all, Pomper's data indicated a trend. Even if his method of percentaging produced somewhat larger estimates of awareness and polarization than those we calculate, his point remains that

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[17] Pomper, "From Confusion..." See also Gerald M. Pomper "Toward a More Responsible Two-Party System, What Again?" *Journal of Politics,* 33 (November, 1971), 916–40.

[18] David E. RePass, "Issue Salience and Party Choice," *American Political Science Review,* 65 (June, 1971), 389–400.

[19] Arthur Miller, Warren Miller, Alden Raine, and Thad Brown, "A Majority Party in Disarray: Policy Polarization in the 1972 Election" (a paper delivered at the 1973 American Political Science Association Meeting, New Orleans). A revised version of this paper appears in the *American Political Science Review,* 70 (September, 1976).

[20] For the reader's convenience, these tables are reproduced in the appendix to this paper.

[21] Kessel, p. 463.

Table 1. Issues and Party Positions, 1956–68
(a recomputation of Pomper's data based on the entire SRC samples).

	Aid to Education				Medical Care				Job Guarantee			
	1956[a]	1960	1964	1968	1956[a]	1960	1964	1968	1956	1960	1964	1968
Democrats Favor	17%	32%	37%	31%	15%	35%	55%	46%	19%	39%	46%	42%
Republicans Favor	13	10	8	11	11	8	3	5	14	9	6	10
No Difference	28	34	25	30[b]	22	31	15	21	25	27	23	27
Don't Know	10	13	12	9	11	18	9	10	8	15	10	10
DK Gov't Policy	23	[c]	[c]	[c]	29	[c]	[c]	[c]	23	[c]	[c]	[c]
No Opinion or No Interest on Issue	10	11	18	19	12	9	16	16	10	10	15	11
N	1749	1898	1563	1553	1749	1905	1559	1541	1750	1893	1564	1541
	Fair Employment				School Integration				Foreign Aid			
Democrats Favor	13%	18%	47%	41%	18%	15%	43%	42%	12%	14%	39%	31%
Republicans Favor	15	17	6	9	16	11	5	7	16	15	5	6
No Difference	29	44	25	31	30	46	28	32	29	46	36	42
Don't Know	10	11	9	7	11	14	12	7	10	13	10	8
DK Gov't Policy	19	[c]	[c]	[c]	13	[c]	[c]	[c]	16	[c]	[c]	[c]
No Opinion or No Interest on Issue	13	11	13	12	12	15	13	11	16	12	11	13
N	1742	1910	1553	1550	1740	1913	1568	1542	1748	1900	1560	1553

[a] In 1956 question of Aid to Education and Medical Care referred to which party was closer to the respondent's own position.

[b] Includes 1% who said Wallace favored aid.

[c] No question on whether R had information about government policy was asked after 1956.

(Source: Inter-University Consortium for Political Research: SRC/CPS American National Election Studies)

there was less confusion about party positions in 1964 and 1968 than in 1956 and 1960.

But unfortunately for Pomper's argument, these trends, to the extent that they are measurable, have not continued into the 1970s. Table 2 contains the results obtained on the only three questions used by Pomper which were asked again in comparable form in 1970 or 1972. When asked which party was more likely to do what respondents wanted, fully 48 per cent of those interviewed in 1970 saw no differences between the parties on the question of school integration. When those who didn't know the party positions or who had no opinion on the issue are added to the "no difference" group, we are left with 34 per cent, barely one-third of the population, who had any possibility of voting on the basis of this issue. This is exactly what the percentage was in 1956—only the 1956 figures included a filter question concerning the respondents' awareness of government policy. Thus, if anything, perceptions of party difference on school integration were less common in 1970 than they were in 1956.

Regarding medical care and job guarantees, the results are less directly comparable with Pomper's, for the questions were asked in an entirely new format. Nonetheless, to the extent they are comparable, the data suggest that the trend toward greater awareness of party difference has reversed itself for the former issue, and has not continued for the latter. Only 36 per cent of the sample had any notion that the Democrats favored or opposed national health insurance; only 37 per cent had any notion about the Republicans' position on this matter. On job guarantees the awareness of party positions was somewhat higher: 54 and 52 per cent of the electorate could attribute a position to the Democratic and Republican parties, respectively. Although the electors still perceived the Democrats to be more liberal on each of these issues, comparison of their estimates of the party positions indicates that the proportion of the electorate who perceived any difference at all between the parties fell to 30 per cent for medical care (national health insurance) and 46 per cent for job guarantees.[22]

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[22] The change of focus of the medical care question from government "to help people get doctors and hospital care at low cost" to "government insurance plan which will cover all medical and hospital expenses" further militates against exact comparisons with Pomper's data. The question of job guarantees, however, is more nearly comparable. Its 1972 form maintains phrases identical to those used in 1964 and 1968 to identify the issue as "the government in

Table 2. Pomper's Issues in the 1970s

School Integration 1970[a]	
Democrats Favor what R wants	23%
Republicans Favor what R wants	11
No Difference	48
Don't Know	5
No Opinion	12
N	882

	Estimates of Party Positions (1972)			
	Medical Care[a]		Job Guarantee[a]	
	Dems	Reps	Dems	Reps
Favor	29%	11%	46%	17%
Neutral	12	13	15	17
Oppose	7	26	8	25
Don't Know	32	31	17	18
No Opinion	18	18	14	14
N	1354	1354	1360	1360

	Comparison of Estimates of Party Positions (1972)	
	Medical Care	Job Guarantee
Democrats More Favorable	26%	39%
Republicans More Favorable	4	7
No Difference	19	20
Don't Know	33	20
No Opinion	18	14
N	1354	1360

[a] In 1970 the school integration question asks which party will do what respondents want, not, as in previous years, which party favors school integration. In the 1972 study respondents were asked to rank the party positions from 1 to 7 from strongly favor to strongly opposing the policy. 1 to 3 indicates favor. 5 to 7 indicates oppose. 4 is neutral.

(Source: Inter-University Consortium for Political Research: SRC/CPS American National Election Studies)

As we look for trends, however, we need not stick to the six issues Pomper chose. Table 3 contains results for three issues that became important in the late 1960s and early 1970s but which were not asked about before 1964. These issues are nondiscrimination in places of public accommodation such as restaurants and hotels, the strength and power of the government in Washington, and the United States policy in Vietnam. It is abundantly clear that for the first two issues the trends of voters' perceptions of party positions run from perceptions of more polarization to perceptions of less polarization, and from greater recognition of party differences to lesser recognition. Only for the Vietnam issue does the electorate perceive greater polarization of the party positions as compared with 1968; and even here only between 48 and 51 per cent actually perceive any party differences, a slight decrease, if anything, from 1968.

On the basis of analysis so far, therefore, we may conclude the following: (1) The trends toward increased awareness of party differences and increased polarization between the major parties on issues of the 1950s and 60s were not as strong as Pomper suggested; (2) The trends, however modest, are not found to continue into the 1970s.

But given another view of issue voting, these conclusions could make no difference. After all, who says that issues selected in this or Pomper's paper are salient to the average voter? In fact, who says that *any* given issue is salient to a majority of voters? A majority of voters may vote on the basis of issues which each considers of importance to himself, but there may be no single issue upon which a majority has strong feelings.

We need only turn to David RePass's paper for a demonstration of the relationship of party choice to those issues spontaneously named by respondents as being "on their mind a lot" or causing "extreme worry" to them in 1964. For each issue named in this manner, more than 60 per cent, on the average, saw party differences. This, despite the fact that no single issue was named by more than 12 per cent of the respondents.

Washington should see to it that every person has a job and a good standard of living."

Table 3. New Issues of the 1960s and 70s

Which Party Favors Integration of Restaurants, Hotels, and Other Places of Public Accommodation?

	1964	1968	1970
Democrats	61%	45%	31%
Republicans	3	7	8
No Difference	30	40	52
Don't Know	6	8	8
No Answer	–	1	1
N	1274	1372	784

Which Party Favors a Strong Government in Washington?

	1964	1968	1972
Democrats	35%	30%	16%
Republicans	8	12	18
No Difference	19	25	32
Wallace	–	1	–
Don't Know	8	6	5
Not Interested	31	27	29
N	1565	1552	1261

Which Party Favors a Stronger Stand on Vietnam?

				1972[b]	
	1964[a]	1968	1970[a]	Pre-election	Post-election
Democrats	17%	18%	21%	6%	5%
Republicans	19	34	16	42	46
No Difference	44	27	54	25	22
Wallace	–	3	–	–	–
Don't Know	14	18	9	18	18
No Opinion	7	–	–	9	9
N	1274	1548	1495	1331	1115

[a] In 1964 and 1970 R's were asked which party was more favorable to R's position on Vietnam.

[b] In 1972 questions on Vietnam were in an entirely different format based on a seven-point scale from immediate withdrawal to complete military victory.

(Source: Inter-University Consortium for Political Research: SRC/CPS American National Election Studies)

RePass goes on to demonstrate that when voters' attitudes on several salient issues leaned toward one party, they were likely to vote for that party, regardless of their own party identification. Furthermore, when he compares the effects of party identification and issue orientation (controlling for each other's effects and for the effect of candidate orientation), RePass makes the astonishing discovery that *"salient issues had almost as much weight as party identification in predicting voting choice."*[23]

These remarkable results deserve closer examination. Table 4 presents a straightforward tabulation of the views of citizens about which party is likely to do what they want on the most important issue which they think government should take care of. The results are obvious. Once all the respondents interviewed (minus those giving no answers) are considered, it becomes clear that fewer than 50 per cent could distinguish differences between the major parties on the issue that concerned them most in 1972. And for 1964 and 1968 the figures were hardly better—bare majorities of 54 and 51 per cent. On the other hand, the percentage of voters who clearly report seeing no difference between the parties has nearly doubled from its low of 21 per cent in 1965 to a high of 41 per cent in 1972.[24] These figures for "salient" issues are no more impressive than those given for the possibly "nonsalient" issues

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[23] RePass, p. 400.

[24] Not only do increasing proportions of the electorate as a whole report "no difference" between the parties on the most salient issues over time, but so do increasing proportions of southern whites, nonsouthern whites and (since 1964) blacks. Moreover, these increasing proportions are found regardless of the specific content of the most salient issue. See Merle Black and George Rabinowitz, "An Overview of American Electoral Change: 1952–1972" (a paper delivered at the Southern Political Science Association, New Orleans, November 1974), tables 10–12.

Table 4. Most Important Problem Government Should Take Care of: Which Party has Best Position?

	1964	1968	1972	1964[a]
Democrats	35%	21%	23%	7%
Republicans	19	30	24	16
No Difference	21	35	41	16
Don't Know	7	10	8	7
Wallace	–	2	1	–
No Problem	18	3	3	54
N	1565	1547	1066	1556

[a] In 1964 the SRC also asked respondents which was the most important problem government should stay out of. Results to this question are included alone in Column 4 above. RePass's analysis is based upon combining the answers of those who name a problem in Column 1 or Column 4.

(Source: Inter-University Consortium for Political Research: SRC/CPS American National Election Studies)

in Tables 1 through 3. Why then do RePass's findings *seem* so impressive?

The simple answer is that he has misinterpreted the significance of his data. If one examines the 1500 or so respondents interviewed in 1964, it soon becomes apparent that strong Democrats who have a high Republican issue orientation do indeed vote Republican—all five of them do![25] In fact, of the 30 strong or weak Democrats with medium or high Republican issue orientations some 22 of them (more than 70 per cent!) vote Republican. Similarly of the 18 strong or weak Republicans with high or medium Democratic issue orientations, 14 (70 percent) vote Democratic. The point of course is that such results say nothing about 97 percent of the respondents.

The effects of the small N's become abundantly clear when RePass reveals the beta weights upon which he bases the italicized conclusion (cited above) that salient issues are nearly as strong as party identification in determining voting choice: Party identification has a beta weight of .27; issue partisanship has a weight of .23. In plain English issue partisanship accounts for 5.3 percent of the variance in presidential voting choice compared to 7.4 percent for party identification. What RePass demonstrated, therefore, was that the vote of approximately one person in 20 could be accounted for by issue orientation in 1964.[26] Such a figure suggests that the electoral impact of issue voting had not changed significantly from that described in *The American Voter* for 1956.

To recapitulate: our main criticism of RePass is not that he failed to find instances of issue voting.[27] Rather, it is that he failed to gauge the relative importance of the impact of this type of voting on the electoral outcome.

Miller et al. claim to have found evidence of a stronger impact of issue voting in their study of the 1972 election. To begin with, this study deals with an election in which, unlike 1964, the main defections come from the majority party. And in this case the defections are massive: some 40 per cent of all Democrats who turned out switched to Nixon. Moreover, to a remarkable extent the distribution of the issue positions of these Democrats matches the distribution of issue positions of other Nixon voters and contrasts with that of McGovern voters. Still because of low turnout the 40 per cent represented fewer than one quarter of all Democrats. And it hardly needs repeating that correspondence of issue positions with voting patterns is no guarantee that the former caused the latter. Miller et al. feel constrained, therefore, to offer firmer evidence of issue voting.

First of all they employ a normal vote analysis developed by Richard Boyd for his study of the 1968 presidential election.[28] In order to separate the effects of issue position from those of partisanship Boyd developed two measures of effect: a long-term component and short-term component. The long-term component represents the average deviation from the population proportion expected on the basis of a calculation of the normal vote for each issue position. The short-term component measures the average deviation of the observed vote from the predicted normal vote for each issue position.[29] On the basis of these measures issues may be characterized as to the extent to which their impact is independent of that predicted by the normal vote (and hence party identification). For instance, for the 1968 election Boyd found that on issues like federal guarantees of civil rights and social welfare the long-term component accounted for most of the variation. On the other hand, for issues like the war in Vietnam, and urban unrest, the short-term component was stronger, an indication of the independence of these issues from traditional party ties.

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[25] RePass's Table 6 is included in the appendix.

[26] By the same token, the explanatory power of party identification—accounting for the votes of approximately one person in 14—is by no means impressive. See the discussion of Miller et al. below for comparable figures on the decline of the power of party identification as an explanatory factor in accounting for presidential votes.

[27] Indeed, a large number of RePass's voters satisfy at least three of the four prerequisites for issue voting discussed above: (1) they have an opinion on the given salient issue; (2) 60 per cent perceive differences between the parties on the issue; (3) and by definition of salience, they all care about how the issue is resolved. We lack information only about whether or not the voters are aware of the current government policy concerning the issue.

[28] Richard W. Boyd, "Popular Control of Public Policy: A Normal Vote Analysis of the 1968 Election," *American Political Science Review*, 65 (June 1972), 429–49.

For 1972 Miller and his co-authors find that the short-term component has at least twice the magnitude of the long-term component for a variety of issues, including withdrawal of troops from Vietnam, granting of amnesty, legalization of marijuana, quelling campus unrest, giving government help to minorities, and pushing for continued desegregation. In addition, even for composite scores across four general issue dimensions–social, economic, cultural, and war-related–the magnitudes of the short-term components exceed those of the long-term ones. Furthermore, the relationship between issue positions and observed votes holds even when partisanship is controlled. The distribution of votes is associated with the issue positions of the voters, regardless of the voters' partisan identifications.

Have we at last discovered a presidential election where issue position accounts for most of the variance in voting behavior? Unfortunately not. And it is Miller et al. themselves who show us why not.

If one seeks to demonstrate issue voting, Boyd's normal vote analysis contains a major (perhaps fatal) flaw–it aggregates responses–and it thereby reduces the variance to be explained.[30] For example, on the issue of Vietnam withdrawal, some 2240 responses are aggregated into seven proportions, each representing respectively the normal vote prediction for respondents in each of seven issue positions. With only seven data points to explain, issue position does rather well as a predictor, yielding a linear correlation of .936. The same sorts of high correlations hold true for virtually all of the issues with strong short-term components. But what happens when the votes are disaggregated, when issue positions are employed to explain the behavior of each individual separately, over two thousand data points, instead of only seven?

Quite simply the bottom falls out of the association between issue position and vote. When placed in a nine-variable multiple regression analysis, all four issue clusters together explain only three per cent of the variance.[31] Indeed, even when higher educated voters alone are considered–the group we would expect to be the most issue oriented–the explanatory power of issue position does not improve. Most variance is explained by voters' subjective ratings of the candidates as measured by a "thermometer" scale of 0 to 100, and for the higher-educated and most active (age 30–59), also by self ratings on a liberal-conservative scale. The only important point about issue position is that it provides at least as strong–and usually stronger–explanations of variance in voting as does party identification. Accepted on its own terms, then, the analysis presented by Miller et al. fails to demonstrate the importance of issues relative to other variables as determinants of the vote.

Beyond this, however, the validity of the entire multiple regression analysis remains open to question. What is the theoretical justification for throwing together four issue clusters, two candidate thermometers, ideological identification, party identification, and estimates of campaign performance into one regression equation? Is it not likely that some of the issue items will be correlated with some of the other independent variables, or that some of these latter variables will be highly correlated with one another?[32] In short, is there not likely to be so much multicollinearity in this nine-variable system as to suggest that no valid conclusions at all can be drawn about the amount of variation each variable explains? An effort to develop a theoretically justified recursive regression system—if not a complete return to the *American Voter*'s conceptually direct formulation of issue voting—would seem more appropriate.[33]

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[29] These are weighted averages: see Boyd, pp. 448–49.

[30] See Philip Converse, "Of Time and Partisan Stability," *Comparative Political Studies,* 2 (1969–70), 162–63 for another discussion of this point.

[31] The relevant figures are reproduced in the appendix.

[32] On the correlation of issue position with candidate orientation and party identification, see Angus Campbell and Donald E. Stokes, "Partisan Attitudes and the Presidential Vote," *American Voting Behavior,* ed. Eugene Burdick and Arthur Brodbeck (Glencoe, Illinois: The Free Press, 1959), pp. 353–371; John E. Jackson, "Issues, Party Choices and Presidential Votes," *American Journal of Political Science,* 19 (May 1975) 161–85; and Samuel Kirkpatrick, William Lyons, and Michael Fitzgerald, "Candidates, Parties and Issues in the American Electorate: Two Decades of Change," *American Politics Quarterly,* 3 (July 1975), 315–339. On the association of ideological identification with party identification, see Lloyd A. Free and Hadley Cantril, *The Political Beliefs of Americans: A Study of Public Opinion,* (New Brunswick, N.J.: Rutgers University Press, 1967), pp. 137–142. In the revised version Miller et al. employ a

What is to be Done?

In a recent issue of the *American Political Science Review* Stanley Kelley, Jr. and Thad Mirer present a brilliantly conceived empirical study of the voting decision.[34] Their conception is brilliant for its simplicity. Instead of moving immediately to cleverly devised scales which summarize the respondents' attitudes, they attempt to squeeze all the information they can directly from the respondents' answers. Modifying the original notion of components of electoral decision developed by the SRC, the authors suggest the following:

> The voter canvasses his likes and dislikes of the leading candidates and major parties involved in an election. Weighing each like and dislike equally, he votes for the candidate toward whom he has the greatest net number of favorable attitudes, if there is such a candidate. If no candidate has such an advantage, the voter votes consistently with his party affiliation, if he has one.[35]

Applying this rule to SRC samples for presidential elections from 1952 to 1968, Kelley and Mirer are able to make predictions about the vote of more than 98 per cent of the respondents. But more importantly, they are able *to predict correctly* the voting choice of from 85 to 90 per cent of these respondents in the years 1952–64 and of 82 per cent of them in 1968. The comparable figures we calculate for 1972 are: predictions for 99 per cent with 87 per cent correct.)[36] Not only are these levels of accuracy impressively high as compared to those of the studies discussed above, they are achieved through actual predictions from pre-election interview data as opposed to explanatory *a posteriori* predictions generated from regression analyses using postelection reports.

The Kelley-Mirer rule has important theoretical underpinnings as well. After all, if there is one thing that political scientists can state with certainty, it is that politics are definitely not a central concern of the average citizen. As a consequence, the Kelley-Mirer rule which characterizes voting as a simple act, entailing little agony of decision for the average voter, makes theoretical sense. It assumes that the average voter entertains little expectation that those for whom he votes will touch his life directly. He simply adds up his likes and dislikes about the candidates and parties in an offhand manner, and he then arrives at a voting decision.

Both RePass and Miller et al. in fact achieve their highest predictive successes when they employ decision rules similar to this one. RePass's measure of partisan issue orientation, which was so successful in predicting direction of votes even against party identification, was nothing more than the sum total of salient issues for which the voter was favorable to one party minus those for which he was favorable to the other. Similarly Miller's most successful predictors of voting choice, the candidates' thermometer ratings, were positively correlated with summary measures of the likes and dislikes voters had about the candidates.[37]

"stage-wise" multiple regression. This controls the order in which variables enter the equation, but it still does not solve the problem of multicollinearity. See Norman Draper and Harry Smith, *Applied Regression Analysis* (New York: Wiley, 1966), pp. 173–77.

[33] Mark A. Schulman and Gerald M. Pomper develop such a system in "Variability in Electoral Behavior: Longitudinal Perspectives from Causal Modeling," *American Journal of Political Science*, 19 (February, 1971), 1–18. Using a seven-variable theoretical model based upon that presented by Arthur Goldberg, ("Discerning a Causal Pattern Among Voting Data," *American Political Science Review*, 60 [1966] 913–922), Schulman and Pomper demonstrate that the influence of issue position upon the vote has not only increased in magnitude from 1956 to 1972 but that it has also increased relative to the influence of party identification. Nonetheless, the influence of issues remains rather modest: the direct path coefficient from issue position to vote increases from .060 in 1956 to .223 in 1972; the indirect coefficient increases from .042 to .108; and the entire seven-variable regression explains 55 per cent of the variance in 1956 but only 48 per cent in 1972. Also: Cf. John E. Jackson *(passim)* for a nonrecursive model, which, though based only upon the 1964 SRC election data nonetheless argues that issue position causally precedes party affiliation.

[34] Stanley Kelley, Jr. and Thad W. Mirer, "The Simple Act of Voting," *American Political Science Review*, 67 (June, 1974), 572–91.

[35] *Ibid.*, p. 574. If the voter does not favor any candidate and does not have a party affiliation, no prediction is made. For a logically similar model (with similarly high accuracy) based upon candidate thermometer ratings, see Richard A. Brody and Benjamin Page, "Indifference, Alienation, and Rational Decisions: The Effects of Candidate Evaluations on Turnout and the Vote," *Public Choice*, 15 (Summer, 1973), 1–19.

[36] Among those whose net likes and dislikes did not cancel each other out, the accuracy of prediction was 89 per cent; among other voters—those whose votes were predicted on the basis of party identification alone—it was only 56 per cent. Calculations are made from 1972 SRC/CPS election study data.

[37] The net sum of likes and dislikes of Nixon correlated with his thermometer ratings at .65 (least

Even though the simple voting decision rule propounded by Kelley and Mirer is a far better predictor than any of the measures of issue position examined above, it most certainly does not obviate all further attempts to measure the impact of issue voting on elections. To begin with, the Kelley-Mirer rule mixes together all sorts of considerations: issue positions, candidate evaluations, party images, and the interests of groups with whom the voter identifies. If we seek a more detailed explanation of the vote predicted by the rule, therefore, it still seems worthwhile to attempt to assess the relative importance of each of these components. Furthermore, the rule does not take into account two recent developments which increase the potential for issues to have a greater impact on elections.

The first is the growth of feelings of distrust of and inefficacy toward government which have been documented by several recent studies.[38] Among other things these feelings are associated with lower turnout in elections and the eschewing of identification with either of the major political parties. While growth of these feelings has been most prevalent among the young, it has extended to all segments of the population. It is possible that those with feelings of low trust and low efficacy with regard to government share some common (but as yet undiscovered) issue positions, for which the existing political parties offer no suitable means of expression.[39] At any rate, as identified by Miller et al. in 1972, this low trust-low efficacy group contained 44 per cent of all citizens. In comparison to high trust-high efficacy citizens (27 per cent of the population), this group reported a lower turnout (62 per cent versus 89 per cent) but a higher vote for McGovern (49 per cent versus 21 per cent).[40]

The second development is that in recent elections, voters have shown an increased level of consistency among their opinions about issues and candidates.[41] This emergence of an "ideology" among voters, however, has not been associated with their traditional political loyalties.[42] Thus, although the inability of voters to relate issues to party positions is conspicuous at present, a base of ideologically sophisticated voters lies dormant, perhaps just

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squares linear correlation). Net likes and dislikes of McGovern correlated with McGovern's thermometer ratings at .63. Net likes and dislikes of parties and candidates correlated with thermometer ratings of Nixon and McGovern at .62 and −.64 respectively. (N = 1372). Of course if we choose to correlate net likes and dislikes with *mean* thermometer ratings, we can do much better. (See the argument on pages 38–39 above.) Net likes and dislikes of Nixon correlate with his mean thermometer ratings at .98! The same high level of association is found between net likes and dislikes of McGovern and his mean thermometer ratings. (N = 7 in both cases). Overall, the net likes and dislikes of candidates and parties correlate with mean Nixon and mean McGovern thermometer ratings at .96 and −.98 respectively (N = 25).

[38] Cf. Philip E. Converse, "Change in the American Electorate," *The Human Meaning of Social Change*, ed. Angus Campbell and Philip Converse (New York: Russell Sage Foundation, 1972); Louis Harris and Associates, *Confidence and Concern: Citizens View American Government* (a report to the Subcommittee on Intergovernmental Relations of the U.S. Senate Committee on Governmental Relations) (Cleveland: Regal Books/King's Court Communications, 1974), chapter 1; Arthur Miller, "Political Issues and Trust in Government: 1964–1970," *American Political Science Review*, 67 (September, 1974), 951–72; Lewis Lipsitz, "On Political Belief: The Grievances of the Poor," in *Power and Community: Dissenting Essays in Political Science*, ed. Philip Green and Sanford Levinson (New York: Vintage Books, 1970), pp. 142–72; Sidney Verba and Norman Nie, *Participation in America*, (New York: Harper and Row, 1972), chapter 20; M. Kent Jennings and Richard Niemi, "Continuity and Change in Political Orientations: A Longitudinal Study of Two Generations," *American Political Science Review*, 69 (December, 1975), 1316–1335.

[39] Both Miller, "Political Issues and Trust. . . ," and his critic, Jack Citrin, "Comment: The Political Relevance of Trust in Government," *American Political Science Review*, 68 (September, 1974), 973–988, agree that those with low trust in government tend to see the major political parties as failing to offer suitable policy choices. Although neither suggests that citizens with low trust currently share common issue positions, a common hostility to the status quo could provide the base for public support of an issue not yet put forward. Alliances of the cynics of the left and right (Miller's interpretation), or of partisan groups dissatisfied with current policies (Citrin's interpretation), formed in order to bring down moderate centrist governments are not uncommon. Weimar Germany, the French Third and Fourth Republics, and Italy's current government provide classic examples. In the United States the McCarthy victory over Johnson in the 1968 New Hampshire primary is attributed to exactly such an alliance. Those who voted against the Johnson delegates were both hawks and doves who objected to the "moderation" of the Johnson administration's policies toward the war in Vietnam. Moderation itself had become an issue. Cf. Philip E. Converse, Warren E. Miller, Jerrold G. Rusk, and Arthur C. Wolfe, "Continuity and Change in American Politics: Parties and Issues in the 1968 Election," *American Political Science Review*, 63 (December, 1969), 1092–93.

[40] Miller et al., Table 4

[41] Cf. John Osgood Field and Ronald E. Anderson, "Ideology in the Public's Conceptualization of the 1964 Election," *Public Opinion Quarterly*, 33 (Fall, 1969), 380–98; John C. Pierce, "Party Identification and the Changing Role of Ideology in American Politics," *Midwest Journal of Political Science*, 14 (February, 1970), 25–42; Norman Luttbeg, "The Structure of Beliefs Among Leaders and the Public," *Public Opinion Quarterly*, 32 (Fall, 1968), 398–409; Norman Nie and Kirsti Andersen, "Mass Belief Systems Revisited: Political Change and Attitude Structure," *Journal of Politics*, 36 (August, 1974), 540–591.

[42] See Tables 2 through 4 above and also Nie and Anderson, pp. 586–87.

waiting for a dissident faction within the established parties or for a new political phenomenon like Governor Wallace to mobilize it. This too is a fruitful area for future research on the impact of issues on elections.

One final point: In studying issue voting it still remains important to keep in mind the features of this phenomenon as originally described in *The American Voter*. Voters must know about the issues, the party positions, and the current government policies. Voters must also care about how the issues are resolved. In measuring these features experience suggests that the best strategy to follow is the simplest one. Stick as closely as possible to what the respondents say, and develop more sophisticated scales of issue positions only after simple measures have proved unsatisfactory.

I. Pomper's Appendix Tables 1–3 Referred to in Text

Table 1. Party Identification and Policy Position, 1956, 1960, 1964 and 1968
(In Percentages Supporting "Liberal" Position)

	Aid to Education				Medical Care				Job Guarantee			
Party Identification	**1956**	**1960**	**1964**	**1968**	**1956**	**1960**	**1964**	**1968**	**1956**	**1960**	**1964**	**1968**
Strong Democrat	80.0	66.8	51.0	53.6	74.2	74.5	78.2	81.3	75.6	71.2	52.6	53.1
Weak Democrat	78.1	59.0	44.1	38.3	67.3	60.2	65.2	72.1	64.0	62.4	38.4	39.7
Independent	71.0	53.2	39.3	32.9	55.8	56.7	57.2	55.3	55.0	56.6	31.0	27.0
Weak Republican	68.7	39.1	21.5	22.5	51.4	47.5	43.5	39.3	59.5	43.9	25.9	24.9
Strong Republican	67.7	44.5	15.5	12.0	45.9	54.2	23.6	42.7	51.5	52.7	16.1	25.4
Gamma	.15	.20	.34	.36	.24	.18	.45	.41	.19	.16	.31	.25

	Fair Employment				School Integration				Foreign Aid			
Party Identification	**1956**	**1960**	**1964**	**1968**	**1956**	**1960**	**1964**	**1968**	**1956**	**1960**	**1964**	**1968**
Strong Democrat	73.3	63.0	56.3	61.9	38.7	39.8	53.7	58.9	49.5	51.4	64.7	51.3
Weak Democrat	71.3	63.1	42.9	43.5	44.4	37.5	43.2	44.6	55.4	48.8	59.2	45.8
Independent	66.6	65.4	50.3	37.7	48.8	47.1	49.0	37.3	49.9	53.2	57.5	42.7
Weak Republican	70.8	62.7	36.3	37.8	49.3	43.0	50.5	37.4	48.2	54.0	56.6	47.0
Strong Republican	66.8	65.9	20.6	31.3	38.3	41.5	34.8	31.5	51.4	61.5	49.7	41.8
Gamma	.04	–.02	.22	.24	.04	–.01	.08	.43	.01	–.03	.08	.04

Table 2. Perceptions of Parties on Policy Issues, by Party Identification

	Existence of Party Differences *(Percentages of Those with Opinions Perceiving Party Differences)*											
	Aid to Education				Medical Care				Job Guarantee			
Group	**1956**	**1960**	**1964**	**1968**	**1956**	**1960**	**1964**	**1968**	**1956**	**1960**	**1964**	**1968**
Strong Democrat	58.1	62.8	73.8	75.7	63.2	68.6	86.4	85.8	73.0	74.5	80.3	78.1
Weak Democrat	44.2	53.2	60.9	53.0	48.9	59.0	77.2	71.2	54.2	66.3	69.3	65.4
Independent	43.4	52.3	53.5	48.0	41.2	45.3	77.4	62.3	36.4	57.6	55.2	54.2
Weak Republican	48.0	41.8	58.9	53.3	50.0	63.6	75.3	65.8	50.3	50.5	61.3	64.1
Strong Republican	59.0	63.0	78.4	73.3	64.9	66.7	87.7	75.9	63.7	42.5	76.7	79.1
Total Sample[a]	52	55	66	59	54	58	82	71	55	64	70	63

	Fair Employment				School Integration				Foreign Aid			
Group	**1956**	**1960**	**1964**	**1968**	**1956**	**1960**	**1964**	**1968**	**1956**	**1960**	**1964**	**1968**
Strong Democrat	50.0	52.5	77.5	78.5	55.6	50.6	70.0	75.9	46.5	44.7	61.5	64.7
Weak Democrat	46.9	38.7	70.1	64.9	55.6	34.9	63.7	62.5	40.4	37.5	52.7	43.8
Independent	38.5	38.7	58.8	55.0	42.3	30.8	59.0	51.1	33.6	32.7	50.8	39.3
Weak Republican	46.0	44.3	53.2	47.7	50.0	36.4	52.1	53.1	50.0	32.0	50.3	42.8
Strong Republican	52.3	50.0	75.0	60.5	57.5	39.2	66.2	60.5	63.9	47.4	61.8	48.0
Total Sample[a]	47	44	69	62	51	36	64	60	49	39	56	46

[a]Note that these are average percentages not Ns.

Table 3. Consensus on Positions of Parties on Policy Issues, by Party Identification

	Consensus on Party Positions *(Percentages of Those Perceiving Difference Selecting Democrats as Liberal)*											
	Aid to Education				Medical Care				Job Guarantee			
Group	1956	1960	1964	1968	1956	1960	1964	1968	1956	1960	1964	1968
Strong Democrat	90.5	95.2	96.5	94.1	93.9	95.8	98.2	98.8	85.5	98.8	98.8	97.0
Weak Democrat	86.5	90.5	93.6	82.6	84.6	90.4	95.8	88.7	71.8	93.3	93.9	88.5
Independent	66.3	74.6	83.8	64.3	76.4	87.7	93.5	82.7	61.9	82.3	89.3	71.4
Weak Republican	37.2	60.9	55.8	58.7	64.3	52.2	77.6	85.0	60.0	67.3	72.5	69.4
Strong Republican	31.6	47.9	55.0	41.2	57.8	51.3	79.8	63.6	47.0	32.3	63.8	53.9
Total Sample[a]	67	76	81	73	77	81	92	87	69	80	88	80

	Fair Employment				School Integration				Foreign Aid			
Group	1956	1960	1964	1968	1956	1960	1964	1968	1956	1960	1964	1968
Strong Democrat	64.6	83.5	96.9	97.3	39.1	34.7	96.2	95.3	57.1	80.9	98.1	93.0
Weak Democrat	63.6	65.5	93.9	88.3	51.5	42.5	90.3	87.1	53.0	60.2	94.2	89.2
Independent	49.4	53.4	89.5	75.6	45.8	46.4	91.8	83.4	54.5	46.8	88.1	83.1
Weak Republican	23.6	18.3	81.0	70.2	45.1	49.3	88.9	75.4	46.2	18.0	67.8	81.5
Strong Republican	6.0	17.2	67.6	56.4	59.4	48.5	71.3	69.5	29.3	21.4	70.8	61.6
Total Sample[a]	48	52	89	83	48	44	89	86	43	49	88	85

[a] Note that these are average percentages not Ns.

II. RePass's Table

Table 6. Direction of Vote by Strength of Issue Partisanship Controlling for Party Identification

Strength of Issue Partisanship	Party Identification									
	Strong Dem.		Weak Dem.		Ind.		Weak Rep.		Strong Rep.	
High Democrat	2%	(55)	4%	(22)	9%	(11)	0%	(3)	–	
Medium Democrat	1%	(77)	2%	(41)	4%	(28)	20%	(10)	40%	(5)
Low Democrat	0%	(83)	11%	(63)	8%	(40)	18%	(28)	33%	(6)
No Difference	10%	(37)	21%	(57)	22%	(45)	56%	(39)	94%	(16)
Low Republican	20%	(10)	45%	(20)	58%	(33)	77%	(31)	83%	(17)
Medium Republican	33%	(3)	62%	(13)	81%	(16)	93%	(14)	96%	(27)
High Republican	100%	(5)	89%	(9)	93%	(27)	90%	(20)	99%	(71)

The entry in each cell is the percent of the voters in the cell who voted Republican.

III.

Regression Analysis Explaining Two-Party Vote (From Miller et al. Table 6)[b]

	Rating Thermometer										
Total Population	Nixon	McGovern	Lib-Con	War Issues	Social Issues	Economic Issues	Cultural Issues	Party ID	McGovern Campaign Performance	R	R^2
Beta	.27	–.23	.17	.11	.11	.11	.02	.10	.10	.81	.66
% Variance Added	41%	14%	5%	1%	1%	1%	a	1%	1%		
N=2705											
College Educated											
Beta	.23	–.21	.16	.10	.13	.05	.01	.15	.06	.82	.68
% Variance Added	12%	6%	45%	1%	1%	a	a	2%	1%		
N=789											
Age 30–59 Beta	.31	–.25	.19	.09	.09	.05	.01	.07	.05	.79	.63
% Variance Added	13%	7%	41%	1%	1%	a	a	a	a		
N=1326											

[a] Less than 1%.

[b] For all other breakdowns, no variables other than thermometer ratings explained more than 7% of the variance.

Presidential Popularity and Negative Voting: An Alternative Explanation of the Midterm Congressional Decline of the President's Party*

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There exist two distinct bodies of knowledge on the subject of midterm voting. The first, developed by political scientists, emphasizes the stability and continuity of midterm voting from one election to another; while the second, nurtured by politicians, understandably focuses on short-run forces which stir national electoral shifts. The differences between the two reflect more than the normal problem of the scholars informing the politicians; they represent divergent professional goals. Since the research reported here draws upon and has implications for both perspectives, it is appropriate at the outset to review and compare political scientists' knowledge with politicians' wisdom.

Most of what we know about the voting habits of the American electorate has come from intensive research on presidential elections. The effects of party identification, candidate appeal, and issue orientation have been thoroughly elaborated for each of the last five presidential elections.[1] During these elections voting for congressional seats has been viewed as reflecting the presidential vote largely through the effect of presidential coattails.[2] The study of midterm congressional elections by contrast has been scanty, probably because the findings which have been reported comport so well with existing knowledge and theory about voting behavior.

The "Surge and Decline" Thesis

Compared to presidential elections, midterm congressional contests are portrayed by the literature as rather unexciting and routine events both for the voter and for the political system. Without highly visible presidential candidates barnstorming the country and advertising national political issues, midterm congressional campaigns generate lower media coverage, lower voter attention, and consequently lower election-day turnout—usually by about 15 percentage points.[3] The short-run forces of personalities and stylized issues which arouse voter interest during presidential elections are also responsible for much of the vote volatility which assures the Republican nominee a chance to overcome his initial partisan deficit. The absence of these forces during midterm elections means that voting will even more greatly rest on the voter's party identification. According to survey findings of the mid and late 1950s, defections from party represent only about 10 per cent of the votes cast, and independents make up a smaller proportion of the midterm electorate.[4] Thus, while each presidential campaign will be marked by its unique configuration of competing personalities and issues, and often high levels of defection at the polls, a midterm congressional election is characterized by lower interest, reduced turnout, and party voting.

This scenario of midterm voting portends several political consequences. First, it means

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*I would like to thank Terence Ball, W. Phillips Shively, Aaron Wildavsky, and Gerald Wright for their helpful comments on an earlier draft of this article.

[1] There is little need to repeat here the long and well known list of election-year studies. Several are particularly important, however, for providing the theoretical grounding for many of our ideas on midterm voting. Principally important, of course, is Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: Wiley, 1960). Also valuable is the collection of essays contained in their subsequent work, *Elections and the Political Order* (New York: Wiley, 1966). For an excellent summary of this body of conventional wisdom see Fred I. Greenstein, *The American Party System and the American People,* 2nd edition (Englewood Cliffs: Prentice-Hall, 1970), chapter 3.

[2] This literature, on the other hand, is somewhat less prestigious and conclusive in its findings. Important among the research examining coattails with individual level data is Warren E. Miller, "Presidential Coattails: A Study in Political Myth and Methodology," *Public Opinion Quarterly,* 19 (1955–56), 353–368. Also, William N. McPhee and William A. Glaser, *Public Opinion and Congressional Elections* (New York: Free Press, 1962).

[3] Angus Campbell, "Surge and Decline: A Study of Electoral Change," in *Elections and the Political Order,* pp. 41–43. Also, Robert B. Arseneau and Raymond E. Wolfinger, "Voting Behavior in Congressional Elections" (paper delivered at the APSA convention, 1973, Figure 1).

[4] Campbell et al., *Elections and the Political Order,* Table 11–1, p. 197.

that as long as Democrats remain the majority party, they should win most of the congressional seats during midterm contests. Indeed, only once since the New Deal realignments have the Republicans managed to win control of Congress during a midterm election. More importantly, low voter interest and party voting mean that major gains in congressional seats produced by the coattail of the victorious presidential candidate will be short-lived. Every four years since 1938 marginal voters who were stimulated by the winning candidate fail to show up at the polls, and defectors, who apparently went disproportionately to the winner, return home. As a result, the President's party loses congressional seats; the magnitude of these losses directly reflects the magnitude of the gains produced by the winner's coattails.[5] Thus, midterm congressional elections serve to restore party competition and equity by repairing the damage done to the losing party by the presidential election two years earlier.

Two Problems With Surge and Decline. At first glance "surge and decline" presents us with a tight structure underpinned by the foundations of modern voting theory. The voter's marginal interest in politics, the dominant role of party identification in electoral decisions, and the sources of stimulation for both voting participation and defection serve as basic ingredients of the surge and decline thesis. Employing these features, surge and decline pulls off the neat trick of explaining the apparent anomaly that every two years there is a substantial, nationwide shift in the electorate's political preferences. Low stimulation and disinterest—not highly motivated policy voting—account for the systematic alternation of success and failure at the polls.

Despite this virtue, two distinct issues lead one to question the adequacy of surge and decline in explaining midterm voting. The first criticizes its failure to predict and explain—or even to address—important electoral outcomes. This criticism argues that surge and decline is incomplete; its sins are those of omission. The second issue is potentially even more destructive; rather than being merely incomplete, the surge and decline conclusions based on the 1958 election are inaccurate when applied to other midterm elections.

The few midterm defections which do occur are conventionally viewed as reflecting peculiar features of the local campaign or individual voter qualities, rather than as representing a widespread effort to reward or punish a given political party. Two frequently cited sources of defection are "friends-and-neighbors" voting and name familiarity of the candidate.[6] For the purpose of identifying or explaining a national electoral movement during the midterm election away from one party and toward another, neither variable is very helpful. Although these variables may be important for explaining variance among individual voters and thereby informing us that in many instances voter defection is politically idiosyncratic, they are silent about the systematic influences which may be politically decisive in determining party control of the national legislature. The preoccupation of the existing research with the politically more sterile aspects of defections can be seen from the following passages both of which draw their conclusions from the 1958 SRC survey. Donald Stokes and Warren Miller acknowledge as an afterthought: "... there is *some* variation, and these moderate swings must be attributed to forces that have their focus at the national level. . . . Our main point is rather that . . . the proportion of deviating votes that can be attributed to national politics is likely to be a small part of the total votes cast by persons deviating from party in a mid-term year."[7] In his important article, "Surge and Decline: A Study of Electoral Change," Angus Campbell reveals his focus with the following understatement: "... the partisan movement in 1958 cannot be entirely attributed to a normal decline toward standing party loyalties after the displacement of the vote in a surge year."[8]

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[5] Ruth C. Silva, "A Look Into a Crystal Election Ball," *New York Times Magazine*, 10 October 1954; p. 13. V. O. Key, Jr., *Politics, Parties and Pressure Groups*, 5th edition (New York: Thomas Y. Crowell, 1964), pp. 567–574; and Barbara Hinckley, "Interpreting House Midterm Elections: Toward a Measurement of the In-Party's 'Expected' Loss of Seats," *American Political Science Review*, 61 (September, 1967), 694–700.

[6] Donald E. Stokes and Warren E. Miller, "Party Government and the Salience of Congress," in *Elections and the Political Order*, pp. 204–209.

[7] Ibid., pp. 202–203. Elsewhere Stokes estimates that the proportion of variance in congressional district voting which is explained by national forces is 32 per cent as compared to 49 per cent for district forces. See "A Variance Components Model of Political Effects," in *Mathematical Applications in Political Science*, ed. John M. Claunch (Dallas: Southern Methodist University Press, 1965), pp. 61–85. Richard S. Katz persuasively argues that Stokes's estimate for the influence of national-level forces is too low; Katz in his correlation analysis revises the estimate upward to 54.6 per cent. See Katz "The Attribution of Variance in Electoral Returns: An Alternative Measurement Technique," *American Political Science Review*, 67 (September, 1973), 817–828. This estimate has been statistically corrected to 45 per cent. See communication by Christopher Achen, *American Political Science Review*, 68 (March, 1974), 1272–1273. It should be remembered that these estimates of the sources of the congressional vote include presidential election years.

The genuinely curious thing about these conclusions is that they are based on the election which produced the largest midterm landslide since the 1920s. More than 56 per cent of the total vote went to the Democratic party and, as a result, the Democrats picked up 48 congressional seats from Republicans who had not benefited greatly from President Eisenhower's victory two years earlier. If ever there were an ideal election for discerning what V. O. Key described as a "national party battle," the 1958 election was it.[9] Yet most conclusions founded on the 1958 election fail to recognize this.

The second problem is whether the thesis is descriptively accurate. Examining surge and decline for the last three midterm elections, Arseneau and Wolfinger have discovered some important departures from the 1958 election-based conclusions.[10] The critical evidence for the partisan implications of the midterm "decline" should be a disproportionately large withdrawal from participation by independents and a significant reduction in partisan defections. In Table 1 we can see that both the independent dropout rate and the level of party-line voting were higher in 1958 than for the other midterm elections. In fact, comparing the partisan composition of the midterm electorates with the presidential electorates yields little evidence to verify the surge and decline phenomenon. In two of the four midterm elections, independents voted in greater proportions to the total electorate than for the preceding presidential elections. Arseneau and Wolfinger conclude that the differences in the partisan complexion between presidential and midterm electorates are relatively unimportant.[11]

Surge and decline accents the dominant influence of party voting at the midterm. Even if the expected differences in the composition of the presidential and congressional electorates had emerged, the greatly reduced levels of party voting which has occurred since the mid-1960s would render surge and decline less adequate for explaining congressional voting in general.

The primary disagreement of this new evidence with the established surge and decline explanation is the failure of independents and partisan defections to decline from their proportions of the electorate two years earlier. Given this finding, much of what surge and decline attributes to midterm elections begins to unravel. Contrary to plausible expectations, the reduced stimulation of the midterm campaign—verified by the large, overall reductions in turnout—fails to produce a disproportionate reduction in participation among the generally more marginal independent voters. The absence of presidential candidates in the campaign and on the ballot for some reason fails to reduce the level of partisan defection. In turn the systematic decline of the victorious party in the presidential election two years earlier can no longer rest solely on differential turnout and party voting. With surge and decline in disrepair as presently formulated, we again find ourselves trying to account for the highly regular midterm shift in party fortunes.

A Different View of Midterm Elections

Presidents dreading the prospects of a less friendly Congress, and congressmen sensing that their political careers may be subject to the vagaries of short-run national forces, adopt a

Table 1. Close Resemblance between Midterm Electorates and Presidential Electorates[a]

	1956	1958	1960	1962	1964	1966	1968	1970
Party-line votes	82	84	80	83	79	76	74	76
Defection	9	11	12	12	15	16	19	16
Independents[b]	9	5	8	6	5	8	7	8
	100%	100%	100%	101%	99%	100%	100%	100%

[a] From Table 5 of Arseneau and Wolfinger.

[b] Independents exclude leaners.

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[8] Campbell, *Surge and Decline* . . . , pp. 55–57; a similar theme is also given in his "Voters and Elections: Past and Present" in *Journal of Politics,* 26 (November, 1964), 745–757. For both Campbell and Hinckley (whose measurement is the loss and gain of seats) the systematic partisan shift becomes the residual difference between the predicted normal vote (a seat loss) and the actual results. Defining the error term as the partisan shift prevents us from testing the accuracy of surge and decline and ignores the causes of a national partisan movement.

[9] Key, pp. 569-571.

[10] Arseneau and Wolfinger, *passim.*

[11] Ibid., p. 17.

perspective for understanding elections far different from that described above. For them understanding the election requires that they attempt to gauge national shifts in the electorate's opinions and evaluations. Believing that the public is much more politically aware and issue oriented than scholarly research has proved it to be, candidates for office attempt to discover shifts in national sentiment and react accordingly.

Of course the truism that "one sees what one wants to see" is probably especially true for politicians. Many politicians and pundits have shown themselves highly imaginative in predicting and explaining victory and defeat. One columnist for a national news magazine decorated his attack on urban political machines with the proclamation that the recently past Republican congressional victory of 1950 revealed the public's disgust with big city bossism.[12] Another, writing in *Newsweek*, read into the Democratic landslide of 1958 the public's displeasure with the failure of President Eisenhower to balance the budget and more diligently combat communism abroad, as if congressional Democrats favored these policies.[13] But after perusing popular analyses of midterm elections in the press and reviewing biographical accounts of the participants over the last 25 years, one is impressed by the general continuity of the assumptions and beliefs that careful observers and participants have employed to predict and explain elections. Long before behavioral political science had identified and specified the effects of presidential coattails, politicians already had developed through experience a rather accurate conventional wisdom about which party's congressional candidates benefited during the presidential victory, which suffered two years later, and why. Another equally pervasive theme has, however, received no systematic inspection—much less verification; this is that the President's public standing is a major component of those national shifts of public opinion which shape voting during midterm elections. Before testing this belief empirically, perhaps it will prove useful to describe briefly its effects on politicians' behavior and campaign strategies.

The unprecedented "in" party midterm victory of 1934 was widely hailed as an affirmation of the public's enchantment with Franklin Roosevelt; according to his biographers, the President agreed with this interpretation privately as well as publicly.[14] And twenty-eight years later we learn from Arthur Schlesinger Jr. that President Kennedy was concerned with the potential damage the Cuban missile crisis might have on his public prestige and as a result on the fall's congressional elections.[15]

Occasionally the President's popularity drops to a point where the opposition can comfortably make the President's record a campaign issue. President Truman, whose sharp fluctuations in popularity did indeed make him appear to be riding a tiger, had to face the unpleasantness of political assault both in 1946 and 1950. During the 1946 congressional campaigns the potential vulnerability of an unpopular president to partisan attack was fully realized. President Truman became the primary issue as Republican candidates universally attempted to associate him with their Democratic opponents. Just as zealously Democrats resisted. Democratic leaders asked Truman not to campaign on their behalf, and, desperately, many Democrats played recordings of the late President Roosevelt hoping to conjure up a more favorable image.[16] Columnist Raymond Moley provides a typical description of the campaign:

> Organization Democrats far and near acted as if they were ashamed of their own President. His name was barely mentioned in speeches and campaign literature. With an eye to the Gallup polls, which indicated a drop in Mr. Truman's popularity from a honeymoon percentage of 87 percent to an October brown of 32 percent, they decided that he was to be written off as a loss. Republicans made the most of him as an issue. He was surrounded by Communists and bunglers, they said. He was weak and wobbly. Congress must seize the reins from his faltering hands.[17]

By comparison, the congressional elections of 1950 were less dramatic. President Truman occasionally even tried campaigning for favored Democratic candidates. But again his popularity was low, Democrats preferred to run alone, and the President became a target of Republican attack. A pre-election survey of Washington newsmen that year found agreement in the belief that "administration incompetence" would be a decisive issue in the election.[18]

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[12] "The Election," *Time Magazine*, 13 November 1950, p. 19.

[13] Raymond Moley, "What Happened," *Newsweek*, 10 November 1958, p. 61.

[14] James MacGregor Burns, *Roosevelt: The Lion and the Fox* (New York: Harcourt, Brace and World, 1956), pp. 198–203; and William E. Leuchtenburg, *Franklin D. Roosevelt and the New Deal* (New York: Harper and Row, 1963), pp. 822–823.

[15] Arthur M. Schlesinger, Jr., *A Thousand Days* (Boston: Houghton Mifflin, 1965), pp. 822–823. Robert F. Kennedy also mentions this consideration in his account of the crisis, *Thirteen Days* (New York: Norton, 1969). Also see Graham Allison, "Conceptual Models of the Cuban Missile Crisis," *The American Political Science Review*, 63 (September, 1969), 712–714.

[16] James D. Barber, *Presidential Character* (Englewood Cliffs: Prentice-Hall, 1973), pp. 278–280.

[17] Raymond Moley, "The Presidency," *Newsweek*, 11 November 1946, p. 116.

The midterm elections of 1954 and 1962 presented the voters with popular presidents and thus apparently an opposite set of decisions by the political actors. At the beginning of the 1954 midterm campaign in September, 65 per cent of the public approved President Eisenhower's job performance, and in early September of 1962, President Kennedy's popularity was at a comparable level of 67 per cent. Instead of abandoning their President as the Democrats had done during the 1946 and 1950 campaigns, the congressional candidates from the President's party were anxious to find a coattail, and both Presidents were constantly importuned to campaign on behalf of the numerous candidacies. Unlike Truman's requested campaign silence, both Eisenhower and Kennedy, sensing an opportunity to increase their party's congressional majority, were extremely active in behalf of favored party candidates.[19] Given a popular incumbent, congressional candidates from the opposition party refrained from vituperative attacks.

Even during periods of divided government, politicians view the public as continuing to hold the President's party responsible for governmental performance. During both the 1958 and 1970 congressional elections, with a Republican in the White House, economic prosperity became a major issue as Democratic candidates disputed the administration's economic policies. In both instances the President campaigned actively against the Democratically controlled Congress, but in each he appeared unsuccessful as his own party sustained losses in its congressional strength. In late 1958, when a worsening economic recession (which improved in early 1959) coincided with the congressional election, the Republican defeat was particularly severe.[20] After the election, Vice President Richard Nixon, who would face a similar situation twelve years later as president, interpreted the Republican defeat:

> The power of the "pocketbook" issue was shown more clearly perhaps in 1958 than in any off-year election in history. On the international front, the Administration had had one of its best years . . . Yet, the economic dip in October was obviously uppermost in the people's minds when they went to the polls. They completely rejected the President's appeal for the election of Republicans to the House and Senate.[21]

What this review of modern midterm congressional elections suggests is that political actors do believe that the President's popularity is causally related to their party's electoral success.[22] Consequently, the several key sets of political participants—the President, candidates from his own party, and candidates from the opposition party—all adapt their own campaign roles and strategies to the President's level of public approbation.[23] The role of the President as a campaigner, the campaign posture of his party's candidates toward him, and his status as a political issue to opponents are primarily determined by his public prestige.

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[18] "Correspondents' Election Preview," *Newsweek*, 6 November 1950, p. 25. Also, Key, pp. 563–568.

[19] Whatever misfortune or shortcomings Eisenhower may have displayed in his dealings with Congress or his own cabinet, it is difficult to gainsay the General's adroitness as a campaigner, and the 1954 congressional elections are no exception. The following passage reveals that he recognized that his public prestige was important to others, and he intended to exchange it for his own profit. Joseph Meek, the Republican senatorial candidate running against the incumbent Illinois Democrat Paul Douglas, had publicly opposed certain features of Eisenhower's foreign aid program during the campaign. Eisenhower describes the affair in his memoirs:

> In April, I had told him I would not back him until he announced that he would support my program. Consequently, I remained silent on his candidacy until he gave public assurance that "you can count on my loyalties and my support as the junior senator from Illinois." On August 12 I was photographed with seventeen candidates, not yet members of the Congress, who because they promised to support my program had won the approval of the Citizens for Eisenhower, who were again readying themselves for action in the campaign. I wanted and needed a Republican Congress; but I could see no sense in working for office-seekers who were ready to object to every proposal I made. Dwight D. Eisenhower, *Mandate for Change* (Garden City, N.Y.: Doubleday, 1963), p. 433.

To the degree that politicians believe the first premise that the President's popularity is important to their own success, the incumbent President possesses a "bargaining advantage" which, as we see here, can be used to purchase future congressional loyalty.

[20] James L. Sundquist provides an interesting analysis and persuasive discussion on this point in *Politics and Policy: The Eisenhower, Kennedy and Johnson Years* (Washington, D.C.: Brookings Institution, 1968), pp. 452–466. During the campaign Republican senatorial and congressional candidates publicly disavowed allegiance to President Eisenhower's economic programs. On this point see Key, pp. 569–570.

[21] Richard M. Nixon, *Six Crises* (New York: Doubleday, 1962), p. 310. Campbell et al. in *The American Voter* find that 40 per cent of the families felt a direct impact of the recession and that their responses to the economy correlated with evaluations of the Eisenhower administration and 1958 congressional vote intentions (pp. 386–391).

[22] The only systematic investigation of the effects of the strategies of congressional candidates seeking to dissociate themselves from their party's national leadership was performed by Robert A. Schoenberger for the 1964 presidential election. Republican candidates who severed any connection with the Goldwater candidacy received on the whole a larger share of the congressional vote than did those congressional candidates who maintained party loyalty. "Campaign Strategy and Party Loyalty: The Electoral Relevance of Candidate Decision-Making in the 1964 Congressional

The political science literature acknowledges pervasive national trends in voter defections but fails to identify their sources. The conventional wisdom of politicians and pundits, on the other hand, locates a national source, one which thoroughly complements the image of the typical voter developed by modern survey research. The issue positions of candidates may be ambiguous and confusing, but the electorate perceives clearly (or at least thinks it does) the policies and actions of the incumbent president. The party control of Congress may be unclear, but everyone knows the President's party and views him as its leader. Given the public's low levels of interest in and knowledge about politics, what better criterion exists for voting one's satisfaction or dissatisfaction with current government programs and performance than by voting for or against the President's party? To the extent that the President's policies reflect those of his party's congressional candidates—and they generally do—voting according to one's evaluation of the President becomes a viable and rational procedure for expressing one's own policy preferences and establishing the parameters of future government policy. Given this reasoning, the discovery of the relationship between presidential popularity and congressional midterm voting may inform us about more than simply short-term national shifts in congressional voting. If such a relationship does exist we may have identified an important—albeit imperfect—ingredient of representative government in the United States. There is good evidence that this view of democratic politics in America has been appreciated for over a century. James Bryce, describing American political institutions at the turn of the century, wrote:

> Members [of the House of Representatives] are elected for two years, and the election always takes place in the even years, 1908, 1910, and so forth. Thus the election of every second Congress coincides with that of President; and admirers of the Constitution find in this arrangement another of their favourite "checks," because while it gives the incoming President a Congress presumably, though by no means necessarily, of the same political complexion as his own, it enables the people within two years to express their *approval or disapproval of his conduct by sending up another House of Representatives which may support or oppose the policy he has followed*[24] (emphasis added).

The important question arises: Do voters take their evaluation of the President into account when voting in midterm congressional elections?

A Highly Suggestive Midterm Election Trend

The date (1913) of Bryce's observation suggests that political actors have subscribed to these beliefs for some time. Traditionally midterm election campaigns have revolved around attacks upon, and defense of, the President's policies, presumably with the view that if the President's party could be discredited, so would his party's congressional candidates. Only during the last thirty-five years, however, has the relationship between the congressional vote and the incumbent President's popularity been directly observable. Figure 1 displays a rough parallelism of trends in presidential popularity and the congressional vote—a finding which supports the politician's pragmatic view. During both Democratic and Republican administrations, increases and decreases in the President's popularity are matched by a similar change in his party's congressional success. As one would

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Elections," *American Political Science Review*, 63 (June, 1969), 515–520. For a more general analysis of politicians' beliefs about the importance of campaign strategies see John W. Kingdon, *Candidates for Office: Beliefs and Strategies* (New York: Random House, 1969).

[23] The author is currently investigating the degree to which career decisions of politicians are based on the President's public standing. Entry onto a number of rungs in the career ladder may be affected. The number of candidates seeking each party's congressional nomination, the percentage of congressmen from each party trying to become a senator or governor, and the percentage who decide to retire from the House are currently being examined. If presidential popularity is an important consideration in career movement we should find an inverse correlation between the Democratic and Republican percentages on the above measures. If a low presidential popularity is found to discourage viable candidates from the President's party from attempting to get on or move up the office ladder, while at the same time encouraging candidacies from the opposition, the overall congressional vote could, in part, reflect systematic party differences in the quality of the candidacies.

[24] James Bryce, *The American Commonwealth* (Norwood, Mass.: Macmillan and Company, 1913), p. 128. This "parliamentary" perspective of British observers continues today. See Nelson W. Polsby, "Review Article: The British Science of American Politics," *British Journal of Political Science*, 2 (Oct., 1972), 492.

expect, presidential popularity exhibits much higher volatility than congressional voting. President Truman's popularity plummeted to 34 per cent in 1946, but his party still managed to garner more than 44 per cent of the vote. At the top of the figure, in 1962 President Kennedy was enormously popular immediately after the missile crisis (74 per cent approving), yet the Democratic share of the congressional vote was only a little more than 52 per cent. Despite a 40 percentage point spread in presidential popularity between the elections of 1946 and 1962, the Democratic proportion of the total congressional vote varied by only eight percentage points.

This information accords well with individual-level findings that congressional voting is highly stable. From the politician's perspective—which is necessarily one of looking at the margins—an eight-point spread may spell the difference between defeat and victory. For example, the 1946 election resulted in the greatest Democratic loss in thirty years—20 per cent of the Democrats' House seats—while in 1962 they almost managed to avoid the apparently ineluctable midterm decline. Thus, both perspectives may easily be reinforced by the trend presented in Figure 1. *Levels of presidential popularity and the party distribution of the congressional vote covary in a predictable manner, but do so within the confines of a general continuity of the vote.*

Figure 1 contains additional information which cannot be easily seen in its present format. In Figure 2 the data are rearranged to make this additional information more easily discernible. Along the base line of Figure 2 the midterm election years are listed according to the Democratic percentage of the congressional vote.On the left end of the horizontal axis is the election of 1946, the Democratic debacle, which surrendered Congress to the Republicans; and on the right end is the almost as disastrous defeat of the Republican party of 1958, which resulted in overwhelming Democratic control of both houses of Congress. Figure 2 clearly shows that the best Democratic election years occurred during Republican administrations and vice versa. Only in 1962, when the Democratic President's popularity reached an extraordinarily high level, did the Democratic share of the congressional vote equal its smallest percentage of the vote achieved under Republican presidents. The data are consistent. *Both political parties fare worse in midterm congressional elections when an incumbent from their party occupies the White House than when they represent the opposition.* It should be remembered that this conclusion concerns the party division of the total vote—not losses and gains

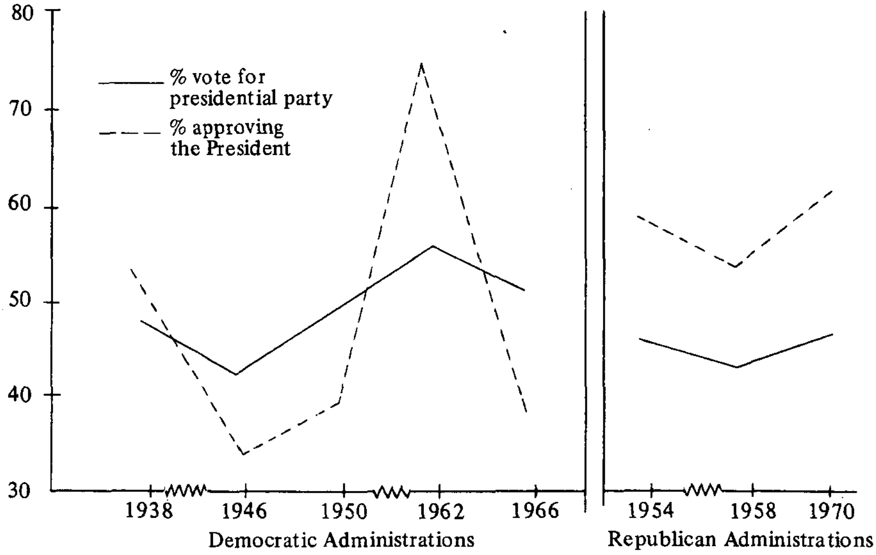

Figure 1. Variations in Presidential Popularity Matched against Variations in Midterm Congressional Vote.

Sources: *Statistical Abstract of the United States* for the years 1967 and 1970 (Washington: U.S. Bureau of the Census). For information not available in the primary data set the *Gallup Opinion Index* (#56, February, 1970) was used.

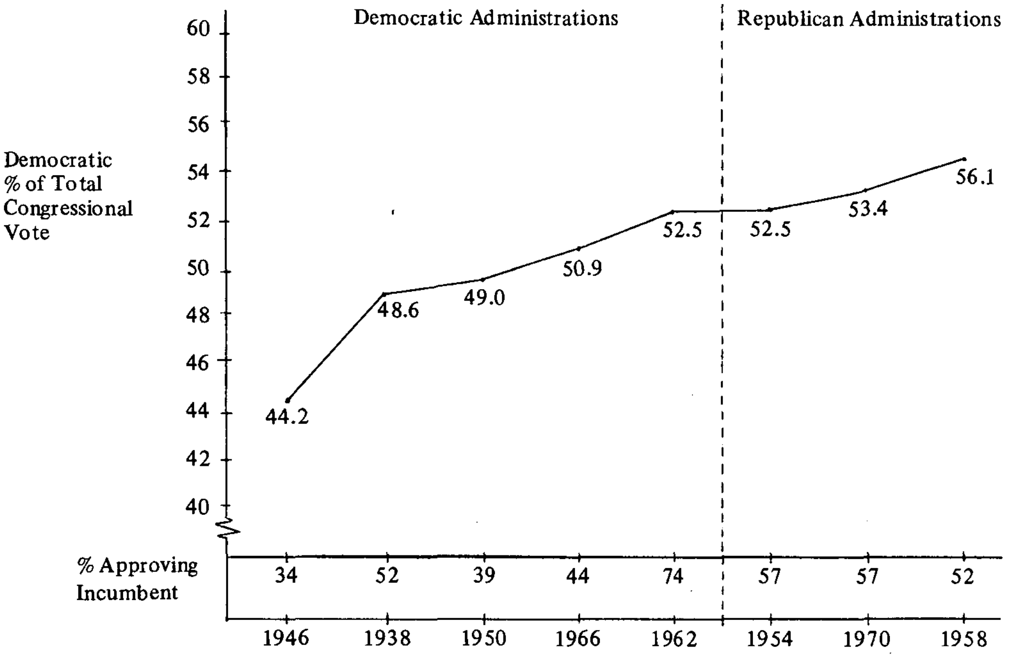

Figure 2. Midterm Congressional Elections
Ranked According to the Democratic Proportion of the Vote

in congressional seats—across a series of midterm elections. It does not compare the change in a given midterm vote with the presidential vote two years earlier. Surge and decline proffers an explanation of why a given candidate does better or worse at midterm *compared to the previous presidential vote.* The theory ignores changes in the partisan distribution of the vote *across a series of midterm elections.* Reduced participation among marginal voters and lower levels of defection tell us little about why a party's candidates receive a smaller vote in those midterm elections during which their party occupies the White House. And although the folk wisdom of politicians identifies the President's public standing as an important variable, it too fails to explain this particular bias.

Why would a party be more successful in opposition than in control of the presidency? Intuitively, one might think that just the opposite would be the case. A popular president can provide valuable assets to his party's congressional candidates, including visibility from a public endorsement and a timely visit to the congressman's district. Yet even during periods when relatively popular incumbents hold office—periods such as 1954, 1962, and 1970—the President's party has still failed to achieve the same level of electoral success attained when it was the "out" party. A frequently mentioned but never fully elaborated theme in political science literature which may provide a clue is that the electorate votes *against* policies and incumbents to a greater degree than it votes *for* new policies and candidates. *The American Voter* closes with an important discussion of party equilibrium in which negative voting is an integral ingredient:

> . . . the party division of the vote is most likely to be *changed* by a negative public reaction to the record of the party in power. . . . A majority party, once it is in office, will not continue to accrue electoral strength; it may preserve for a time its electoral majority, but the next marked *change* in the party vote will issue from a negative response of the electorate to some aspect of the party's conduct in office, a response that tends to return the minority party to power.[25]

Recent research in social psychology largely complements the view that negative opinions exercise disproportionate influence in political behavior. Studies utilizing a variety of experimental settings have consistently shown that the perceived negative aspects of a stimulus object are more determinative of the overall evaluation of the object than its positive aspects.[26] When this asymmetry of evaluations is applied to voting behavior, the proposition that negative stimuli are more instrumental to the vote choice than positive would appear to be reflected in two ways. First, failures of incumbents are more important than their achievements, and despite a long list of accomplishments, a conspicuous failure may threaten re-election.[27] Second, the greater strength of negative evaluations suggests that voters upset with an incumbent's performance will be more activated to vote against the individual than are satisfied voters likely to support him. Thus, even a popular president is not immune from negative voting; he still must work to overcome the disproportionately greater turnout and defection among those voters who are displeased for one reason or another with his performance.[28]

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[25] They continue, "The crux of our theory is that changes in the party balance are induced primarily by negative rather than positive attitudes toward the party controlling the executive branch of federal government" (*The American Voter,* p. 554).

Tailoring this thesis to the question of the influence of presidential popularity on congressional voting, a negative voting model hypothesizes that citizens displeased with a president's performance are more likely to vote against his party's congressional candidates than are satisfied voters likely to vote for them. The President's party label becomes a target for retribution.[29] The relationship depicted in Figure 2 suggests that the benefits of negative voting accruing to the party out of the White House will probably exceed any salutary electoral effects of having a popular president from one's own party. High presidential popularity reduces the deleterious consequences largely to the degree that it limits the number of voters who are dissatisfied (or at least attribute blame) and are thus likely to engage in negative voting.

The negative voting model is interesting in several respects. First, it parsimoniously incorporates both variables presented in Figures 1 and 2; presidential popularity operates within the context of the overriding qualitative variable, the President's party. Low popularity, according to this model, indicates that a large proportion of the electorate may be disposed toward negative voting. The Republicans won 56 per cent of the vote on such an occasion in 1946. The Democrats until 1974 had not been in such an auspicious position; the 1954, 1958, and 1970 elections featured relatively popular Republican incumbents. At the beginning of August it appeared that the 1974 congressional elections would present a critical extension and test of the model, but the resignation of President Nixon, the assumption of office by Gerald Ford, and his initially strong public approval made the effect of Ford's popularity on the election less certain. The evidence available leaves little doubt that former President Nixon's low public support contributed greatly to the Democratic landslide that year. In March of 1974, when Nixon's popularity in the wake of Watergate had fallen sharply to only 26 per cent approval, a cross-section of the public was asked which party it would like to see win the next congressional elections (using a test instrument of proven value in accurately predicting the actual nationwide congressional vote).[30] Among respondents giving an opinion,

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[26] David E. Kanouse and L. Reid Hanson, Jr., *Negativity in Evaluations* (Morristown, N.J.: General Learning Press, 1972). The authors summarize the experimental research: "It seems that negativity biases occur against a backdrop of perceived bliss—indeed perhaps because of it. Given that most people perceive the world as a predominantly positive place, there are a number of reasons why one might expect them to weigh positive information rather more lightly than negative. First, there is the well-known judgmental anchoring, or contrast, effect. In a world of ointment the fly seems bad indeed. Second, if most choices and behavior-relevant evaluations are made from a range of general positive alternatives, it is simpler and less effortful to sort the alternatives on the basis of their few negative aspects rather than the many positive ones," p. 10. Another review of a negativity bias is Nehemiah Jordan's, "The 'Asymmetry' of 'Liking' and 'Disliking': A Phenomenon Meriting Further Reflection and Research," *Public Opinion Quarterly,* 29 (Summer, 1965), 315–322.

[27] Perhaps no better recent illustration of this phenomenon exists than the case of Hubert Humphrey in 1968. Throughout the 1950s and mid-1960s Senator Humphrey, co-founder of the Americans for Democratic Action, was the chief originator and advocate of liberal social policies in the Senate, yet because of his association with President Johnson's Vietnam war policies, Humphrey suffered a severe drop in popularity within the liberal community during his 1968 bid for the presidency.

[28] Although negative evaluations may be more instrumental, positive evaluations are generally preferred and more pervasive. Thus, incumbents may be able to overcome negative voting largely as a result of their generally positive evaluation by the citizenry. See David O. Sears and Richard E. Whitney, *Political Persuasion* (Morristown, N.J.: General Learning Corporation, 1973), pp. 10–17.

[29] Howard S. Bloom and H. Douglas Price in a time-series study of the effects of short-run economic change on congressional voting find a negativity bias operating. "Political Parties are 'punished' by the voters for economic downturns but are not 'rewarded' accordingly for prosperity." *American Political Science Review,* 69 (December, 1975), 1240–1253. Other studies use the party of the administration as the benchmark for testing the effects of economy on short-run political change. See Gerald H. Kramer, "Short-term Fluctuations in U.S. Voting Behavior, 1896–1964," *American Political Science Review,* 65 (March, 1971), 131–143.

[30] The survey question asks which party the respondent would like to see "win his state in the next congressional election." Responses to this item correlate at .80 (Pearson *r*) with reported actual vote.

66 per cent preferred the Democrats, a proportion that would represent the largest Democratic landslide of the twentieth century! Although a fuller examination of the relationship between Nixon's decline in public esteem and preferences for the 1974 congressional elections will follow, it should be noted here that this finding is fully consistent with the pattern plotted in Figure 2.

Second, the negative voter model sustains another popular belief that presidential efforts to influence the midterm congressional vote generally do more harm than good. Despite this long-standing dictum and little historical evidence to the contrary, recent presidents have done more midterm campaigning than their predecessors.[31] When the President increases the public association between himself and a congressional candidate, he unwittingly facilitates the transference of negative affect as well as positive.[32] This is undoubtedly why the more perspicacious party leaders asked President Truman to stay at home during the 1946 campaigns. To the degree that negative evaluations are more determinative than positive ones, even a popular president may prove to be a net liability to his party's congressional candidates. Negative voting offers a more plausible alternative explanation to the usual surmise that presidents are not successful because voters dislike presidential meddling in local affairs.

Third, the negative voting model offers an important alteration of the surge and decline thesis. In Table 1 we saw that the composition of midterm electorates with respect to party defections and the proportion of independent voters is similar to that of presidential electorates. Although each midterm election has remained consistent with the pattern of lower overall turnout, it appears that the reduction in turnout is not so uneven as to affect greatly the partisan complexion of the electorate. Since the negative voting model posits the evaluation of the President as a pivotal motivational cue, we may find that there will be in fact differential withdrawal from the electorate but that it will not necessarily show up in the turnout levels among the categories of voters identified by the surge and decline thesis—party voters, defectors, and independents. Each group's relative level of participation may remain more or less stable, yet within each the net partisan preferences of those who do vote may systematically become less favorable to the President's party from the presidential to the midterm election. For example, independents who disapprove the President may be more activated to vote. Whatever the overall changes in their proportion of the electorate, internally they may be stacked during the midterm with greater proportions of disapproving voters. Thus, unless the President's popularity remains very high, his party's overall percentage of the congressional vote declines. Differential turnout remains central to the outcome, but different categories of voters, disapprovers and approvers, are identified as respectively staying in and dropping out.

The negative voting model possesses the desirable quality of being both simple and capacious. It fits neatly with the conventional wisdom of politicians, who hold that the presidency is an important ingredient in the success of congressional candidates even during off-year elections. It explains routine nationwide trends of decline in the appeal of the president's party just two years after victory. To the extent that negative voting does not require a highly informed or politically involved citizenry, it accepts and incorporates much of what we know and believe about the average voter's decision-making processes. Indeed, the model appears tailored to facilitate policy voting among the poorly informed and inattentive public in that the criterion for the voting choice becomes the most visible participant in the American political system. And finally, the model presented here appears capable of explaining and predicting national shifts in party fortunes even at their extremes.

The question which remains then is, Is it valid? To avoid committing an error akin to the ecological fallacy by accepting too readily a model of individual voting behavior built exclusively of aggregate percentages and relationships, a thorough analysis of individual-level data is required.

The Schema for Testing the Negative Voting Model

The negative voting model contains two components. First, it posits an association between presidential popularity and midterm voting, and second, it argues that the effects of presidential approval and disapproval are unequal, with negative opinion being more determinative of voting behavior. These two aspects of negative voting point to two alternative models which must be shown to be less adequate explanations of the findings. First, there is the null hypothesis that no relationship exists between voting and presidential popularity. If the virtual absence of study on the subject indicates anything, it may be that the null hypothesis represents the favored view of political scientists (although not as we have seen, of politicians). Voting for aspiring officeholders to an institution clearly separate from the presidency and with little if any connection between the candidates and the incumbent President who is absent from the ballot, so the argument might go, should naturally divorce opinions about the President from congressional preferences.

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[31] Nixon's 1970 congressional election campaigning has been described by Evans and Novak as a "white heat." See Rowland Evans Jr. and Robert D. Novak, *Nixon in the White House: The Frustration of Power* (New York: Random House, 1971).

[32] At the presidential level, Richard W. Boyd in his analysis of the 1968 presidential election found such a transference from President Johnson to his party's nominee, Hubert Humphrey. See Boyd, "Popular Control of Public Policy: A Normal Vote Analysis of the 1968 Election," *American Political Science Review*, 66 (June, 1972), 440.

The only published research found which empirically investigates the relationship between midterm voting and presidential approval, is a now obscure article by John Harding in the *American Political Science Review* in 1944.[33] Conducted under the constrictions of a wartime setting, Harding's investigation was hampered by a small national sample of 230 respondents. Largely as a consequence, Harding failed to find a statistically significant relationship between evaluation of President Roosevelt's job performance and the respondents' reported vote in the 1942 congressional elections. Failing to disconfirm the null hypothesis, Harding moved on to other, more promising, explanatory variables. For three decades this conclusion has gone unquestioned.

The second alternative set of hypotheses may be called the "consistency" model. Like negative voting it posits a direct relationship between presidential popularity and congressional voting choices. Voters whose evaluations of the President are congruent with their party identification (for example, a Republican approving a Republican president) should show up at the polls in higher proportions and vote overwhelmingly according to the party line. Because party identification is a primary determinant of presidential approval, we should expect to find that most voters do hold congruent and reinforcing attitudes and as a result vote for their own party's candidate. By contrast, the smaller group of voters whose evaluations of the President are in conflict with their party identification, should show somewhat lower levels of participation and greater levels of defection than the consistent group. The size of the defection to or from the presidential party's candidate would reflect the relative independent strength of party identification and presidential popularity. We may predict that party identification should more often be the more powerful variable. What distinguishes the consistency model from negative voting is that, *ceteris paribus,* defection to the President's party by approvers should be as great as defection away from it by his detractors. It is this feature—the implicit assumption that approval and disapproval of the President have an equal motivational impact on the vote—that separates the consistency voting predictions from those of negative voting. The predictions for both models, and for the null hypothesis are delineated in Table 2.

The crucial predictions for the null hypothesis concern turnout and defection, predictions I and II respectively. Approvers and disapprovers should resemble each other both in their levels of voting participation and their degree of party voting; the predictions for III and IV are derivative of the first two. The turnout prediction of the consistency model that "a>b" (i.e., approvers from the President's party are more participant than disapprovers) borrows from cross-pressure research which has found withdrawal from conflictual settings a common method of tension reduction. It is this prediction that most differentiates the consistency and negative voting models. Negative voting predicts the opposite ranking, with disapprovers within the president's party more likely to vote than his party's approvers (i.e., "a<b"). Moreover, the negative voting model predicts that independents who disapprove of the President vote at a higher rate than independents who approve, that is, "e<f." The same theme governs predictions II and IV (partisan defection and independent attraction-repulsion) where the basic difference between the consistency model and negative voting is the hypothesis that negative evaluations will be more determinative of the vote choice. Prediction III also incorporates basically the same idea: if negative evaluations of the president are more important, they should reinforce party loyalties to a greater degree than positive evaluations. With these predictions we have a succinct and convenient method of exploring the data and providing a thorough assessment of negative voting against simpler and more conventional expectations.

The primary data set consists of six national surveys taken immediately after each midterm election from 1946 through 1966 by the American Institute of Public Opinion.[34] Fortunately, each postelection survey during this period solicited the vital information necessary for this inquiry: evaluation of the president's

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[33] John Harding, "The 1942 Congressional Elections," *American Political Science Review,* 38 (February, 1944), 41–58.

[34] Each poll was taken within one month of the election. Unfortunately, in 1970 A.I.P.O. failed to ask the presidential popularity item in its postelection survey.

Table 2. Predictions of Competing Models Concerning the Effects of Presidential Popularity on Midterm Congressional Voting

		Party Identification		
		Same as President	Other	Independent
Presidential Popularity	Approve	a	c	e
	Disapprove	b	d	f

Criteria of Comparison	Predictions of Alternative Models: Null	Consistency	Negative Voting
Voting Turnout			
I. Turnout effects: Percentage voting in election.	$a=b$ $c=d$ $e=f$	$a>b$ $c<d$ $e=f$	$a<b$ $c<d$ $e<f$
Voting Preferences			
II. Defection effects: Percentage who *defect* from Party identification.	$a=b$ $c=d$ $b=c$	$a<b$ $c>d$ $b=c$	$a<b$ $c>d$ $b>c$
III. Reinforcement effects: Percentage with consistent attitudes who vote for own party.	$a=d$	$a=d$	$a<d$
IV. Attraction and repulsion among independents: Percentage of approving who vote for presidential party compared to percentage disapproving who vote against.	$e=f$	$e=f$	$e<f$

job performance, voting participation in the midterm election, voting preference, and party identification. Although A.I.P.O. has altered its sampling procedure somewhat over the two decades covered in this study, the data remain satisfactorily comparable.[35]

The predictions given in Table 2 reflect only the motivational aspects of behavior, under the assumption that the opportunities for their expression—such as defecting to the other party's candidate—are available. During the twenty year period included in this study, however, such an assumption would be clearly inappropriate. In many congressional districts, especially in the South, there was a total absence of meaningful two-party competition. By focusing on the non-southern states we achieve a better sample for exploring the motivational impact of presidential popularity on voting behavior.[36]

The Findings

Prediction I: Presidential Popularity and Voting Turnout. Three sets of predictions concerning the relationship between the midterm voting turnout and evaluation of the President have been described. The first hypothesizes simply that the two variables are unrelated for any partisan group; the second suggests that evaluations of the President reduce turnout whenever they conflict with the voter's own party identi-

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[35] Norval D. Glenn has thoroughly examined this issue and determined that any systematic sampling bias differences are relatively minor. Polls taken before 1950 used quota sampling and as a result slightly under-represented the lower income and poorly educated citizenry. "Problems of Comparability in Trend Studies with Opinion Poll Data," *Public Opinion Quarterly,* 34 (Spring, 1970), 82–91.

[36] It was necessary to follow A.I.P.O.'s classification of southern states. They are Virginia, North Carolina, South Carolina, Georgia, Florida, Kentucky, Tennessee, Alabama, Mississippi, Arkansas, Louisiana, Oklahoma, and Texas. All others are classified as non-Southern. In the table below we can see that the South contained the bulk of non-competitive congressional districts.

Percentage of Non-Contested Congressional Elections Located in the South[a]

Election Year	%	*N*
1946	86	85
1950	88	103
1954	91	90
1958	88	95
1962	95	59
1966	86	51

[a] Includes cases where opposition candidate receives less than 10% of vote.

Table 3. Prediction I: The Relationship Between Presidential Approval and Midterm Election Turnout (Percent Voting, Non-South Only)

Party Identification	Presidential Popularity	1946		1950		1954		1958		1962		1966	
		%	N	%	N	%	N	%	N	%	N	%	N
Same as President's	Approve	65	(434)	74	(220)	80	(280)	80	(274)	69	(1381)	64	(754)
	Disapprove	66	(332)	70	(131)	50	(16)	61	(39)	72	(18)	77	(160)
	Difference	−1		+4		+30		+19*		−3		−13*	
Other	Approve	67	(245)	66	(61)	56	(163)	74	(161)	74	(501)	66	(238)
	Disapprove	83	(727)	85	(277)	70	(180)	70	(197)	85	(280)	78	(446)
	Difference	−16*		−19*		−14*		+4		−11*		−12*	
Independent	Approve	54	(188)	54	(96)	56	(117)	58	(136)	56	(517)	55	(289)
	Disapprove	70	(381)	75	(138)	70	(44)	66	(65)	78	(64)	56	(229)
	Difference	−16*		−21*		−14		−8		−22*		−1	

*Percentage point differences statistically significant at .05 or better. Negative sign confirms prediction of Negative Voting Model.

fication; and the third, negative voting, predicts that voters who disapprove of the President, whatever their partisan affiliation, will turn out in greater proportions. With the information provided in Table 3 we can evaluate these predictions.

Scanning the percentage point differences, one finds that persons who disapprove the President's job performance do on the whole vote in higher proportions. Of the 18 comparisons of approvers with disapprovers given in this table, 14 show disapprovers voting in greater proportions as predicted by negative voting. Examining the deviations more closely, however, we note that three of the four instances where the approvers turn out in higher proportions are among identifiers of the President's party as the consistency model predicts. Indeed, *among voters who identify with the President's party the conclusion to be drawn about the relationship between turnout and presidential popularity is unclear.* The evidence is contradictory. One of the percentage point differences is large and negative, two are large and positive, and two are very small, perhaps in all reflecting the simultaneous operation of both the consistency and negative voting processes which pull in opposite directions. Among the southern electorate (not shown) the differences in turnout are uniformly in the direction predicted by negative voting.[37]

Those who identify with the party out of the White House yield relationships that are much clearer. Given the reinforcing character of the consistency and negative voting processes for these voters, it is not surprising to find disapprovers substantially more likely to vote. Of the 6 percentage-point comparisons, only one deviation occurred and that was in 1958, ironically the midterm election which has probably received more attention than all of the others combined. (For the southern electorate the percentage point differences were uniformly in the predicted direction and larger.) Although the turnout for these voters over the years does not help us in deciding between the consistency and negative voting models, it does argue persuasively against accepting the null hypothesis.

Among independents, for whom only the negative voting model predicts differential rates of turnout, we find that those citizens who disapprove the President's job performance uniformly showed up at the polls in higher percentages.[38] Earlier we reviewed the surge and decline thesis which argues that the midterm resurgence of the loser two years earlier can be largely explained by the withdrawal of marginal voters–disproportionately numbered among independents–from the midterm electorate. The present findings suggest that the independents' decline in participation may not occur at an equal pace among the Presidents' admirers and detractors, however. The contented, so the negative voting model argues, have less incentive to vote. Table 4 provides more direct evidence on this point. Here the sample consists only of independents who reported voting in the preceding presidential election. Consistent with the relationships reported above, the dropout rate from the presidential to the following congressional elections two years later is uniformly greater among the president's approvers.[39] In each election at least four-fifths of those independents who disapprove remain in the electorate. The size of the percentage point differences, however, varies greatly and perhaps is in part attributable to the size of the subsamples. The weak turnout among President Truman's followers in both the 1946 and 1950 elections evidences one aspect of the Democratic party's poor showing during those years.

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[37] Within the South, the percentage point differences were −18 points for 1966. For 1954 and 1958 there were too few southern Republicans who disapproved to permit computation of percentages. Note that the higher turnout among disapprovers in both regions was particularly strong during the last two midterm elections for which there is evidence. If this indicates a forming trend, it may reflect another symptom of the much belabored and recurrent theme of weakening party ties. With party identification becoming less important, attitude conflict involving party identification is less likely to be resolved in its favor. For a sampling of this literature see Walter Dean Burnham's "The Changing Shape of the American Political Universe," *American Political Science Review*, 59 (March, 1965), 7–28. This thesis is thoroughly explored in Philip E. Converse, "Change in the American Electorate," in *The Human Meaning of Social Change*, ed. Angus Campbell and Philip E. Converse (New York: Russell Sage Foundation, 1972), pp. 263–337.

[38] Independents in the present paper include respondents who when questioned further acknowledged leaning toward one of the two parties. Thus some of the "effects" of presidential popularity reported are open to the suspicion of partial spuriousness from concealed partisanship. Therefore, the analysis was replicated where possible using this purer group of independents; except for added random variation due to the reduced sample size, the same conclusions hold. Including leaners among identifiers

Table 4. The Percentage of Independents Who Voted in Both the Midterm and Preceding Presidential Election[a]

	Presidential Popularity		
	Approve		Disapprove
1946	55 (148)	<	84 (270)
1950	39 (61)	<	84 (119)
1954	65 (110)	<	81 (37)
1958	68 (121)	<	82 (61)
1962	73 (152)	<	80 (35)

[a]Southerners included.

To summarize, with a few noted exceptions, midterm electorates over the years have been marked by somewhat greater turnout among those citizens who disapprove of the President's job performance. The negative voting model clearly attributes this difference to the motivational impact of disapproval. One may plausibly argue, however, that the association between disapproval and turnout is produced by greater political involvement among some segments of the electorate. Many of the social and psychological antecedents of high participation may also lead the individual to assume a more critical, less deferential posture toward political leaders. To discover that the correlation between presidential evaluation and turnout is in this manner spurious, would in fact substitute an equally important explanation for midterm voting. A discussion of this explanation and the evidence testing it are presented in an appendix. Although limitations of the data make it impossible to determine conclusively which causal process produces this relationship, the available evidence (described in the appendix) falls heavily in favor of a *direct* one advanced by the negative voting model.

Discovering that citizens who disapprove of the President are somewhat more likely to vote in the midterm congressional elections tells us only part of the story. The crucial question remains, do many citizens make their congressional vote choice on the basis of their evaluation of the President's performance? Without the additional influence of presidential popularity on actual voting decisions the significance of the relationship for turnout described above would be trivial.

Predictions II and III: Presidential Popularity and Partisan Voting. Although partisan defections constitute only a small portion of the total midterm vote (see Table 1), they may be very important for explaining marginal variations in each party's share. Defection suggests attitude conflict; evaluations of competing candidates are based on criteria which are in conflict with, and more important than, party identification. In investigating the contribution of presidential popularity to national fluctuations of party successes, close attention should be given to the relatively small but important volatility of this vote.

In Table 2 the competing predictions concerning partisan defections for each of the three models are delineated. Briefly, the null hypothesis predicts that there should be no systematic differences in defection rates between the President's approvers and his disapprovers. The consistency model predicts that persons whose evaluation of the President is incongruent with

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would have merely transferred this issue of hidden spuriousness to the party identifiers.

[39] The relationships between turnout and popularity are similar for the South and have been included in Table 4 to increase the sample size. The reported presidential vote for 1964 was unavailable, and therefore, the 1966 data had to be excluded. Of course, evidence based on recall two years earlier must be looked upon with circumspection. In this instance a systematic recall bias–as compared to fading memory which distributes randomly–does not appear to be a problem.

their own party identification should defect in larger proportions than voters whose presidential evaluation is consistent with party identification. The negative voting model includes this prediction and goes further to suggest that presidential disapproval, when in conflict with party identification, will produce greater strain toward defection than will approval in a similar context. Consequently, in any given midterm election, the rates of defection among identifiers of the President's party who disapprove of their incumbent should be higher than among identifiers of the other party who approve of his job performance, or according to the schema given in Table 2, "b>c." Similarly, disapproval should more strongly reinforce existing party affiliation when congruent. Here again, members of the "out" party should be more inclined to vote the party line (or a<d in Prediction III) according to negative voting. If these hypotheses are verified, we shall have gone some distance in explaining why the political parties seem to perform more poorly in midterm elections when they occupy the White House.

In Table 5 we find that respondents whose evaluations of the President are consistent with party identification uniformly display higher levels of party voting. The percentage point differences are not great, but they do reflect a stable if small relationship between presidential popularity and congressional voting preferences. Moreover, in each instance defections are greater for members of the President's party.

The greater influence of disapproval in producing defections (and preventing them) can also be seen by comparing instances of attitude conflict (Prediction II) and reinforcement (Prediction III) across partisan groups. In every midterm election the level of partisan defection has been higher among disapprovers within the President's party (category b of Table 2) than among his approvers within the other party (category c). These differences in defection can be seen in the first row of Table 6. The prediction of negative voting, and not the consistency model, is borne out. *Disapproval of the President is more influential than approval in producing defections when in conflict with party identification.*[40]

As to the reinforcement prediction (III), again the negative voting thesis is confirmed. Disapprovers within the other party (category d) in row 2 of Table 5 display extremely high levels of party loyalty in each election. (Keep in mind that in 1954 and 1958 this category of voters is composed of Democrats.) Approvers within the President's party (category a), while showing the effects of reinforcement, fail in all six elections to reach the same extreme level of party voting.

Table 5. Presidential Popularity and Partisan Defections (Non-South), 1946–1966 (Percentage Voting for Own Party's Congressional Candidate)

Party Identification	Presidential Popularity	Category (Table 3)	Midterm Election Year					
			1946	1950	1954	1958	1962	1966
Same as President	Approve	a	89	96	96	93	90	92
		N =	(284)	(92)	(225)	(219)	(950)	(463)
	Disapprove	b	78	79	–	81	62	78
		N =	(218)	(164)	(8)	(24)	(13)	(123)
		Difference	11	17*	–	12	28	14*
Other Party	Approve	c	90	93	89	89	91	90
		N =	(165)	(40)	(92)	(119)	(370)	(155)
	Disapprove	d	98	98	97	96	98	97
		N =	(603)	(243)	(126)	(138)	(238)	(341)
		Difference	8*	5	8	7	7*	7

*Percentage point differences significant at .05 or better.

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[40] At this point some bemused readers may be entertaining the idea that "approval" and "disapproval" do not measure sentiments of equal intensity. Perhaps disapproval represents an extreme response volunteered only when the respondent feels strongly on the issue. After all, when a new president enters office without a track record a large majority of the public prefer to approve his job performance rather than withhold judgment. If approval frequently substitutes for no opinion then it is not too surprising to find it relatively uninfluential in guiding voting choices. Two pieces of evidence challenge this argument: First, from all of the available surveys, when respondents were asked their opinion on a ten-point "strongly like" to "strongly dislike" scale, a majority who disapproved of the President's job performance, nonetheless, placed themselves on the liking end of the scale. This suggests that disapproval should not be viewed as registering extreme negative evaluation. Second, Richard Boyd, using a refined five-point job performance index from the 1968 SRC survey, finds a very similar relationship, with the benefits accruing from strong approval ("very good") not nearly so great as the harm caused by strong disapproval ("very poor"). Boyd, "Popular Control of Public Policy . . . ," Figure 9, p. 440.

Table 6. Disapproval of the President as More Powerful in Producing Defections and Reinforcing Party Identification (Percentages drawn from Table 5)

	1946	1950	1954	1958	1962	1966
Attitude Conflict (Percentage Defecting) Prediction II. $b > c$	+12[a]	+14	–	+8	+29	+12
Reinforcement (Percentage Voting Party-Line) Prediction III. $a < d$	+9	+ 2	+1	+3	+ 8	+ 5

[a] Percentage point differences between identified categories. Positive differences are in the predicted direction.

To summarize, negative voting operates to reduce the presidential party's share of the congressional vote through party defections in two ways. First, in cases of attitude conflict between the evaluation of the President and one's party identification, disapproval produces more defections than does approval. Second, in situations of congruence between these attitudes, disapproval reinforces party line voting (within the opposition party) more strongly than approval (within the President's party). Approvers remain more susceptible to counter influences. *In sum, because of negative voting the President's party is more vulnerable to midterm partisan defection.*

Other things being equal, exit from the President's party at the midterm should exceed movement toward it. But other things being rarely equal, a number of variables may obscure the independent effects of negative voting. Primary among them is the unequal size of the political parties which has fluctuated from virtually even in 1946 to a 3:2 ratio in favor of the Democrats by 1966. Consequently, defections consistently came more frequently from the Democratic party (see Table 7, row 1). Even in 1954 and 1958 with a Republican in the White House, Democratic defections at least equaled those of the President's party. In row 2, however, the effect of unequal size has been eliminated and percentages more closely reflecting the effect of negative voting can better be appreciated. Adjusted for party size, the expected pattern emerges clearly with the President's party now containing a significantly larger share of party defections in each election.

Prediction IV: Presidential Popularity and Midterm Preferences of Independents. Independent voters represent a rather sizable percentage of the midterm electorate (approximately 20 per cent, including leaners). Moreover, without strong attachments to one of the political parties, independents should be more susceptible to the influence of other political forces such as presidential popularity. Given these features, they may represent a key group of voters for explaining systematic but marginal variations in the overall congressional vote—variations which parallel fluctuations in presidential popularity.

The null hypothesis, which has been thoroughly rejected thus far, predicts that the President's approvers and disapprovers divide the congressional vote between the two parties equally. The consistency model makes much the same prediction but for a different reason. Rather than having no effects, approval and disapproval are viewed as influencing independents similarly. Because the relative strengths of the two evaluations are assumed to be equal, the model predicts that approvers will vote for the President's party at the same rate as disapprovers vote against it—that is, for Prediction IV of Table 2, "$e = f$." Negative voting predicated on the thesis of the differential influence of negative evaluations predicts a different outcome. Disapprovers should vote against the President's party to a greater degree than approvers will vote for it, or "$e < f$."

From Figure 3 we can see that, to a substantial degree, disapproval of the incumbent administration will be reflected in a

Table 7. Contribution of Negative Voting to Increased Defections Among the President's Party Identifiers

	1946	1950	1954	1958	1962	1966
Percentage of total defections from President's party	75	71	50	46	74	76
Percentage of total adjusting for party size	75	57	71	67	62	64

support for the opposition party's congressional candidates. In only one midterm election, that of 1950, when there were few independents approving President Truman's job performance, did the President's party substantially benefit from approval among independents. On the other hand, in two of the elections, 1946 and 1966, even independents who approved the President favored the opposition party candidates in larger numbers. *The real advantage of being a popular president comes primarily through a reduction in the size of the group of disapprovers. For while approval of his performance gives the presidential party's congressional candidates no large advantage over the opposition, disapproval certainly places his party at a distinct disadvantage.*

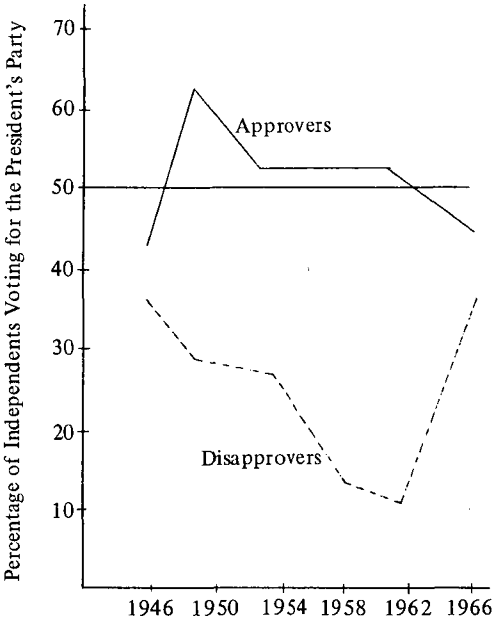

Figure 3. Extent to Which Disapprovers are More Likely to Vote against the President's Party than are Approvers to Vote for It (Percentages for non-South only)

Summary of Individual-Level Findings

The preceding analysis has uncovered the following:

(1) With the exception of members of the President's party for whom the evidence is inconclusive, disapproval of the President's job performance is associated with higher midterm turnout.

(2) When the party identification of the respondent is controlled, presidential popularity is found to correlate with congressional preferences.

(3) Disapproval of the President is a stronger source of party defection than is approval.

(4) Among independents, disapproval appears to exercise a greater influence on voting choices.

Mutually complementary, these findings together offer strong support for the negative voting model which has been proposed here to explain marginal variations in midterm party fortunes. Negative voting provides an alternative to surge and decline for explaining the midterm electoral decline of the President's party. It is not a disproportionate withdrawal of independents from the electorate which explains the regular midterm shifts, for in fact independents maintain their contribution to the total midterm vote (see Table 1). Rather, in explaining midterm losses, one should know that independents who do remain in the electorate at the midterm disapprove of the President in greater numbers than those who drop out (Table 4). Among the independents who vote, disapprovers appear more likely to base their congressional choices on their presidential evaluation than do approvers. And it is not important that the level of partisan defections declines (for it does not), but that the defections at the midterm occur *disproportionately within the President's party (Table 7), primarily among his detractors (Table 6).* The electoral biases against the administration party erase that party's gains made two years earlier.

In addition, the negative voting thesis allows us to address important movements of public preferences across a *series* of midterm elections. Perhaps the sensitivity of negative voting to politically important, marginal shifts in the total vote is what most commends the negative voting model to our attention. Unlike other sources of defection (such as friends-and-neighbors voting) which tend to cancel out, defections based on negative voting accumulate systematically across congressional districts. In order to appreciate more fully the relationship between the President's popularity and variations in aggregated totals, we return now to where we began—namely, to variations in the congressional vote over time.

Aggregating Individual-Level Relationships

Moving from individual-level relationships to marginal variations in the national congressional vote presents several difficulties. One has to deal with the unequal contributions of presidential approval and disapproval on both turnout and defection. More confounding is the averaging of rates of turnout and defection, which vary in strength from year to year. Add to this the assumption that presidents become more or less popular among all partisan groups at roughly the same rate.[41] Finally, include in the estimate the uneven distribution of party loyalties in the electorate, which because of the greater influence of negative evaluations means that the larger the President's party the greater the electoral costs of becoming less popular. Given these problems, any estimation of the aggregate effects of popularity must be taken as tentative. With these caveats in mind the calculations were made, however, and they indicate that *for every nine-point change in the percentage approving of the President, his party's congressional vote will change 1.4 percentage points.*[42]

Of course, an easier way of reaching the same results is to work directly from aggregate percentages such as those displayed in Figures 1 and 2. Edward Tufte has performed a regression analysis of these data and concluded that "a change in presidential popularity of 10 percentage points in the Gallup Poll is associated with a national change of 1.3 percentage points in the national midterm vote for congressional candidates of the President's party."[43] It is gratifying to find that by using negative voting, estimates based on survey research are independently confirmed by the actual congressional vote.

Richard Nixon and the 1974 Congressional Elections

Earlier it was suggested that President Nixon's public standing at the time of his resignation may have produced the Republican disaster in the 1974 congressional elections. Although the extraordinary nature of events made the election less suitable for testing the negative voting thesis, we can, nonetheless, trace the erosion of public support for Republican candidates into late summer.

The most visible indications of Republican trouble were the consecutive Democratic victories in congressional by-elections for traditionally safe Republican seats. But politicians who must augur the political future far in advance had been aware of Republican vulnerability because of Watergate since the beginning of the year. In mid-January when the Republican governors convened in Memphis, it was no coincidence that the featured speaker was George Gallup. By early April, twenty-two House and Senate Republicans had already announced their retirement. Throughout the Spring, Senator Goldwater had been advising his colleagues that any Republican incumbent who had received less than sixty per cent of the vote in the previous election was in jeopardy in November.[44] Were Republican forebodings well-founded? The negative voting thesis would suggest that they were.

In Figure 4, responses to the presidential job performance and congressional preference items are plotted for all of the available Gallup polls during President Nixon's tenure. Even the most casual perusal of the trends reveals a close relationship between these variables. Statistically, President Nixon's job performance rating explains more than three-quarters of the variance in the percentage who prefer a Republican victory in their districts' next congressional election. In Figure 4 the x's which shadow the congressional preferences are the estimates of per cent Republican "predicted" from the President's popularity.[45] They offer impressive visual evidence of the explanatory power of presidential popularity at the aggregate level. Clearly many Republicans' ambivalence about being too closely associated with their president was well founded. President Nixon was pulling the Republican party down with him.[46]

Discussion: Negative Voting and Other Electoral Settings

At first glance, midterm congressional elections would appear to be unlikely forums for negative voting. The central figure—the Presi-

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[41] John E. Mueller, *War, Presidents and Public Opinion* (New York: John Wiley and Sons, 1973), chapter 10.

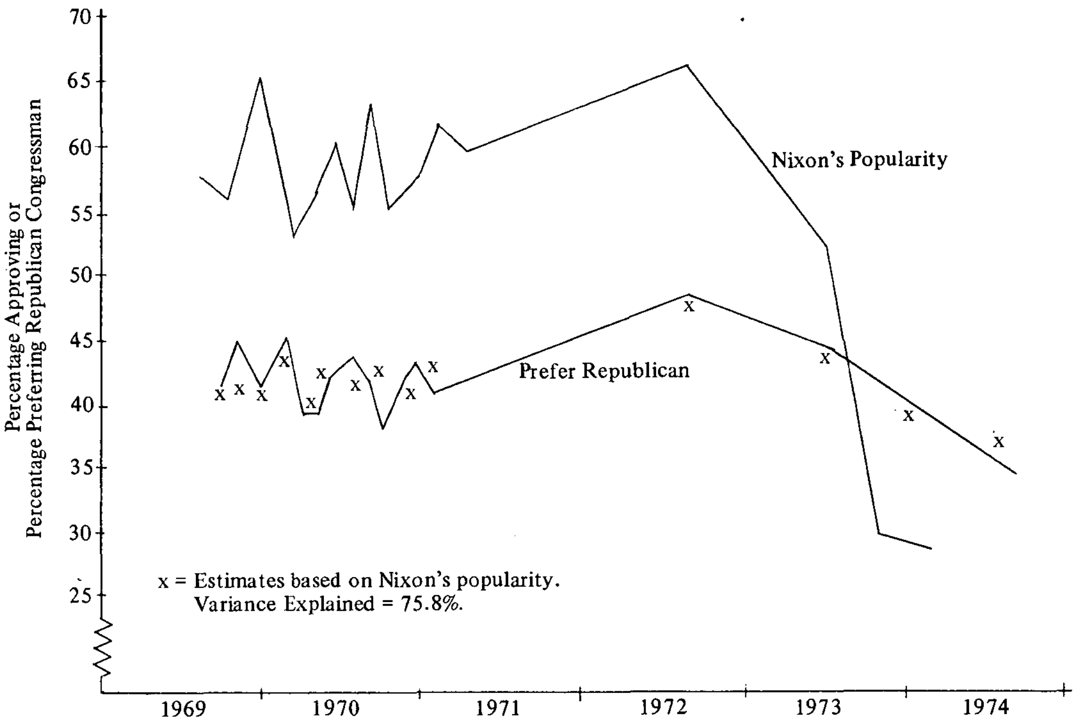
[42] During Republican administrations this figure is slightly lower at 1.3 percentage points.

[43] Edward Tufte, "Determinants of the Outcome of Midterm Congressional Elections," *American Political Science Review*, 69 (September, 1975), 812–826.

[44] Christopher Lydon, "The Awful Arithmetic," *New York Times*, April 21, 1974; R. W. Apple, Jr., "Election Problem of G.O.P. Essayed," *New York Times*, February 9, 1974.

[45] Regression analysis further reveals that a ten-percentage point decline in President Nixon's popularity reduced preference for his party's candidates by 2.67 percentage points. Because the percentages in Figure 4 refer to the preferences for the entire public—with nonvoters included—they may not in fact reflect the potential vote. Probably inclusion of nonvoters who disproportionately favor the Democratic party significantly underestimates Republican strength, despite the fact the negative evaluations are more stimulating. None of the estimates appear to be contaminated by serial correlation according to the Durbin-Watson test.

[46] To see if the Watergate affair has had any special influence on Republican preferences other than through Nixon's popularity, the analysis was repeated with the data since the election of 1972 excluded. The projection of the slope for the pre-Watergate data estimates closely the proportion favoring a Republican victory given the reduced support for President Nixon.



x = Estimates based on Nixon's popularity.
Variance Explained = 75.8%.

Figure 4. Strong Correlation of President Nixon's Popularity With Public Preferences for his Political Party.

dent—is not on the ballot, and the offices being filled belong to a constitutionally separate institution. On closer inspection, however, two features make midterm elections particularly suitable for negative voting: Unlike presidential elections, the President is the only highly visible national actor in public view. Given poorly identified congressional contenders in many districts and no national counterpart from the other party, the president becomes the most prominent reference for choosing between candidates. Second, the midterm election is a period of low voter stimulation as evidenced by the overall lower turnout. With fewer additional incentives to participate, the differential effects of negative voting on turnout should be especially great.

Both of these ingredients are absent during presidential elections. For example, the number of political references are greater. An incumbent president as a candidate is contrasted with his opponent, and strongly held opinions about the challenger may well prove more important. Such a situation has been frequently mentioned to explain the 1972 election outcome, with voters viewed more as voting *against* McGovern than as voting *for* Nixon.[47] Also, presidential elections provide far more sources of voter stimulation, perhaps reducing the relative contribution of negative voting in influencing participation.

With some tailoring, however, the negative voting model could be adapted to a presidential election setting. Richard Boyd, in his normal vote analysis of the 1968 election, reports a relationship strongly suggestive of negative voting: ". . . those who thought well of Johnson's performance outnumbered those who thought poorly of it by a comfortable margin. However, Johnson's admirers gave Humphrey only a normal Democratic vote, while his detractors voted heavily against him."[48]

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[47] There is some evidence which argues this view, however. In a late August Gallup survey, respondents were asked, "Would you say that your choice for President is more a vote FOR your candidate or AGAINST the other candidate?" Of those with an opinion, only 28 per cent viewed their vote as against the other candidate (*The Gallup Opinion Index,* October, 1972, p. 7).

[48] Boyd, p. 440.

Elections in parliamentary systems pose some interesting issues for the negative voting thesis. Unlike midterm congressional elections, parliamentary elections are attended by national candidates from each party barnstorming the country with evaluations of the "chief executive" (i.e., prime minister, chancellor, premier, etc.) rarely the only available cues. And in countries which have mandatory voting, the differential motivational impact of negative evaluations on turnout will be eliminated. Other features of parliamentary systems, however, could conceivably lead to higher levels of negative voting. The usual constitutional attachment and partisan congruence of the chief executive with the legislature may promote a close association between evaluations of the chief executive and voting for his party's legislative candidates.[49] Also, to the degree that legislative representatives are less well known to their constituency, there will be fewer "local" references competing with evaluations of the chief executive.[50] By investigating the importance of approval and disapproval of the administration on voting choices for a variety of political settings, we should arrive at a better appreciation of the effects of structural features of political systems on short-term political change.

Appendix. Examination of Alternative Explanation of Turnout

There is some reason to suspect that general political involvement may produce both higher levels of disapproval of the President and greater voting turnout. Fred Greenstein has described the President as a "cognitive aid" for the less sophisticated public.[51] One may speculate that the President should receive a more positive evaluation from those citizens who are dependent on him for understanding and evaluating the political environment. On a similar theme Kernell, Sperlich, and Wildavsky have found that adherence to norms that the President should be supported comes disproportionately from the less politically aware and less participant segments of the population.[52] Greater levels of education, psychological flexibility, and political sophistication are all associated with a tendency to disavow blanket support for the President. If we translate low support for norms into actual support for the incumbent President, we may find persons who are *more* likely to vote in low-stimulus elections are also as a group *less* generous with the President, whoever he might be.

In order to test this argument we first need to determine whether high levels of political sophistication and involvement are in fact related to a more critical posture toward the President. In examining this question, we must avoid a conceptual snag. If, as the negative voting model hypothesizes, disapproval activates electoral participation it should concomitantly foster political interest in the campaign and election outcome. Implicit in negative voting then is the claim that political interest—that is, interest about the upcoming election—may directly reflect disaffection with the incumbent administration. Short-run interest measured by such variables as campaign participation, perceived importance of elections, attention to the campaigns, and the desire to vote should, according to *both* the negative voting thesis and the alternative explanation, be associated with presidential approval. Most measures of political interest and involvement employed in election-year surveys focus on these short-run qualities and are therefore for our purposes less helpful in comparing the competing explanations.

What is needed to test for spuriousness are measures soliciting information about more general and durable forms of political involvement and sophistication. Variables representing a continuing interest in politics and reflecting in large part the cognitive and expressive skills of the individual should be most relevant to a "critical" evaluation of a president's job performance. Operational measures of such variables are not nearly so abundant in public opinion surveys as those tapping transitory campaign interest. In fact, the conceptual variable which I shall call general political involvement is rarely distinguished from and generally combined with campaign interest to measure a more encompassing and poorly specified quality of political interest. Given this deficiency it is difficult to determine conclusively whether general political involvement leads to higher rates of disapproval. On those occasions where the Gallup poll administers an item which may be interpreted as tapping general involvement, the item is usually asked just prior to the election in order to filter out potential nonvoters. Given this timing, such items could easily become contaminated by transient enthusiasm for the upcoming election; the SRC surveys have a similar "problem" of timing.

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[49] There is some evidence of a close association between voting preferences and evaluations of the chief executive. For Great Britain, see C. A. E. Goodhart and R. J. Bhansali, "Political Economy," in *Political Studies*, 18 (March, 1970), 43–106. For France, which is more presidential than parliamentary, see Jean Charlot, *Les Français et De Gaulle* (Paris: Plon, 1971), pp. 215–238, 245.

[50] Susan E. Howell compares legislator visibility for five systems in "System Effects on Legislator Visibility in Five Democratic Countries" (paper presented at the 1974 APSA meetings).

[51] Fred I. Greenstein, "Popular Images of the President," in *The Presidency*, ed. Aaron Wildavsky (Boston: Little, Brown, and Co., 1969), pp. 287–295.

[52] Samuel Kernell, Peter W. Sperlich, and Aaron Wildavsky, "Public Support for Presidents" in *The Presidency*, 2nd ed. (Boston: Little, Brown and Co., 1975), pp. 148–181.

Given these limitations of the data, perhaps one of the more revealing measures for general political involvement is the respondent's education. Repeatedly education has been shown to be closely associated with such variables as political sophistication, a sense of political efficacy, and various forms of political participation, and is commonly viewed as one of their primary antecedents. According to the alternative explanation, education should correlate inversely with presidential approval; that is, more highly educated respondents should be less likely to approve the incumbent's job performance. To test this I ran correlations between presidential popularity and educational achievement for each partisan group for fifty-eight different Gallup surveys spanning a twenty-year period. Of the 174 different correlation coefficients (Pearson r), none reached even .25, either positively or negatively. With the exception of the Truman presidency, where small but persistent inverse correlations (near −.10) were found, correlations were weak and just as likely to be positive as negative. Given this substantial evidence, there can be little doubt that *increased education fails to produce systematic negative assessments of presidents.* Citizens who on the whole should be more knowledgeable and attentive to day-to-day political occurrences are apparently no more likely to disapprove than are those citizens who are unsophisticated and disinterested in politics.[53]

Occasionally questions soliciting information about general and long-standing political involvement were asked in the Gallup survey. Note in Table A-1 that on most occasions the items were asked during the middle of an election campaign. Despite their manifest content of general involvement, it remains uncertain whether such items actually elicit basic, enduring dispositions or instead reflect current political interest stimulated by disapproval as well as the election campaigns. For the most part these questions yield only slight inverse relationships with presidential popularity, certainly not of a magnitude to suggest that the relationship between presidential approval and voting turnout displayed in Table 3 could be spurious.

One last set of data will be examined to test the alternative model. The SRC presidential election surveys have over the years been noted for their breadth of coverage of political beliefs and attitudes. These surveys represent the best source available for evaluating various forms of political interest and sophistication. Unfortunately, only in 1972 was Gallup's presidential popularity item administered to SRC's nationwide sample.[54] Thus, in evaluating the relationship between presidential popularity and different types of political interest, one must remember that the setting is a presidential—not a midterm—campaign occurring six years after the last midterm election analyzed in this paper. To the degree that the percentage point differences shown in Table A-2 support any thesis, they buttress the negative voting model. Presidential popularity is inversely related to interest and participation in the current campaigns just as negative voting suggests, but as we move to the more general statements of political involvement, these inverse relationships disappear which is the opposite of what one would expect if the alternative explanation were accurate. *For each of the items eliciting general political interest, the President's approvers—not his disapprovers—were the more politically involved.*[55]

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[53] Collapsing categories of education and checking for curvilinearity failed to uncover any systematic relationships hidden by the statistics.

[54] I wish to thank Professor Richard Brody for allowing analysis of restricted data.

[55] Controlling for party—Strong Democrat, Weak Democrat, Independent, Republican (the strong and weak categories had to be combined because of a small *N*)—the relationships reported in Table A-2 remain essentially unchanged.

Table A-1. Relationship Between Presidential Approval and Opinion Items Measuring General Political Involvement (Items from Available Gallup Surveys)

		Presidential Popularity	
Poll Date	**Item**	**Disapprove**	**Approve**
10/20/1950	Do you vote in all elections or only those that interest you?		
	Those that interest	21%	28%
	Qualified	5	8
	All	74	64
		100	100
10/7/1952	Generally speaking, how much interest would you say you have in politics?		
	None, Little	23	30
	Fair	51	48
	A great deal	26	22
		100	100
10/7/1952	How often would you say you vote?		
	Never, Seldom	11	15
	Part time, Nearly always	40	39
	Always	49	46
		100	100
4/12/55	Have you ever voted in any election or don't you pay attention to politics?		
	No attention, Never Vote	12	17
	Have voted	88	83
		100	100
10/13/58	How often would you say you vote?		
	Never, Seldom	14	14
	Part time, Nearly always	35	33
	Always	51	53
		100	100
10/13/60	Generally speaking, how much interest would you say you have in politics?		
	None, Little	23	24
	Fair	41	53
	A great deal	36	23
		100	100

Table A-2. The Relationship Between Presidential Popularity and Various Forms of Political Interest (1972 SRC Pres. Election Survey)

	Presidential Popularity	
	Disapprove	Approve
Interest in the Political Campaign		
1) Some people don't pay much attention to political campaigns. How about you? (Item #163)		
Very much	34%	29%
Somewhat	37	46
Not much	29	26
	100	101
2) Campaign Participation Index (#468, 469, 470, 471, 472)		
High (3-6)	11	5
Moderate (1-2)	31	33
Low (0)	58	63
	100	101
General Political Involvement		
3) Some people seem to follow what's going on in government and public affairs most of the time, whether there's an election going on or not. Others aren't that interested. Would you say that you follow what's going on in government and public affairs . . . ? (Item #476)		
Most of the time	37	38
Some	35	38
Only now and then/Hardly at all	28	25
	100	101
4) Aside from this particular election campaign, here are some other ways people can be involved in politics. Have you ever written a letter to any public official giving them your opinion about something that should be done? (Item #474)		
Yes	27	26
No	73	74
	100	100
5) In the elections for president since you have been old enough to vote would you say you have voted in . . . (Item #156)		
All of them	45	52
Most	26	22
Some	16	16
None	13	11
	100	100

Thought Reform and Cultural Revolution: An Analysis of the Symbolism of Chinese Polemics*

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Can it be that "national character"
is so difficult to change?
If so, we can more or less guess
what our fate will be.
As is so often said,
"It will be the same old story."

–Lu Hsün (1925)**

The weapon of criticism certainly cannot replace the criticism of weapons; material force must be overthrown by material force; but theory, too, becomes a material force once it seizes the masses. Theory is capable of seizing the masses once it demonstrates *ad hominem*, and it demonstrates *ad hominem* once it becomes radical. To be radical is to grasp matters at the root. But for man the root is man himself.

–Karl Marx***

Ten years after the Chinese Cultural Revolution, the various theories about its causes and purpose seem to have sorted themselves out into two main schools of thought: "Power struggle" and "line struggle." The former interprets the Cultural Revolution as a premature succession struggle involving factions formed on an expediential *ad hoc* basis in order to dominate positions of formal leadership. The latter interpretation views the Cultural Revolution as the outcome of a long struggle between two policy "lines" whose positions on a wide series of issues were internally consistent and mutually exclusive.

Although the Cultural Revolution certainly involved widespread "struggle" for both power and principle, both of these explanations tend to reason *post hoc ergo propter hoc* and to confuse *effects* with *intentions*, and neither provides a logically sufficient explanation. The basic problem with the "power struggle" thesis is that there is still no conclusive evidence (and the Chairman himself has always denied) that Mao had been forced from power prior to 1966 and was therefore induced to mobilize forces outside the Party in order to regain his position of influence.[1] Power struggle among competing factions at both the mass and elite levels was clearly an *epiphenomenon* of the Cultural Revolution, but not necessarily its cause. The "line struggle" interpretation can also be discounted, except in the area of cultural policy (and particularly concerning the issue of "cultural revolution" itself), because the notion of two distinct policy "lines" within the Politburo is a largely inaccurate *ex post facto* verdict based on generalizing to the past the errors committed during the Cultural Revolution.[2] If the purpose of the movement were to resolve a "line struggle" that had reached a deadlock within the top leadership by airing the issues in a larger forum, one would expect a profound transformation of policy to take place as soon as the struggle had been decided; but in fact we find that the struggle was followed by the reconstruction of pre-1966 political and socio-economic institutions with surprisingly little alteration, except in the cultural and educational realms.[3]

This paper's assumption is that the central purpose of the Cultural Revolution was to transform China's political culture, to achieve the same sort of "revolutionary breakthrough" in the cultural sphere that had already been achieved in the military-political and socio-economic realms.[4] This assumption is premised partly on the testimony of Mao Tse-tung himself, who did more than any other single person to precipitate the Cultural Revolution. "In the past you were concerned with only industry, agriculture, and communications; you have never thought of *cultural* revolution," he pointed out to some Central Committee colleagues early in 1966. "Now we must turn our thoughts to this."[5] Mao had indicated in many of his recent statements that he now considered the transformation of the socioeconomic infrastructure less important than the transformation of the cultural superstructure, either because the former had already been more or less completed or because the latter would have greater and more lasting impact on human behavior.[6] Thus in a speech given in the summer of 1967 he relegated the "struggle between two lines" to secondary importance, emphasizing that the goal was to "transform . . . the cultural superstructure," to "construct a revolutionary world-view and eliminate the roots of revisionism."[7] He was haunted by the prospect that the revolution might rigidify around the emotions of elite arrogance and complacency and mass inhibitions toward authority, a process he already saw occurring in the Soviet Union. That these were Mao's motives does not imply that he knew exactly what he was doing and how to do it and should therefore be held responsible as the purposive agent behind everything that occurred; nor does it imply that he was successful in his vague and ambitious endeavor. On the contrary, the movement unfolded toward its objectives in a fitful and seemingly experimental way, and "developed so rapidly as to surprise" even the Chairman.[8] And it was quite mixed in its consequences. But if these were Mao's motives, then the Cultural Revolution must be analyzed and evaluated in a different light, for his purpose presumably inspired not only his own actions but also informed many of the events that took place in his name with a logic not otherwise comprehensible.

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*I wish to thank Tang Tsou, Nathan Leites, Susanne Rudolph, John Starr, and many others for their comments on earlier drafts. I am also grateful to participants in a China seminar at the University of Toronto in November 1974 for their suggestions on an earlier version.

**"Sudden Notions," *Selected Works of Lu Hsün* (Peking: Foreign Languages Press, 1957), vol. 2, p. 108.

****Critique of Hegel's "Philosophy of Right,"* ed. Joseph O'Malley (Cambridge: Cambridge University Press, 1970), p. 137.

[1] Cf. J. D. Simmonds, *China: Evolution of a Revolution, 1959–1966* (Canberra: Australian National University, Department of International Relations Working Paper no. 9, 1968).

[2] See my "Theory and Practice of 'Line Struggle': The Origins of the Chinese Cultural Revolution Reconsidered" (paper presented at the 28th meeting of the Association for Asian Studies, March 19–21, 1976, Toronto, Canada).

[3] A. Doak Barnett, *Uncertain Passage: China's Transition to the Post-Mao Era* (Washington, D.C.: The Brookings Institution, 1974).

[4] Cf. Kenneth Jowitt, *Revolutionary Breakthroughs and National Development: The Case of Romania, 1944–1965* (Berkeley: University of California Press, 1971).

Given the assumption that cultural transformation was indeed the purpose of the Cultural Revolution, the purpose of this paper is to show what this transformation involved and how it took place. This is not the first attempt to analyze the Cultural Revolution as a "cultural" revolution, but earlier attempts have encountered various conceptual difficulties that I hope to avoid. Robert Lifton discarded the psychoanalytic techniques he used so insightfully in his early study of thought reform, correctly observing that "the general psychological principles governing the thought reform process could not adequately illuminate the Cultural Revolution." Yet his own attempt to formulate a method of analysis more suitable to the Cultural Revolution confines itself to an examination of Mao's personal motives for launching the movement, implicitly assuming the rest of China to be Mao Tse-tung writ large.[9] Lucian Pye's *Spirit of Chinese Politics* and the methodologically more sophisticated, but thematically derivative, work of Richard Solomon[10] also tend, in my judgment, to oversimplify the process of cultural transformation. As in Lifton, change is construed as "the effort of one highly unusual, powerful man—Mao Tse-tung—to transform Chinese society."[11]

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[5] Mao Tse-tung, "Speech at a Central Committee Work Conference" (October 25, 1966), in *Selections from Chairman Mao,* translated by U.S. Joint Publications Research Service *(JPRS),* no. 49826 (February 12, 1970), p. 15. Emphasis added.

[6] In his letter to Joseph Bloch written September 21–22, 1890, Engels said: "We make our history ourselves, but, in the first place, under very definite assumptions and conditions. Among these the *economic ones are ultimately decisive.* But the political ones, etc., and indeed even the traditions that haunt human minds also play a part, although not the decisive one." Lewis S. Feuer, ed., *Marx and Engels: Basic Writings on Politics and Philosophy* (New York: Doubleday Anchor, 1959), p. 398 (emphasis added). In contrast, Mao asserted: "True, the productive forces, practice and the economic base generally play the principal and decisive role; whoever denies this is not a materialist. But it must also be admitted that in certain conditions, such aspects as the relations of productions, theory, and the superstructure in turn manifest themselves *in the principal and decisive role." Selected Works* (Peking: Foreign Languages Press, 1965), vol. 4, p. 336. In 1959 or 1960, Mao wrote in criticism of Stalin's *Economic Problems of Socialism in the Soviet Union:* "From the beginning to the end, this book by Stalin has never talked about superstructure and has not considered man; it sees things but not man. . . . The viewpoint of Stalin's last letter is almost completely wrong." *Mao Tse-tung ssu-hsiang wan sui* [Long live the Thought of Mao Tse-tung], 1967 edition, p. 156 (emphasis added).

[7] Mao, "Speech to Foreign Visitors" (August 31, 1967), in *Ming Pao,* July 5, 1968, p. 1.

[8] Mao, *Selections,* in *JPRS,* no. 49826, pp. 13–14.

[9] Robert Jay Lifton, *Thought Reform and the Psychology of Totalism* (New York: Norton, 1961); and *Revolutionary Immortality: Mao Tse-tung and the Chinese Cultural Revolution* (New York: Norton, 1976), p. xiii.

[10] Lucian Pye, *The Spirit of Chinese Politics* (Cambridge, Mass.; The M.I.T. Press, 1968); and Richard Solomon, *Mao's Revolution and the Chinese Political Culture* (Berkeley: University of California Press, 1971). Both of these pathbreaking studies depict a static political culture being induced to change by a dynamic political personality. I hope to show that the elite/mass relationship was reciprocal and that Chinese political culture also *contains* the potential for change.

[11] Michel Oksenberg, "Political Changes and Their Causes in China, 1949–1972," *The Political Quarterly,* 45, no. 1 (January-March, 1974), 104–106.

These conceptual difficulties in the study of Chinese political culture derive in part from certain underlying weaknesses in the theoretical conceptualizations of political culture. By conceiving political culture as the subjective perception of an objective political reality, this literature assumes a normal equivalence between the two, making it difficult to conceptualize cultural conflict or change except in pathological terms. Political culture thus appears as a profoundly conservative force, functioning to reinforce the persistence and equilibrium of the system. Moreover, the conceptual boundaries between structure and culture tend to blur, obscuring the perception of political culture as an independent variable.[12] Missing from these formulations is any conception of the role of *symbols* as distinctly cultural variables mediating between political culture on the one hand and collective psychology on the other.

In the following study I shall therefore begin by articulating my own model of *political culture as a semiological system,* focusing on political symbols and on their interactions within a communications network. I shall then attempt to apply this model to the Chinese Cultural Revolution, showing how the process of cultural transformation takes place symbolically via mass criticism. In the conclusion I shall attempt to assess the social and psychological implications of this process.

Political Culture as a Semiological System[13]

"Semiology" (or semiotics) is the systematic analysis of meanings. A semiological system consists of relationships among two types of variables: communicative variables, and linguistic variables. Communicative variables consist of a minimum of two actors within a communications network: a speaker and a listener. Speakers and listeners communicate through signs. The linguistic components of a sign are a signifier (usually a word), a referent (i.e., the empirical datum signified), and a signification (i.e., the lexical meaning of the sign, a definition that applies to the entire sign class).

In the system of political culture, the relevant actors are *masses* and *elites.* They are defined here solely in terms of their communicative functions—elites manipulate symbols, masses interpret symbols and respond—and it is not necessarily assumed that elites control other values, such as wealth or even political power. Elites and masses interact through political *symbols.* Symbols are a particular type of sign, distinguished by their metalinguistic and their connotative properties. Their *metalinguistic* property is that they seem to refer to a concrete empirical datum (i.e., a "new deal" refers to a move in a card game) but actually refer to a congeries of events (i.e., a "New Deal" refers to a redistributive legislative program), which are linked by analogy or other principles of association to the original empirical datum. Their *connotative property* is that they convey value and emotion (i.e., a "new deal" represents a chance to start anew). The communicative and linguistic variables relate to one another within a political culture system in the following way:

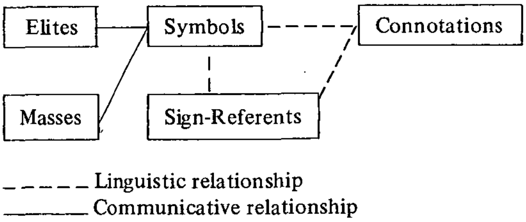
Elites
Symbols
Connotations
Masses
Sign-Referents
----- Linguistic relationship
_____ Communicative relationship

Figure 1. The System of Political Culture

This system may be analyzed on the basis of three semiological relationships among its constitutive variables: pragmatics, semantics, and syntactics.[14] *Pragmatics* refers to the communicative relationships between speakers and listeners in the network (in this case between masses and elites). From the pragmatic perspective the symbol has meaning *for someone:* special focus falls on the speaker's intention to express or communicate something and on the reactions of the listener. The *semantic* relationship refers to the relationship between the symbol and the political reality to which it refers. *Syntactics* refers to the "formal" relationship among signs, abstracted from the communication event, the speaker, and the listener; it is of particular importance in projecting the connotations of the symbol. Having conceptualized the variables comprising the political culture system and defined their interrelationships, we can now apply the model to the Cultural Revolution.

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[12] The seminal theoretical formulations of political culture are Gabriel A. Almond, "Comparative Political Systems," *Journal of Politics,* 18 (1956), 391–409; Gabriel A. Almond and Sidney Verba, *The Civic Culture: Political Attitudes and Democracy in Five Nations* (Boston, Mass.: Little, Brown, 1965); and Lucian Pye and Sidney Verba, eds., *Political Culture and Political Development* (Princeton, N.J.: Princeton University Press, 1965).

[13] For a more comprehensive elaboration of this model see my article, "Political Culture and Political Symbolism: Toward a Theoretical Synthesis" (paper presented to the American Political Science Association, September, 1976, Chicago, Ill.).

[14] Cf. Charles W. Morris, *Signs, Language and Behavior* (New York: Prentice-Hall, 1946 and 1950); and his "Foundations of the Theory of Signs," in *Foundations of the Unity of Science,* ed. Otto Neurath et al. (Chicago: University of Chicago Press, 1971), I, 77–139.

Cultural Revolution: The System

The Great Proletarian Cultural Revolution is but one of several variants of "mass movement" that recur quite frequently within the Chinese political culture system. The Cultural Revolution takes place through "mass criticism," a type of public degradation ceremony[15] that projects the basic relationships of the small-group criticism and self-criticism session to the nationwide level through the mass communications media. In the process of mass criticism, elites and masses interact through polemical symbols, or "criticisms." These criticisms refer to a target, consisting of a person or group of persons. The connotations of the criticism consist of norms and repressed grievances. The norms are the obverse of the criticisms: a criticism involves a reproach to the target because a situation is not as it ought to be, and this "ought to be" is the *norm* of the criticism, while the reproach expresses normally repressed *emotions*. The process of mass criticism fits within the model of political culture described above with only minor modification:

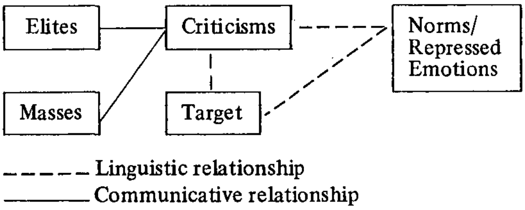

Figure 2. The Process of Mass Criticism

The basic purpose of the Cultural Revolution was to change people's ways of thinking and relating to one another, a pragmatic objective concerning the triangular relationship among elites, masses, and the target to which considerations of semantic accuracy and logical syntactic order were subordinated. By drawing the attention of the masses to the target's deviation from the norm, and by dramatizing that deviation by means of exaggerated polemical symbolism, the elites seek to persuade the masses to embrace and internalize the norms. At the same time, by permitting the masses to displace repressed negative emotions against the target, the elites seek to enhance the sense of solidarity within the community and increase the masses' commitment to the norms. The implications of this process for the target are that he should rectify his deviation through self-criticism and reintegrate himself within the community. Thus the target may hope to atone for his sins and become a model of the type of moral transformation expected of the masses. Having outlined the basic semiological network of cultural transformation through mass criticism, I shall now proceed to examine the network's pragmatic, syntactic and semantic dimensions in turn.

Pragmatics. We have noted that pragmatics focuses on the speaker's intentions to express or communicate something and on the reaction of the listener, unavoidably bringing psychological questions into play. In the mass-criticism model, the actors whose intentions are relevant are the elites, the masses, and the target. In the Cultural Revolution specifically, the national communicative elites were Mao Tse-tung and the Maoists (i.e., those who maintained personal contact with the Chairman); the masses were the "revolutionary masses" or Red Guards and Revolutionary Rebels; and the principal target was Liu Shao-ch'i, erstwhile Chairman of the People's Republic and Mao's heir apparent. To simplify somewhat, the masses' intentions may be generally characterized as "catharsis," the elites' intentions may be characterized as "exhortation," and the target's intentions as "expiation." By examining the particular motives for these intentions and showing how the intentions interacted with one another we can comprehend the pragmatic implications of mass criticism.

Catharsis consists of the expression of emotions normally repressed in China, such as resentment, vengeance, self-interest, or ambition for fame or power. Mao has long considered catharsis an integral part of his program for transforming Chinese political culture (without necessarily endorsing the content of the opinions vented), under the assumption that if emotions are pent up they acquire destructive potential, whereas their expression can unleash creative energy. Some of the grievances and demands that found cathartic expression during the Cultural Revolution were generally shared, such as resentment of authority; others were specific to a particular group or quasi-group and tended to reflect the social backgrounds and interests of that group. Those groups most actively engaged in the movement were (in descending order of activism) students, workers, and peasants.

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[15] Harold Garfinkel, "Conditions of Successful Degradation Ceremonies," *American Journal of Sociology,* 61 (March, 1956), 420–424.

Students as a whole manifested an idealistic conception of politics, indignation about its underlying coercive aspect, and a demand for the emancipation of all disprivileged groups. Most Red Guard organizations consisted of students (whereas Revolutionary Rebel organizations usually consisted of workers), and their motives may be characterized with some confidence on the basis of a content analysis of the themes of criticism in their publications. In contrast to official publications, which showed relatively high concern with the erosion of "proletarian dictatorship" (by functional experts, the free market, etc.), Red Guard posters and tabloid newspapers evinced an intense preoccupation with issues of political persecution and repression. This was also reflected in their choice of the dates of confrontations between students and authorities as anniversaries or as faction names, as in the "May 16 group" *[wu i'liu]*, "Black March," the "fifty days," etc. Their anti-authoritarian themes and eagerness to "seize power" presumably reflect the students' sense of status incongruence between their low positions on the Chinese stratification pyramid and their socially sanctioned ambitions to rise to the top of it, together with frustration over the growing obstacles to these ambitions.[16] Red Guard criticisms also displayed greater outrage concerning incidents of apparent irreverence toward Mao Tse-tung or his Thought than did official publications. The emotional cogency of lèse majesté to the Red Guards (whose primary social roles were still those of children in Chinese families) may derive from their incomplete separation of politics from the household: Mao appears as a benign father, Liu Shao-ch'i as a unfilial son, and the young rebel feels the need to deny analogous sentiments even while demonstrating his revolutionary ardor.[17] Finally, the policy areas selected for special emphasis by student polemicists (viz., education and cultural affairs) reflected their scholastic backgrounds.

These themes characterized nearly all student groups, but there were also a number of issue areas in which their interests diverged or even clashed. The two primary social bases for cleavage were class origin and academic achievement. Those with "five red" class origins (i.e., children of workers, poor peasants, revolutionary martyrs, cadres, and soldiers) were generally eager to attack "bourgeois intellectual authorities" (i.e., teachers and functional experts) but had a vested interest in maintaining the existing class structure that discriminated in their favor politically, and recoiled from any determined assault on the Party or on its successor as a pillar of established authority, the People's Liberation Army (PLA). Students from "five black" backgrounds (children of counterrevolutionaries, bad elements, rich peasants, landlords, and bourgeoisie) and children of the "intermediate" classes (e.g., teachers, doctors, urban middle-class professionals), on the other hand, had previously been excluded from participation in school Youth League and Party activities; they took advantage of their enfranchisement to articulate a radical critique of the status quo that threw the entire 17-year history of Chinese Communist rule into question.[18]

Academic achievement tended to cut across the class cleavage, inasmuch as children from unreliable class origins tended to excel scholastically because of their culturally advantaged family backgrounds and desire to compensate for political vulnerability. Thus students from "bad" class backgrounds did not necessarily become radical activists, particularly if their academic achievements had given them bright career prospects and a stake in the status quo; and students with "good" (i.e., "red") class backgrounds did not always join in defense of the status quo, particularly if low academic achievement beclouded their futures. Work-study students (who were both underpaid as part-time workers and undereducated as part-time students) and students who had been "transferred down" *[hsia-fang]* for work in the countryside were conspicuously prominent among the radicals.[19]

A third basis of cleavage among students (and indeed, among all groups) was regionalism. When Red Guards fanned out across the countryside from the urban centers where the movement originated to "spread the sparks of revolution," they typically engaged in more "radical" tactics than they permitted themselves in their home towns, and their condescending attitudes toward "backward" native youth provoked a regional xenophobia that local elites often encouraged for their own self-protection.[20]

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[16] Most manpower studies have noted the tightening professional job market in China. Cf. John Philip Emerson, "Employment in Mainland China,"in *An Economic Profile of Mainland China,* Studies Prepared for the Joint Economic Committee of Congress, Vol. 2 (February, 1967), 458–459; also Leo Orleans, "Communist China's Education: Politics, Problems, and Prospects," *ibid.,* p. 515.

[17] Michael Walzer, in *The Revolution of the Saints* (New York: Atheneum, 1973), pp. 13–14, considers separation of politics from the household one of the preconditions for the development of modern radicalism. It is true that the Red Guards' unconditional loyalty to Mao left them in an untenable position when Mao withdrew his support.

[18] Cf. Hong Yung Lee, "The Political Mobilization of the Red Guards and Revolutionary Rebels in the Cultural Revolution (Ph.D. dissertation, University of Chicago, 1973).

[19] *Ibid.*

In their late response to mobilization, initial support of the local authorities, and anti-intellectual orientation, Chinese *workers* seemed to fit the profile Lipset called "working-class authoritarianism": leftist on economic issues, but rigid and intolerant on political/cultural issues.[21] This posture disposed the workers to resist initial Red Guard incursions into their factories. As the "power seizure" movement of January 1967 gathered momentum, the workers joined in, grasping their chance to strike for higher wages ("the students wanted power, but the workers wanted money," a former Red Guard noted acerbically).[22] As in the case of students, however, workers were internally divided by conflicts of interest. With their participation in the movement came a tendency to splinter into factions, usually based on trade or income (a tendency condemned in the official press as a "guild mentality"). The most conservative "guilds," composed of senior, skilled, and unionized workers, lent active support to Party or PLA forces in their conflicts with radical student units. The nonunionized contract, piece, rotation, or apprentice workers received low wages and were subject to layoff on a last-hired, first-fired basis; they understandably tended to adopt more radical postures, demanding major changes in the industrial wage scale and administrative hierarchy. Workers from small handicraft industries also seemed more inclined to join radical factions than did workers in large state factories,[23] which can probably also be explained by relative deprivation theory.

Peasants did not generally become actively engaged in the movement. As victims of the disastrous aftermath of the Great Leap Forward and beneficiaries of "revisionist" recovery policies of the early 1960s that gave more latitude to private plots, domestic industries, and the rural market, the peasants seemed generally opposed to radical social programs. So they opposed the Red Guards who espoused such programs, in some cases even threatening to "encircle the cities from the countryside" and quell the radicals. On the outskirts of some large industrial cities, on the other hand, *jacquerie*-like movements occurred. These may perhaps be ascribed to the salience of urban-rural income disparities in the suburbs, and to the rather large concentrations of rusticated urbanites in these locations, many of whom were quite ready to march back into the cities and demand redress.[24]

Mao long believed that his hortatory intentions were promoted rather than frustrated by permitting the masses to achieve some measure of cathartic release through mass criticism. As he put it in 1957, "What is correct invariably develops in the course of struggle with what is wrong. The true, the good, and the beautiful always exist in comparison with the false, the evil, and the ugly, and grow in struggle with the latter."[25] In a chat with Andre Malraux about the Communist victory over the Kuomintang (KMT), he commented: "What was it that won over most villages to us? The expositions of bitterness."[26]

The Maoists realized their hortatory intentions through mass criticism by blaming the target for previous failure to achieve the norms, thereby encouraging the masses to believe that these norms were after all realistic objectives that could now be achieved with more strenuous efforts. To the extent that the criticisms functioned effectively to fulfill hortatory objectives, they were logically impelled to accuse the target of all those moral lapses that had hindered previous realization of those objectives: thus Liu Shao-ch'i was lambasted for proposing a complete "bourgeois reactionary line" designed to lead China down the "capitalist road." By criticizing the Liuist program, the masses would forswear its blandishments and reflexively affirm their commitment to the "proletarian revolutionary line of Chairman Mao."

The critique of Liuist "revisionism" was comprehensive, embracing every policy area from foreign policy to agricultural mechanization, and it remained essentially consistent throughout the campaign. But as the elites lost control of the movement they began to improvise criticisms to counter various undesirable tendencies that arose spontaneously among the masses. For example, the criticisms of Liu Shao-ch'i's alleged "economism" coincided with a wave of strikes by workers for higher wages in January-February 1967. The accusations that Liu had encouraged a "mountain-stronghold-mentality," or "theory of mass spontaneity" (all quite contrary to earlier charges that he enforced excessive "centralism") first appeared in the late summer and fall of 1967, to coincide with a Maoist drive against Red Guard factionalism. The criticisms of his Party-building policies appeared in tandem with the effort to rebuild the Party committees in the fall of 1968. All of these criticisms seemed to conform reflexively to the immediate hortatory objectives of the Maoists rather than to Liu's actual record.[27]

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[20] Ken Ling, *The Revenge of Heaven: Journal of a Young Chinese* (New York: G. P. Putnam's Sons, 1972), pp. 70–71.

[21] Seymour Martin Lipset, *Political Man* (New York: Doubleday, 1963), pp. 87–126.

[22] Ling, *Revenge*, p. 243.

[23] Lee, "Mobilization."

[24] Alan P. L. Liu, *Political Culture and Group Conflict in Communist China* (Santa Barbara, Calif.: Clio Press, 1976), pp. 153–156.

[25] *Jen-min jih-pao* [People's Daily] editorial, June 8, 1966 (hereafter *JMJP*); also quoted in *Hung-ch'i* [Red Flag], no. 9, 1966.

[26] André Malraux, *Anti-Memoirs*, trans. Terence Kilmartin (New York: Holt, Rinehart and Winston, 1968), pp. 369–70.

The hortatory intentions of the elites could be most effectively promoted by symbolically enhancing the contrast between the target and the norm. The logical relationship between target and norm was thus polarized from one of contradiction (white versus non-white) to one of contrariety (white versus black). This opposition was then dramatized by a set of bipolar symbols derived partly from Chinese polemical tradition and partly from Marxist doctrine, to be analyzed in greater depth in the syntactic section. The image of the target was also simplified, depriving Liu of any chronological variability and accusing him of the same criminal tendencies throughout his career, occasionally invisible only because of his tactical shrewdness. The evident intention was to enhance commitment to the norms by avowing their absolute and eternal character, in contrast to the absolute and eternal wickedness of the target. The norms provided the guidelines for the formulation of criticisms, and the criticisms were then projected back like a spotlight into the target's life: any incidents in Liu's biography found to be analogous to or even tenuously associated with the errors of which he was initially accused were highlighted in polemical articles.[28]

The intention of the target is to atone for his sins and reintegrate himself with the community from which he has been ostracized. If he succeeds in this expiation he can contribute to communal solidarity and promote a similar moral transformation among the masses, as Liu Shao-ch'i himself pointed out earlier:

> Experience proves that whenever a comrade in a responsible position seriously practices sincere and necessary self-criticism before the Party membership and the masses, . . . their internal solidarity will develop, their work will improve and their defects will be overcome, while the prestige of the responsible comrade will increase instead of being undermined. There is a good deal of evidence, both in the Party and among the masses, to prove this. On the other hand, where a responsible comrade lacks the spirit of self-criticism, refuses or fears to reveal his own defects or mistakes, or tries to cover them up; when he expresses no gratitude for criticism and instead of being pleased to be told of his faults blushes to the ears and makes acrimonious retorts or looks for a chance to revenge himself on his critics, the result is just the opposite.[29]

Liu delivered at least three self-criticisms in the course of the Cultural Revolution, but each was derisively rejected by Maoist elites as "superficial and insincere." It is difficult to devise any objective measure of "sincerity," but my own impression is that Liu's self-criticisms, though somewhat more defensive before a hostile Red Guard audience than before a Central Work Conference, were sincere and indeed searchingly honest. If this impression is correct, why were Liu's self-criticisms rejected, frustrating one of the central objectives of the criticism movement?

The reason seems to be that Liu's intention to expiate for his "crimes" conflicted with the masses' desires for catharsis and the elites' desires for exhortation. If the target's self-criticism were accepted, this would remove the moral tension between target and norm that propelled the criticism movement: a penitent Liu could no longer serve as an object of catharsis, and his "negative example" could no longer be cited to inspire more zealous adherence to norms. As the Cultural Revolution became a "going concern" its participants also acquired vested interests in its continuation, particularly those symbol specialists (such as K'uai Ta-fu or Yao Wen-yüan) who showed talent for expressing the repressed emotions of the masses and the norms advocated by the elites. And it was in the interest of those who wished the Cultural Revolution to continue for one reason or another to exaggerate the incompatibility between target and norm and to build up the credibility of the *threat* the target posed to the norms. Thus the same considerations that militated against Liu's forgiveness also prevented his purge: although he was demoted from second to eighth place in the Party hierarchy at the 11th Central Committee Plenum in August 1966 and never wielded effective power after that time (according to

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[27] Lowell Dittmer, *Liu Shao-ch'i and the Chinese Cultural Revolution: The Politics of Mass Criticism* (Berkeley: University of California Press, 1974), pp. 297–315.

[28] *Ibid.*

[29] Liu Shao-ch'i, "On the Party" (May, 1945), *Collected Works of Liu Shao-ch'i* (Hong Kong: Union Research Institute, 1969), II, 65.

Japanese reporters, he was placed under house arrest in December 1966), he nominally retained his Party and government positions until the Cultural Revolution was brought to a close in the fall of 1968. When it was reported at one point (July 1967) that Liu was a "dead tiger," the radicals stoutly denied this—they could not so easily dispense with the movement's *raison d'être.* As K'ang Sheng warned in a different but analogous context: "If you strike him to death, you will lose a living target. . . . After "traveling by airplane" [a form of struggle in which the accused is obliged to respond to questions while crouching for long periods with his arms extended behind him], Lu Ting-yi is not required to make further confession."[30]

Whereas in a small-group criticism and self-criticism session the personal intentions of the target might conceivably receive greater solicitude, since after all he would be a visible and articulate participant familiar to everyone in the group, when criticism was projected to a nationwide level none of the checks on aggression characteristic of face-to-face relationships were operative; the target's existence as a person was subordinated to his functional significance as a symbol. And Liu Shao-ch'i—erect, white-haired, stern-looking, the ideal father figure—proved such an effective symbol that his power to exert an evil influence survived not only his purge but his physical death. Although his meaning became somewhat obscured in the early 1970s as a result of his utilization in the polemics against Ch'en Po-ta and the radical "May 16 Group" (who were paradoxically referred to as "people like Liu Shao-ch'i"), Liu seems to have remained a touchstone for evil in the Chinese political system.

Syntactics. Syntactics concerns the organization of various linguistic symbols within the message, while temporarily bracketing the question of their relationship to empirical referents—the *form,* rather than the content, of the rhetoric.[31] Whereas the conscious demands and grievances of the critic become manifest in the empirical content of the criticism, the syntactic form of the criticism appears to embody certain elemental but hitherto obscurely sensed wishes and beliefs latent in the political culture. In other words, syntax bears a particularly close relationship to the emotive and evaluative *connotations* of the symbolism. In the following analysis I shall focus on the imagery and metaphor of the polemics, which literary critics have shown to be most accessible to the expression and conveyance of feeling. My analysis of this imagery and metaphor will reveal a series of sharply contrasting antimonies, or "binary oppositions":[32] (1) light/darkness, (2) revealed/concealed, (3) pure/filthy, and (4) active/passive.

(1) The metaphor of light was pervasive, symbolized by the color red. The orthodox Communist "red/white" color symbolism (as in "white terror," or "Red Army") was even changed into "red/black" in order to conform with the light metaphor. Red denoted ideological legitimacy: "red hearts" *[hung-hsin]* stood for militance and loyalty; a "red lantern" *[hung-teng]* was a source of doctrinal illumination; "red flowers" *[hung-hua]* referred to the Red Guards and other objects of praise.[33] The young rebels even demanded that the "go" signals in traffic lights be changed from green to red! The following passage illustrates the frequency of this color's appearance: "On that day, countless red flags waved in the breeze at T'ienanmen Square. Tens of thousands of Red Guards wearing red armbands and carrying red-colored *Quotations of Chairman Mao Tse-tung* sang with gusto, "Sailing the Seas Depends on the Helmsman." The whole square became a surging ocean of red."[34]

Contrary to red was black (which in traditional Chinese color symbolism had clandestine and sinister connotations, whereas red connoted luck and prosperity). Thus "bourgeois authorities" were said to use "black language" *[hei-hua],* to write "black books" *[hei-shu],* post a "black flag" *[hei-ch'i],* and were characterized as a "black gang" *[hei-pang],* "black line" *[hei-hsien]* or "black inn" *[hei-tien].*[35] Anthony Grey, a British journalist held prisoner several months by Red Guards, was struck by the pervasiveness of this color symbolism upon witnessing his redecorated apartment:

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[30] "Speech to Red Guards," *Hsin Pei-ta* [New Peking University], January 28, 1967.

[31] On the distinction between form and content, see John H. Weakland, "Family Imagery in a Passage by Mao Tse-tung," *World Politics,* 10 (January, 1958), 387–408.

[32] This term betrays the influence of Claude Levi-Strauss. Cf. *Structural Anthropology* (New York: Basic Books, 1963), chaps. 2, 10, 11; also *The Savage Mind* (Chicago: University of Chicago Press, 1966), p. 80.

[33] H. C. Chuang, *The Great Proletarian Cultural Revolution: A Terminological Study* (Berkeley: Center for Chinese Studies, August, 1967), I rely heavily on Chuang in this section.

[34] *JMJP,* September 1, 1966, as cited in Chuang, *Great Proletarian.*

[35] *JMJP,* July 26, p. 4; *Kuang-ming kih-pao* [Enlightenment Daily] (hereafter *KMJP*), July 17, 1966, p. 2; *Hung-ch'i,* no. 9 (1966), p. 35 (all as cited in Chuang).

> Black paint ran down every wall. Every square foot had been daubed with slogans in Chinese and English. . . . Even the sheets of my bed had been daubed with Chinese characters saying *"Ta-tao Gerlai!"*–"Down with Grey!" . . . The bathroom mirror was covered with slogans and there was one other refinement. The bristles of my toothbrush had been carefully painted black with slogan paint, . . . The inside of the bath had been painted black too, putting it out of action.[36]

The primary symbol of light was the sun, which was identified with Chairman Mao or his Thought. In an inadvertent reversion to Chinese geomancy *[feng-shui]*, the exhibition halls of the life of Mao that were constructed throughout the country were invariably built to face east, the source of light, just as emperor's palaces had earlier been built to face south, the source of warmth.[37] Like the sun, Mao's Thought radiated life: "Sun, rain, and dew nourish the pine trees, Mao Tse-tung's Thought nourishes *[pu-yü]* heroes." Thus it was thought advisable to incorporate it into the body: "Mao Tse-tung's Thought is the red, red sun in our hearts."[38]

A secondary symbol of light was fire: "They try everything from struggle to encirclement for attack in their vain attempt to extinguish the flames of the Great Proletarian Cultural Revolution, which are bound to become a prairie fire." "They spread the sparks of revolutionary rebelling." "They light the flames of criticism."[39] Again, only the wicked were assumed to be flammable; fire had an annealing effect on the righteous, "steeling . . . and maturing them in the furnaces of the great Cultural Revolutionary . . . crucible." Yao Wen-yüan was said to have lit the flames of the Cultural Revolution in his November 1965 broadside against Wu Han, and other authorities were warned that they must "not only mobilize the masses and start a fire to burn ourselves, but also take the initiative to appear and carry out self-revolution." Otherwise, "One day, the blazing flames of revolution will burn your monster and devil group all to death." By contrast with the metaphor of fire, the enemy threatens to become a "free-flowing inundation."[40]

The location of the enemies with reference to the light/dark dimension is, of course, in outer darkness. Yet the most sinister danger is posed by those enemies who seek to emigrate from the world of darkness to the world of light under false pretenses: "The enemy in daylight look like men, in darkness devils. To your face, they speak human language, behind your back the language of devils. They are wolves clad in skins of sheep, man-eating smiling tigers. . . . The enemies without guns are more hidden, cunning, sinister, and vicious than the enemies with guns."[41]

(2) The second dimension is that of revealed/concealed, public/private. The enemies are "tigers," who must be "lured from their lair"; "snakes" who "crawl underground," hide in "holes," from where they must be "dragged out"; they "shield" themselves with "masks," or even "fig leaves," which must be "ripped off"; they are "bullets" with "sugar coats," "wolves clad in skins of sheep," etc.[42] "We have torn aside your filthy curtain of counter-revolution and caught you red-handed. We shall strip you of your disguises and expose you in all your ugliness."[43] The archetypal symbol for this imagery is that of an underworld, or Hades, which the Red Guards were also determined to assault: "Overthrow the kind of Hell and free all the little devils!"

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[36] Anthony Grey, *Hostage in Peking* (London: Michael Joseph, 1970), pp. 104–105.

[37] Adrian Hsia, *Die Chinesische Kulturrevolution* (Neuwied, Germany: Hermann Luchterhand, 1971), p. 265.

[38] *JMJP* editorial, March 18, 1967, trans. in *JPRS*, no. 40525 (April 5, 1967). *JPRS* translations are often poor, but wherever possible I have checked them against the originals.

[39] *Chingkangshan* editorial, no. 5 (December 26, 1966), p. 3; "The Struggle Against the Bourgeois Reactionary Line," *Hung-ch'i* (Peking Aeronautical Institute), no. 3 (December 26, 1966), pp. 3–4; K'uai Ta-fu, "Destroy the Liu-Teng Bourgeois Reactionary Line and Strive for New Victories," *Hung-wei-ping* [Red Guard], no. 15 (December 30, 1966), pp. 2, 4, 17–22. All are Red Guard publications.

[40] Wu Pin, "Struggle Firmly Against Class Enemies," *Chung-kuo ch'ing-nien* [China Youth], no. 13 (July 1, 1966), in *JPRS*, no. 39235 (December 22, 1966), pp. 46-48; Commentator, "Cast Away Three Wrong Ways of Thinking," *T'i-wu chan-pao* [Physical Education Battle News], Shanghai, in *JPRS*, no. 41450, pp. 115–116; "The Flame That Cannot Be Put Out," *Tung-fang-hung pao* [East Is Red News], May 9, 1967, in *JPRS*, no. 42503 (September 7, 1967), pp. 129-135; "Resolutely Smash the Counterattacks of the Bourgeois Reactionary Line," *Hung-wei-ping pao*, no. 15 (December 15, 1966), pp. 3, 84 (cited in that order).

[41] *Chieh-fang chün-pao* [Liberation Army Daily] *(CFCP)* editorial, August 23, 1966.

[42] At times, a castration threat seemed implicit in the threat to expose. In a *CFCP* editorial we read, "If we are mentally lazy . . . and are so afraid of being hurt and embarrassed that we will not take off our pants and have our tails removed, our minds will not be properly reformed." "Regard Chairman Mao's Works as the Highest Directive for All Works Throughout the PLA," trans. in *Survey of the China Mainland Press (SCMP)*, no. 3712 (June 6, 1966), p. 5.

[43] "Tear Aside the Bourgeois Mask of 'Liberty, Equality, and Fraternity,'" *JMJP* editorial, June 4, 1966, in *SCMP*, no. 3714 (June 8, 1966), p. 3.

Through the use of such images, the general impression was fostered that there was a deep cleft between the world of appearance and the world of reality, and that the apparent world contained no reliable indicators of the nature of the real world. This disjunction occasioned a sense of outrage and an ambition to reduce appearances to their underlying realities. In short, there was a general suspicion of the conventional, which predicated a correspondence between revealed/concealed and phony/real. The systematically misleading relationship between appearance and reality was more subtly indicated by the addition of inverted commas or the adjective *"so-wei"* [so-called] to the once illustrious title of the target, as in *"hsüeh-she"* [scholars], *"chuan-chia"* [specialists], or *"so-wei ch'uan-wei"* [so-called authorities].

The quest for exposure was to be undertaken "resolutely, thoroughly, wholly and completely" *[chieh-chüeh ch'e-ti kan-ching ch'uan-pu ti]*, to quote one of Mao's contributions to the language,[44] with the ultimate intention of annihilating the sphere of "bourgeois privacy" and realizing the ancient ideal, "all public, no private" *[ta kung wu szu]*. Thus Red Guards whenever possible launched raids on the files of Party and public security offices in search of "black materials" (i.e., incriminating records or documents). "What's so terrific about secrets?" a participant in the notorious raid on the Foreign Ministry files reportedly asked. "To Hell with them!"[45]

This almost obsessive Red Guard concern with exposure seemed, however, to harbor an underlying ambivalence. For although Red Guards denounced authorities for hypocritically concealing their crimes and displaying only their virtues, when an authority actually made a statement revealing opposition to Mao, however obliquely (as in Teng T'o's satires of the early 1960s),[46] he would be condemned for "shamelessly," "audaciously shouting," "fanatically trumpeting," etc. And if he confessed, his confession was invariably rejected as "fraudulent," an "utterly absurd" fabrication.

(3) Filth, feces in particular, became one of the more popular metaphors for the enemy. He was "wallowing in the mire," a "pile of dogshit" who must be "criticized until he stinks." "Where the broom does not sweep, the dirt does not vanish of itself." The Cultural Revolution was a cleansing agent; whereas water assumed a counterrevolutionary aspect in relation to fire, here it became a revolutionary purgative. "The turbulent stream of the revolutionary mass movement has been washing away the filth left by the old society." "The roaring torrent of the great democratic movement under the command of the Thought of Mao Tse-tung is flowing on with surging waves under the bright sun, washing the whole of the old world." "The Great Proletarian Cultural Revolution, like a mighty red torrent, is sweeping away the old," etc.[47] Again, the test of the true revolutionary is his willingness to submit to this overwhelming experience, under the assumption that authentic revolutionary ardor is waterproof: "If you are a genuine proletarian revolutionary . . . you will surely hail and be inspired by the rise of the hundreds of millions of people, join the masses in making revolution and throw yourselves into the torrent for criticism of the bourgeois revolutionary line."[48] The notion of a "test by water" appears again in a *Liberation Army Daily* editorial: "Only by following Chairman Mao's instructions and putting 'daring' and 'doing' above everything else, and courageously plunging into the practice of war—tempering ourselves in the teeth of storms and learning to swim by swimming—can we acquaint ourselves with the laws of war and master them."[49]

(4) In deliberate defiance of the traditional Chinese attachment to peace and harmony, the rhetoric of the Cultural Revolution stressed violent action. The Red Guards called themselves (or were called by others) "shock troops" *[ch'uang-chiang]* and "small generals" *[hsiao-chiang]*, labeled their tabloids "battle news" *[chan-pao]*, and referred to their factions in military terms such as "brigade," "regiment," or "garrison headquarters" (though lacking the corresponding organization). They described

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[44] *Mao Tse-tung chu-hsu yu-lu* [Quotations of Chairman Mao Tse-tung], pp. 98, 143, as quoted in Chuang, *The Little Red Book and Current Chinese Language* (Berkeley: Center for Chinese Studies, 1968), p. 28.

[45] Ross Terrill, "The 800,000,000, Part II: China and the World," *The Atlantic*, 229 (January, 1972), 39–63, at p. 49.

[46] Trans. in Joachim Glaubitz, *Opposition Gegen Mao: Abendspraeche am Yenshan und andere politische Dokumente* (Olten, Switzerland: Walter Verlag, 1969).

[47] *Hung-wei-ping pao*, no. 15, p. 3; Wang Li et al., "Dictatorship of the Proletariat and the Great Proletarian Cultural Revolution," *Hung-ch'i*, no. 15 (December 13, 1966): "A Proposal by 57 Revolutionary Organizations," *Hsin-hua* [New China News Agency], Peking, January 29, 1967, in *JPRS*, no. 41202 (May 29, 1967), pp. 23–27; "Hold Fast to the Main Orientation in the Struggle," *Hung-ch'i*, no. 12 (September 17, 1966), in *JPRS*, no. 29235 (December 22, 1966), pp. 41–44.

[48] "Lord She's Love of Dragons," *JMJP* editorial, December 21, 1966, in *JPRS*, no. 40525.

[49] "Study Problems of Strategy in China's Revolutionary War," *CFCP* editorial, trans. in *Peking Review*, January 13, 1967, p. 18.

their exploits with cataclysmic metaphors that suggested a desire to feel part of a vast, impersonal destructive force: "With the fury of a hurricane," "With the force to topple mountains and upturn seas," "With the power of thunder and lightning from the heavens, this has enveloped all China and the world."[50] The enemies were on the other hand accused of passive, irenic tendencies: they tried to "extinguish class struggle," sought rapprochement with the USSR or USA [sic!], fostered "inner-Party peace," a "parliamentary road" to socialism that circumvented violent revolution, etc. In public struggle meetings against prominent political figures, these respective roles would be acted out: the target would be forced into an abject, dependent position while the surrounding Red Guard interrogators would assume a questing, aggressive stance.[51]

In denouncing these enemies, Red Guard polemicists advocated consequential ruthlessness, renouncing what they conceived to be the characteristic Chinese tendency to develop pity for an enemy midway in the attack and to spare him, with the result that he would revive to counterattack. The contrast between the old and new attitudes toward violence may be illustrated by comparing the Cultural Revolution shibboleths, "Beat the dog in the water" *[ta liu-shiu-kou]*, and "Once you start beating it, beat it to death" (Lu Hsün), "With power to spare we must pursue the tottering foe" (Mao Tse-tung)[52] with Mencius' dictum that if a child fell into a well it was human nature to pull him out, even if he was the child of one's mortal enemy.[53] Although the vehemence of such expressions was perhaps considered necessary to overcome deeply rooted inhibitions against criticism of authority, once these psychic barriers were breached the distinction between symbolic and physical violence proved impossible to maintain, and by the late summer of 1967 the struggle began to escalate to truly lethal proportions.[54]

Thus the dichotomous imagery of Cultural Revolution polemics portrays two worlds: the apparent world is filled with light, purity, and publicity; but this world is suspected of being unreal. Behind "masks," or hidden in "holes," there is a real world of darkness and filth. This underworld is inhabited by all manner of savage beasts: there are "man-eating" *[ch'ih-jen]* tigers, "noxious vermin" *[hai-jen ch'ung]*, "voracious wolves" *[ch'ai-lang]*, "poisonous snakes" *[tu-she]*, etc. As if these metaphors were inadequate to describe the dangers lurking below, the demonology of popular Buddhism is invoked: there are "bull-ghosts and snake-spirits" *[niu-kuei she-shen*, usually freely translated as "freaks and monsters"], "demons" *[kuei-kuai]*, who masqueraded in "painted skin" *[hua-p'i]*, spoke "ghost language" *[kuei-hua]*, and practiced "black magic" *[yao-feng]*.[55]

What divides these two worlds is a forbidding barrier, variously referred to as a "line of demarcation," "shackles," a "fortress," or "frame" *[k'uang-k'uang]*. This barrier is heavily fortified, and must remain so; those who "deliberately confuse the line of demarcation between . . . revolutionaries and counterrevolutionaries" are severely denounced as "two-faced and three-sworded"—i.e., treacherous.[56] Yet paradoxically, it is the wish of the young rebels to shatter this barrier, an act they describe with verbs of violent penetration such as "smash," "crush," "bombard the fortress," and "break the frames." This "penetration" *[shen-ju]* is said to require courage and to occasion high excitement: "With the tremendous and impetuous force of a raging storm [the rebels] have smashed the shackles imposed on their minds by the exploiting classes for so long."[57]

The motives for the penetration appear mixed. On the one hand, the rebels expressed the desire to "destroy all evil winds," "sweep all demons and freaks away," etc. They also wished, however, to emancipate the repressed. An article entitled, "Don't Be Afraid of Washing Dirty Linen in Public" noted, for instance, that "Fear to discuss our shortcomings and mistakes actually is fear to touch our own souls and dig up the dirty things in our minds."[58] It was felt necessary not only to "dig up dirty things," but thoroughly to assimilate them: "The revolutionary young people must tumble millions of times in the mud of the masses."[59] Emancipation and assimilation of the emancipated were desirable because although this hidden world was a source of danger and pollution, it also harbored an uncanny power, and by unleashing this power the rebels could exploit it to confound their opponents and cleanse the world. The effect was like that of a dam bursting:

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[50] "A Proposal by 57 Revolutionary Organizations," *Hsin-hua*, January 29, 1967; "Behind-the-Scenes Story of the Yielding of Power in the Seven Ministries of Machine Building," *Fei Ming Ti* [Flying Whistling Arrowhead], February 17, 1967, in *JPRS*, no. 41779 (July 11, 1967), pp. 101–105; *JMJP* editorial, June 8, 1966; in *Current Background*, no. 392 (October 21, 1969).

[51] Cf. "Three Trials of Pickpocket Wang Kuang-mei" (Material for purpose of criticism and repudiation), pamphlet compiled by the "South Sea Great Wall" Fighting Detachment of the "Chingkangshan" Corps, Tsinghua University, trans. in *Selected Chinese Mainland Magazines*, no. 848, pp. 2–4.

[52] Cited in Chuang, *Little Red Book*, p. 28.

[53] James Legge, trans. *The Chinese Classics, II: The Works of Mencius*, reprint of 1895 ed. (Hong Kong: Hong Kong University Press, 1960), pp. 201–202.

[54] Cf. John Gittings, "Inside China," *Ramparts*, 10, no. 2 (August, 1971), 10–20; also William Hinton, "Hundred Day War," *Monthly Review*, 24 (July-August, 1972).

[55] Cited in Chuang, *Little Red Book*, p. 18.

[56] *Hung-ch'i* editorial, no. 4 (March 1, 1967), in *JPRS*, no. 41450, pp. 46–53.

[57] "Sweep Away All Freaks and Monsters," *JMJP* editorial, June 1, 1966, in *SCMP* no. 3712 (June 6, 1966), p. 2.

> For the sake of our country never changing color, for the sake of the complete liberation of the proletariat, you [viz., Mao] personally lighted the flames of the Great Proletarian Cultural Revolution.... The billows of the historically unprecedented revolution surge and roll in an irresistible force which sweeps over the old world and which will completely bury imperialism and modern revisionism. The hearts of the revolutionary peoples boil with anger and their spirits are soaring.[60]

To smash the frames was to obliterate the distinction between revealed and concealed and to "drag out" those lurking in darkness into the light. The result was that "ghosts" and "men" intermixed freely without distinguishing earmarks, a situation that was termed "chaos" *[luan]*. During the movement's initial stages (i.e., up to the "February adverse current"), chaos was deliberately fostered, in an apparent attempt to shatter the conventional barriers of shame that supported the authority structure of the status quo. As Mao told the rebels: "Do not be afraid to make trouble. The more trouble you make and the longer you make it the better. Confusion and trouble are always noteworthy. It can clear things up . . . wherever there are absesses or infections we must always blow them up."[61] And the rebels responded with enthusiasm: "We want to wield the massive cudgel, express our spirit, invoke our magic influence and turn the old world upside down, smash things into chaos *[luan-luan ti]*, into smithereens, the more chaos *[luan]* the better!"[62]

By dissolving various conceptual and functional distinctions into "chaos," many realms of life that had previously been ordered by incongruent principles (e.g., medicine, science, agricultural engineering) were exposed to criticism on the basis of Mao's Thought. The resulting expansion of the conflict arena was not sanctioned or foreseen in the 16 points of the August 1966 11th Plenum, but seems rather to have been an upshot of the rhetorical animus against "frames."

The Manichaean syntactic structure we have just examined seems to have served both the hortatory and the cathartic intentions of the mass and elite participants in the criticism movement, while thwarting the expiatory intentions of the target. The imagery served a hortatory function by dramatizing the opposition between norm and criticism, making Mao's Thought look exceedingly good and the "capitalist road" look execrable. The cathartic function was served by permitting the polemical expression of repressed grievances. It is the semantic meaning of these grievances that we wish now to consider.

Semantics. Consider once again the structure of the symbolism: what can it tell us about political life in the People's Republic of China? Above, we have the world of appearance, full of light, purity, publicity, and virtuous action; underground, duplicitously concealed, a world of filth, darkness, shame, and passive dependency. These two worlds are divided by a formidable barrier, which seems to arouse intense ambivalence. This symbol structure may be depicted as shown in Table 1.

I hypothesize that this symbol structure corresponds to three dimensions of experience in Chinese political culture: moral, social, and psychological. The *moral* implications are perhaps most easily apparent: the upper row represents virtue and the lower row represents evil. The barrier dividing the two rows represents social and psychological inhibitions against deviation. China is what anthropologists term a "shame" culture, in which virtue is promoted by assuring universal acceptance of a set of norms and by making behavior maximally public, so that any deviant is instantly confronted by unanimous reproof, just as any act of heroism is greeted by widespread applause. Those human impulses that conflict

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[58] *KMJP*, March 23, 1967, in *JPRS*, no. 41450, pp. 53–54.

[59] "Learn to Swim While Swimming," *JMJP* editorial, August 17, 1966.

[60] *JMJP*, June 7, 1966, cited in Chuang, *Great Proletarian.*

[61] "Chairman Mao's Important Instructions" (n.d.), in *JPRS*, no. 49826, p. 23. For a psychoanalytic perspective on "chaos" see Richard Solomon, "Mao's Effort to Reintegrate the Polity: Problems of Authority and Conflict in the Chinese Social Process," in *Chinese Communist Politics in Action*, ed. A. Doak Barnett (Seattle, Wash.: University of Washington Press, 1968), pp. 271–365.

[62] Red Guards of Tsinghua Middle School, Peking, "Long Live the Revolutionary Rebel Spirit of the Proletariat" (June 24, 1966), quoted in *Hung-ch'i*, no. 11 (August 21, 1966), p. 27.

Table 1. Polemical Symbol Structure

Appearance	Light	Publicity	Purity	Activity
Reality	Darkness	Concealment	Filth	Passivity

with public norms must either be repressed or allowed some form of surreptitious or symbolically transformed expression. In such a political culture, any liberalization of normative controls would allow two distinct "worlds" to become publicly visible where only one had been apparent before, and make the barrier between them subject to challenge. Though liberalization might thus threaten moral havoc by subverting conventional controls on immorality, it may also serve a reformist function, stripping public life of the hypocritical moral veneer that often conceals private corruption.

The *psychological* dimension of the symbol structure seems to correspond to the defense-mechanism of repression, in which the world of light represents the realm of conscious freedom and the world of darkness the repressed. Public communications are of course more pervasively censored in China than in pluralistic systems, not by a secret police network but no less effectively by various social controls, and repression is therefore socially instrumental. Two impulses that had been most stringently repressed (as indeed they are to some degree in most societies) were sex and aggression. The imagery of a confrontation between two opposing (and yet strangely attracted) forces, separated by a taboo barrier, the penetration of which is destructive yet compulsory, dangerous and yet thrilling, seems too patently sexual to ignore.[63] This is the imagery of adolescents in one of the world's most puritanical cultures, suddenly freed from kinship and social bonds and permitted to intermix freely while roaming the country almost at will.[64] Notwithstanding the positive value placed on "struggle," aggression is normally prohibited in China and allowed expression only under highly regimented conditions.[65] Following the withdrawal of Party work teams in late July 1966 as a result of a disagreement among Party leaders, control of aggression seemed for the time being to pass from elites to the masses and to assume an openly anti-authoritarian character.

The *social* dimension of the symbolism corresponds to what Liu calls a "political culture of dualism": the Chinese masses have been persistently taught to cultivate "boundless love" and self-sacrifice for the "people" and "boundless hate" for the "enemies of the people."[66] "Contradictions among the people" may arise, but should be resolved through "principled struggle," which is "nonantagonistic" (i.e., nonviolent); but "contradictions between the people and the enemy" must be fought out in an "antagonistic" or "class struggle," which is violent and unprincipled. As long as these distinctions were applied by a disciplined Party organization they could be construed with some tactical flexibility, permitting the Red Army to treat KMT prisoners of war with extraordinary leniency during the Chinese civil war, for example. But when they became assimilated into the popular political culture, their application lent an implacable rigidity to escalating social antagonisms.

Altogether, then, the polemical symbolism of the Cultural Revolution had at least three semantic dimensions, which may be depicted as follows:

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[63] My interpretation that this is defloration symbolism is independently corroborated by John Weakland, "Chinese Film Images of Invasion and Resistance," *China Quarterly*, 47 (July-September, 1971), 438–471.

[64] Ling's sensational accounts (*Revenge*, pp. 14, 31, 30, 119, 121, 146, 250, 332-333). are of dubious authenticity. But see Chou En-lai's statement in Edgar Snow, *The Long Revolution* (New York: Random House, 1971), p. 45.

[65] A Japanese reporter made these observations on a Peking rally in support of North Vietnam in 1965: The buildings in Peking along Ch'ang-an street are equipped with red flags to be hung and illuminated lights to be turned on within minutes after they receive an order. Each of the paving stones in the T'ienanmen Plaza are numbered, so that students can be given standing orders to form great ideographs and geometrical patterns (e.g., "Fifty students from X Commune stand from A-13 to A-15."). The march routes and dispersion points are all designated in advance (e.g., "When the demonstration is over, the W Commune shall turn at the corner of X, march down Y street and disperse when they reach the buses waiting at point Z."). When certain paving stones are removed and a blue canvas tent is erected, certain parts of the road become public lavatories that can accommodate about 30 people within 10 minutes (the lavatories are directly connected with the sewage system). Since the masses become thirsty from shouting slogans and singing songs, first-aid teams are dispersed throughout the crowd, and stands serving hot water are set up, including a red-colored antiseptic solution to disinfect the cups. *Yomiuri*, February 25, 1965, trans. in *Daily Summary of the Japanese Press*, March 3, 1965, p. 16.

[66] Liu, *Political Culture*, pp. 24–31.

Table 2. Semantic Dimensions of the Polemical Symbolism

	Moral	Psychological	Social
World of Light	Heroism	Freedom	The People
World of Darkness	Evil	Repression	The Enemy

Each of these dimensions contained an unwelcome and hitherto invisible contradiction: unwelcome because it contravened the socialist normative objective of "leveling" social differences, hitherto invisible because it was unwelcome. The polemics of Cultural Revolution called these and many other unwelcome contradictions in Chinese political life to public attention—such as the "three great differences" between mental and manual labor, between the city and the countryside, and between industry and agriculture—and proposed a method of resolving them. These contradictions found their symbolic expression in the barrier dividing the two "worlds." By overcoming one's fears and "smashing" the "frames," the Maoist symbolism suggested, these differences could be surmounted, ushering in the Communist utopia.

This symbolic transmutation of reality, while it involved tremendous oversimplification, nevertheless provided a highly effective inducement for mass mobilization. The dynamics of the process that subsequently unfolded will be analyzed in greater depth in the next section, but a few of the problems inherent in this symbolism may be briefly mentioned. First of all, the three semantic dimensions were not entirely parallel. From a moral point of view, penetration of the taboo barrier would be necessary to eliminate hypocrisy and the vice it conceals, but from a psychological perspective penetration can emancipate feelings and impulses that had been repressed and reestablish contact with forbidden but fascinating aspects of the self. This repressed dimension of experience was obviously the home of some extremely powerful emotions, which motivated the young rebels to their most idealistic (as well as their most foolhardy) acts. From a social/political perspective, the symbolism legitimated revolt against oppressive authority, and yet this also meant unleashing the "ghosts and monsters" to mingle among the "people," an intolerably confusing prospect. This ambiguity of consequence may help to account for the ambivalence about the purpose of "smashing the frames" noted previously.

Second, the specific substantive content of each dimension proved to be vague and uncertain, with no dimension more uncertain than the social. If doctrinal criteria are accepted, the "friend/enemy" distinction should correspond to the distinction between proletariat and bourgeoisie, of course, but these class categories were based on Marx's analysis of late nineteenth-century European capitalist industrial economies, already requiring substantial revision to make sense of a centralized bureaucratic empire with a largely peasant base, let alone the confusion that succeeded the fall of the empire. These complications were compounded by the fact that China had already undergone the socialization of its economic infrastructure in the 1950s, obviating any objective economic basis for sorting the Chinese populace into distinct classes. Inasmuch as the logic of the previous classifications no longer seemed compelling, people began to sort themselves into classes anew based on their own estimates of their relative positions on the stratification hierarchy: Party and government functionaries became conscious of their oppression and rebelled against their committee or bureau chiefs; and even Central Committee and Politboro members divided into subjective bourgeoisie and proletarians. Much of this uncertainty and confusion may be attributed to the utter breakdown of organizational discipline.

Cultural Revolution: The Process

How did the polemical idea system affect strategy and tactics during the Cultural Revolution in comparison to more obviously political considerations? Surely much of the Red Guard strategy may be explained by reference to a "rational-choice model," without necessary recourse to political cultural variables. In the balance of power between Red Guards and authorities, the latter had overwhelming political advantages, which they were however officially forbidden to use to interfere with mobilization or to protect their own interests; under these circumstances, it was tactically effective for the Red Guards to make maximum use of publicity to dramatize their "underdog" position and call attention to clandestine attempts by authorities to stifle them. Since they were powerless, to extort concessions from hostile authorities could only be viewed as a temporary and unreliable stopgap; the only way to ensure a permanent redistribution of values was to "seize power!"

If the ends selected by Red Guards seem intelligible in terms of a rational-choice model, the *means* they chose to attain them were notoriously counterproductive. As cases in point, Red Guard tactics of struggle consistently tended toward factional *polarization* and *anarchy*. Although both of these tendencies are irrational in terms of rebel objectives, I shall show how each follows logically from the symbol structure.

Polarization. In most conflict arenas, the "free market" of numerous competing conflict groups lasted only a few months after Red Guard organizations were officially sanctioned on August 18, 1966, thereafter giving way to tendencies toward attrition of intermediate groups and polarization into "two opposing factional organizations" locked in a conflict spiral.[67] For example, in Peking a conflict between the Geology Institute and Peking University soon engaged all other local Red Guard organizations on one side or the other and escalated to sustained warfare between what became known as the Heaven and Earth factions; in Kwangtung, the struggle became polarized between the East Wind and the Red Flag factions; in Kwangsi, between "April 22nd" and "Alliance Command"; in Yünnan, between "August 23rd" and "Yünnan Alliance"; and in Fukien, between the "Revolutionary Rebels" and "August 29th."[68] Such polarization was inconsistent with the interests of the groups as described in the first section, for pursuit of group interests would logically lead to competition among more than two factions. Polarization also militated against the revolutionary objectives common to all factions, making it impossible to "unite 95 per cent of the people and cadres against 5 per cent of the enemy"; in most arenas the two sides were so evenly matched that neither side could destroy the other, with the result that confrontation devolved into extended siege warfare broken by occasional sorties. Mao could not understand this tendency toward polarization, because he discerned no substantive issues at stake between the two factions: "There is no fundamental clash of interests within the working class," he told representatives of two contending factions. "Why should they be split into two big irreconcilable organizations? I don't understand it; some people are pulling the strings. This is invariably the result of the manipulation by capitalist-roaders."[69]

I would argue that this polarizing tendency derived at least in part from the dichotomous syntactic structure of the rhetoric, which in effect denied the possibility of intermediate positions, placing all terms referring to such positions between inverted commas, indicating their nominal or hypocritical character. Within the conceptual framework that the Red Guards in both factions consistently used to order their arguments (and probably their thoughts), it became impossible to draw subtle distinctions; only a zero-sum choice between "bourgeois" and "proletariat" could be made. In a given arena, the conflict inexorably polarized to fit the participants' two-class model of the situation:

> As the two armies face each other, large posters with such slogans as "Provincial Revolutionary Rebel Joint Committee [PRRJC] is very good!" and "Sentence PRRJC to death!" are put up in the streets all of a sudden, and the whole city is resounding with such slogans as "PRRJC is finished" and "PRRJC is growing up admidst curses!" At this critical juncture every revolutionary comrade, every organization, and the political forces of every faction must clearly indicate his attitude and choose sides.
>
> Should the PRRJC really be "Sentenced to death?"
>
> This is a question that must be answered unequivocally.[70]

Although the vivid antipodal imagery indeed made this an urgent question, it contained no answer, tending rather to sustain each faction's faith in its own righteousness and its opponent's perfidy. The polemical rhetoric provided a set of conceptual "trenches" confronting each other, so to speak. It did not specify who should occupy which positions (this was usually decided on the basis of group interests), nor did it contain any instructions about how peace might be negotiated.

Anarchism. Rebel anarchic tendencies were a result of certain basic incongruities that have developed in post-revolutionary China between the ideologically legitimate criteria of stratification and its objective reality, and the consequent attempt by Red Guards to reclassify people into classes on the basis of categories inherent in the polemical symbolism. This attempt at reclassification more accurately reflected the emerging bases of cleavage in Chinese society, but proved incompatible with the doctrinal underpinnings of Communist authority and ultimately had to be compromised.

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[67] *Current Scene,* 6 (May 15, 1968).

[68] For analyses of the Peking and Kwangtung conflicts see Hong Yung Lee, "The Political Behavior of the Radical Students and Their Social Characteristics in the Cultural Revolution," *China Quarterly,* 63 (September 1975); for a brief summary of Red Guard activities in the other provinces see Victor C. Falkenheim, "The Cultural Revolution in Kwangsi, Yünnan and Fukien," *Asian Survey,* 9 (August, 1969), 580–597.

[69] "Chairman Mao's Later Supreme Instructions During His Inspection Tour," *Cheng-fa hung-ch'i* [Politics and Law Red Flag], Canton, combined issues nos. 3–4 (October 17, 1967).

[70] "Kwangtung Rebel Joint Committee Proclamation," *Kuang-tung chan-pao* [Kwangtung Battle News], February 22, 1967, in *JPRS,* no. 41450, pp. 79–82.

By the mid-1960s, at least two serious cleavages had arisen in Chinese society that the Marxist theory of class stratification could not comprehend, based as it was on economic criteria of differentiation. The first was the "class struggle" between a hereditary bourgeoisie and proletariat, a *status* distinction still legitimated in terms of an economic analysis of pre-revolutionary capitalism.[71] The class labels affixed during land reform had become hereditary, and certain "classes" were systematically discriminated against on the basis of bloodlines.[72] The second was the estrangement between the ruling elite and the masses, a *political* distinction legitimated in terms of the exclusive vanguard role of the Communist party. Mao began mooting the political cleavage as early as 1959, criticizing some of his colleagues for "bureaucratism"; and Liu Shao-ch'i apparently tried in the early 1960s to overcome the heridatary cleavage between status-groups by "extinguishing class struggle" and absorbing the most productive elements of the former bourgeoisie into the proletariat, but neither issue became a matter of extensive public controversy until the Cultural Revolution.

During the Cultural Revolution, a critique based on the implications of the psychological and moral dimensions of the polemical symbolism provided the basis for controversy concerning both unresolved cleavages. The psychological dimension implied that repression is outrageous and repressed impulses must be emancipated. The moral dimension implied that conventional taboos should be breached and clandestine evil exposed and eliminated. Translated into political terms this meant that any form of domination—whether based on economic, status, or political criteria—is illegitimate, and that any victim of repression is justified in rebelling. Any attempt by authorities to preserve their internal lines of communication intact signals covert immorality and warrants exposure. Thus did the symbolism define the enemy on the basis of his power position vis-à-vis the self, and suggest a way to "topple" him by exposing his inner workings to the paralyzing light of public scrutiny.

The Cultural Revolution marked the first challenge to the post-1949 stratification system to meet with some measure of success, thanks largely to cleavages within the elite. Members of the Central Cultural Revolution Group initially encouraged the radical groups in disavowing the principle of class inheritance, forming common cause with them in attacking the Party establishment, which children of the proletariat had a vested interest in protecting. But the issue never really found resolution at the highest levels, apparently because the regime's doctrinal and political ties to the proletarian "estate" were so strong that this commitment could not be forsaken without a basic revision of the Marxist theory of the State. In addition, the theory of class implicit in the symbolism gave rise to excesses in rectifying the elite-mass gap that the Maoists were not prepared to countenance. Whereas for Mao, revolutionary officials could still be differentiated from "capitalist-roaders" on the basis of their unselfishness and convivial affinity for the masses, for the Red Guards, lacking sufficient information for such fine discriminations, the two classes were defined in more consistently structural terms, according to which those in authority were almost unexceptionally suspected of being "bourgeois." This not only led logically to the purge of most officials with any experience at running the country (i.e., the incumbents), but also made it impossible to establish any authority whatever. If one Red Guard group managed to "seize power" it would promptly be assaulted by another Red Guard group, which denounced the former in the same symbolism previously used against the authorities. Mao observed this development with considerable dismay: "The Shanghai People's Council offices submitted a proposal to the Premier of the State Council in which they were asked for the elimination of all chiefs," he observed. "This is extreme anarchy; it is most reactionary. Now they do not wish to refer to anyone as chief of such-and-such; they call them orderlies and attendants. . . . Actually, there always have to be chiefs."[73]

As the implications of the rhetoric became inescapable, the Maoists undertook a series of compromises to blunt its polarizing and anarchic tendencies. I shall confine my attention to those compromises designed to cope with the force of the symbol system, since the various organizational adjustments (e.g., the changing percentages of Red Guards, PLA men, and rehabilitated cadres on Revolutionary Committees) have been adequately treated elsewhere.

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[71] See Richard C. Kraus, "The Evolving Concept of Class in Post-Liberation China" (Ph.D. dissertation, Columbia University, 1974).

[72] See for example Lai Ying, *The Thirty-sixth Way: A Personal Account of Imprisonment and Escape from Red China*, trans. Edward Bahr and Sidney Liu (New York: Doubleday, 1969).

[73] "Chairman Mao's Speech at His Third Meeting with Chang Ch'un-ch'iao and Yao Wen-yüan" (February 1967), in *JPRS*, no. 49826, pp. 44–45.

First, the Maoist leadership decided to focus national attention on a "small handful" of capitalist-roaders as "condensation symbols" who would absorb the brunt of the critique and exempt others from further attack. Thus on February 23, 1967, *Red Flag* stressed that "the overwhelming majority of cadres at all levels" were "good or comparatively good" and that criticism should henceforth be concentrated on "China's Krushchev" (viz., Liu Shao-ch'i).[74] Beginning on April 1, a national criticism campaign was launched against Liu in all official media, with the proclaimed intention of deflecting criticism from secondary targets and mitigating factional conflict. This technique seems to have been temporarily successful in absorbing polemical energies, but as it became less plausible that Liu posed any real threat to Mao, Red Guard units tended to ignore Liu, or to adopt him as a symbol for their local opponents, leading to a paradoxical intensification of factionalism.

Second, a campaign was launched,[75] reaching particular intensity in the fall of 1967 and 1968, to transform the polemical disposition toward the taboo "barrier" from a counterphobic to a more conventionally pious one.[76] Increasingly, verbs of violent penetration were replaced by verbs suggesting passive appreciation of the utility of existing arrangements, such as "learn," "study," and "produce." While counterphobic verbs still appeared, the leadership now sought to turn them against the self. Red Guards were told to "dare to rebel against all the things in their minds which do not conform to the Thought of Mao Tse-tung . . . to let the proletariat seize power in their minds" and "revolutionize the self" *[ko tzu-chi-ti ming,* alternately phrased *tzu-wo ko-ming.]*[77] This form of revolution was compatible with a political quietism that left the incumbent hierarchy intact, a form of revolution that could even be performed by penitent cadres: "The fact that a cadre who has made errors can turn from his former adherence to the bourgeois reactionary line, can fight back fiercely against this line and rise up to make rebellion, shows that he has changed his standpoint," a usually leftist journal editorialized. "This 'going over' is a revolution . . . against the viruses of the bourgeois reactionary line in one's mind."[78]

Using such symbolic techniques, the Maoist leadership gradually edged toward a more moderate position, occasionally backtracking to avoid a rightist reversal of verdicts (which ultimately occurred to a considerable extent anyhow). Those elites who remained true to the more uncompromising vision implicit in the polemical symbolism were purged; those Red Guards who continued to struggle (usually against other Red Guards) to achieve that vision were eventually subdued by Mao's Thought propaganda teams and rusticated.

The purpose of this section on process was to show how the symbols of the Cultural Revolution were used by its participants to inform their actions. I am not adopting the Whorfian argument that language is fate, that man is predestined to realize the implications of his native linguistic categories. It seems more reasonable to assume, however, that language deliberately selected for this purpose can provide general guidelines for social movement in the context of widespread uncertainty. This is particularly so if an artificially coercion-free environment is fostered by the leadership, and if the masses are provided access to a mass media network through which they may disseminate their polemics. Of course the same simplifications that make such language emotionally appealing also give rise to many unintended and paradoxical consequences. Thus we find that whereas Cultural Revolution polemics succeeded in symbolizing and legitimating the expression of the masses' repressed grievances and demands, and in bringing to light many deep moral dilemmas in Chinese political culture that had previously been ignored, it also unleashed strong tendencies toward polarization and anarchy. These tendencies, seemingly irrational and previously unexplained, made it impossible to coordinate an effective solution to the problems that the polemics disclosed.

Conclusion

In conclusion I return to the themes heralded in the title. What contributions did polemical symbolism make to thought reform and cultural revolution in China? The first is an essentially psychological question regarding the lasting impact of the Cultural Revolution on the lives of its participants, whereas the second is concerned with the political cultural implications of the mass criticism movement itself.

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[74] "Revolutionary Cadres Must Be Treated Correctly," *Hung-ch'i* editorial, no. 3 (February 23, 1967).

[75] *Hung-ch'i,* no. 15, 1967; "The Great Historical Tide," *JMJP* editorial, September 22, 1967; Mao Tse-tung, "Instruction Given During Inspection Tour," *JMJP,* September 14, 1967.

[76] Cf. Otto Fenichel, "The Counterphobic Attitude," *Collected Papers* (London: Routledge and Kegan Paul, 1955), pp. 163–174.

[77] *JMJP,* February 8, 1967, in *JPRS,* no. 41147.

[78] *Wen Hui Pao* editorial, February 18, 1967, in *JPRS,* no. 41450.

As a form of collective thought reform, the Cultural Revolution is distinguished by its abandonment of "directive" therapy within a small-group context and other highly organized techniques, and its substitution of "nondirective" therapy operating within a communication network temporarily freed from authoritative constraints. The focus is on the transformation of symbols, under the assumption that the political culture and the individual psyche will change accordingly. These symbols are manipulated to foster the internalization of norms and the simultaneous catharsis of previously repressed grievances against targets of criticism. Through the combination of exhortation and catharsis, "norms and values," on the one hand, become saturated with emotion, while the gross and basic emotions become ennobled through contact with social values."[79] Without exhortation, catharsis would be illegitimate; but without catharsis, exhortation would become preachy and ritualized. In previous movements, catharsis was disciplined by the Party apparatus—targets were preselected, and expression of criticism was restricted to official media and cadre-directed small groups—with resulting tendencies toward ritualization.[80] The sudden relaxation of organizational discipline permitted the cathartic function to assume greater prominence than it had in previous movements, giving the Cultural Revolution its superior spontaneity and vitality.

The purpose of the process is to "learn," but in a moral-emotional rather than an intellectual sense. Catharsis seems to facilitate this type of learning by dissipating emotional resistance to new moral commitments through a scapegoat and by permitting some degree of immediate impulse gratification. According to Worsley, this is characteristic of millenarian movements:

> In order to flout the old values, men have to summon up extra untapped sources of energy and resolution; when they have done the act which creates this energy, they are bound together in mutual guilt and mutual support in opposition to all those who still accept the old beliefs. . . . The role of ritual obscenity in providing an occasion for the statement and the reinforcement of moral norms is well-known from many societies, but here it has a revolutionary content, the statement of a new morality.[81]

In Worsley's view, while millenarian movements may be successful in promoting the internalization of new norms, they are less successful in providing the participant with the authentic insight into the meaning of the process he has undergone. Conflicts and resistances are resolved, and moral lessons are learned, not because of the knowledge that the participants progressively acquire of them, but because participation in the movement makes possible a specific experience deemed to have *intrinsic* value. But this would appear to be too harsh a verdict to render on the Cultural Revolution. The movement touched off an explosion of information concerning virtually every aspect of the Chinese political system, providing the younger generation with enhanced political insight and considerable skill.[82] The experience was given an edge of cynicism by the movement's somewhat repressive sequel, but this is perhaps also a valuable political insight.

The most lasting and important contribution of the polemical symbolism to Chinese political culture may be to enrich and enliven the vocabulary of public controversy. Richard Wilson, in a study of Chinese political symbolism, remarked that there are no symbols "available in the Chinese cultural milieu around which forces of opposition may coalesce and which may serve as foci of loyalty."[83] During the Cultural Revolution potent symbols of opposition were invoked, but Wilson is correct insofar as these symbols did not invite forces of opposition to coalesce around them or serve as "foci of loyalty." They were symbols of evil whose ignominious defeat was foreordained, and everyone was anxious to avoid the roles that they "labeled." Yet just the same, this antipodal symbol structure did contribute three significant aspects of political opposition to the Chinese political culture:

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[79] Victor Turner, "Symbols in Ndembu Ritual," in *Sociological Theory and Philosophical Analysis,* ed. Dorothy Emmet, p. 162; see also Turner's *The Forest of Symbols: Aspects of Ndembu Ritual* (Ithaca, N.Y.: Cornell University Press, 1967), pp. 19–47; and *Dramas, Fields and Metaphors: Symbolic Action in Human Society* (Ithaca, N.Y.: Cornell University Press, 1974), p. 37.

[80] Cf. Martin K. Whyte, *Small Groups and Political Ritual in China* (Berkeley, Calif.: University of California Press, 1974).

[81] Peter Worsley, *The Trumpet Shall Sound: A Study of "Cargo" Cults in Melanesia* (London: MacGibbon and Kee, 1957), pp. 250–251. The writings of Frantz Fanon may also be relevant here.

[82] Andrew Watson, "A Revolution to Touch Men's Souls: The Family, Interpersonal Relations and Daily Life," in *Authority, Participation and Cultural Change in China,* ed. Stuart Schram (Cambridge: Cambridge University Press, 1973), pp. 291–331; also Ross Terrill, *800,000,000: The Real China* (New York: Dell, 1971), pp. 51–61.

[83] Richard W. Wilson, "The Learning of Political Symbols in Chinese Culture," *Journal of Asian and African Studies,* 3 (July-October, 1968), 241–257.

First, it offered a means of resolving an elite impasse and generating support for a systematic "line" of policies, regaining popular legitimation and a sense of momentum. In somewhat the same way that a European system resolves a cabinet crisis by dissolving parliament and calling new elections, a deadlocked Chinese regime may hereafter decide to air disputed decisions before a mass forum and seek a new "mandate."

Second, it heightens expectations at both mass and elite levels that authoritative decisions may be made through conflict, thereby preparing the way for the possible future legitimation of political conflict and political opposition. At the mass level, the people have become aware that systematically articulated policy alternatives are available and that the apparently monolithic character of their leadership probably conceals intense debate about these alternatives. By taking the initiative on an ambiguous policy at the grassroots levels the masses may hope to present their undecided superiors with a successful *fait accompli* and gain elite backing. At the elite level, the polemical symbolism seems to have led to more frequent and intractable factional cleavages, at least if the record of political infighting since the Cultural Revolution is any indication. Thus even though no one wishes to assume the opposition role, the polemical symbolism does seem to have increased the incidence of opposition at both mass and elite levels.

Third, the polemics provide an opportunity for repressed demands and grievances to be openly breached without fear of reprisal. Normally, sanctions are attached to the publication of bad news, seriously compromising the statistical and journalistic reporting media. But the polemical rhetoric legitimates the manifestation of all sorts of unwelcome contradictions in socialist society, under the sole proviso that blame be attributed to the appropriate scapegoat. Not only is injustice brought to light, but the most intransigent bureaucratic impediments to its rectification may be removed by direct mass action. To be sure, like all polemics, the rhetoric of the Cultural Revolution has shown itself to be far more effective in exposing problems than in proposing workable solutions.

Perhaps the most serious defects of the Cultural Revolution as a form of cultural revolution concern the role of the target and the lack of formal institutionalization. To what extent target selection is based on the target's cumulative voting record and to what extent on the constellation of political circumstances at the moment of his selection is still impossible to determine definitively, but in any case it seems unfair to pin the sole responsibility on one man for the myriad faults of the ruling apparatus over a long period of time. Once the target is selected, the intensity and bipolar structure of the polemical symbolism seem to press inexorably toward his political destruction and to preclude self-criticism as an acceptable *dénouement*. This is of significance not only for the target's personal career but for the outcome of the movement as a whole, which, deprived of any symbolic model of normative reintegration through expiation, tends to escalate and to polarize with absolute implacability. The polemical lexicon contained few resources for compromise, and controversy intensified unabatedly until the elites enforced de facto martial law.

Formal institutionalization is anathema to the polemical symbolism, with its animus against frames and barriers, and yet without some form of institutionalization the movement is, to change the metaphor, like a tide that ebbs and flows without leaving any permanent mark. As soon as the tide ebbs the bureaucrats who man the institutions may acclaim its regenerative impact but they may also start building dikes to ensure their survival when the tide flows again. Thus the Cultural Revolution set in motion a reaction that dispersed much of its mass constituency to the countryside, eliminated several of its most prominent elite supporters, and led to the renascence of many old institutions, policies, even personnel, with little lasting institutional change.

Still, the Cultural Revolution lives on as a precedent, and its symbolism has become a part of the Chinese political culture, a public resource to be drawn upon by its people when need and opportunity arise. Mass criticism may also serve political functions for elite symbol specialists, but as long as the revolutionary transformation of Chinese political culture remains a serious objective it must contain an element of catharsis, inviting the "ghosts and monsters" of Chinese society to reemerge from their repressed oblivion in dramatic confrontation with the light of the norms.

What Is Political Obligation?*

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It is sometimes said that the problem of political obligation is the fundamental or central problem of political philosophy. This may or may not be true: it depends on what we take "political obligation" and "political philosophy" to mean. Questions concerning obligation are, of course, important to political philosophers and citizens alike. But beyond this we find neither a consensus on how the problem of political obligation is to be solved nor even an agreement that it can be solved.[1] Indeed, one writer has recently gone so far as to suggest that we can do without the very concept of political obligation.[2]

This proposal is certainly extreme, but such suggestions are to be expected when important problems are clouded by conceptual confusion. I hope in this essay to dispel some of this confusion. Unlike those who would dissolve the problem of political obligation by abandoning the concept, I propose to examine the concept to see if the problem is properly stated. I shall advance two theses: (1) the problem of political obligation is fundamentally misconceived when it is (taken to be) expressed by the familiar question, "Why should I obey the law?"; and (2) the problem of political obligation can be solved in principle, although this solution holds no immediate promise of eliminating our theoretical and practical difficulties.

What I shall offer, then, is less a theory of political obligation than a preface to such a theory. Since "political obligation" is a compound of two distinct concepts, I shall begin by looking at "obligation." This alone should suffice to show that the question "Why should I obey the law?" is an improper statement of the problem of political obligation; the implications of this demonstration may not be evident, however, until we examine "*political* obligation."

The Concept of Obligation

If we are to understand what "obligation" means, we must see how the word is used in ordinary language. A catalogue of uses, though, will not serve the purpose, for the propriety of certain uses is a matter of controversy. In what follows, therefore, I argue for a restricted conception of obligation on the grounds that such a conception preserves important distinctions which wider conceptions of obligation destroy. These distinctions, or conceptual relationships, provide both the foci for my explication and a pair of questions which must be answered by anyone hoping to come to terms with the concept of obligation. The questions are (1) what is the relationship between "obligation" and "ought"? and (2) how is obligation related to coercion?

(1) *"Obligation" and "ought."* There is a tendency, especially noticeable in philosophical discourse, to treat certain uses of "obligation" and "ought" as interchangeable. This is most clearly the case in statements which address moral considerations, where one may find, for example, claims that we have an obligation to contribute to charity, to aid those in distress, and so forth. "Obligation" here supplants "ought" in an attempt to strengthen the imperative—to say that "one ought to be charitable" seems to leave matters open in a way which "one has an obligation to be charitable" does not. For this reason, "obligation" has often been regarded as the principal subject of moral philosophy, much as it has been considered the central concern of political philosophy.

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*I am indebted to Terence Ball and Rolf Sartorius for their encouragement and for their comments on drafts of this essay.

[1] See Margaret Macdonald, "The Language of Political Theory," in *Logic and Language*, ed. A. G. N. Flew (Garden City, N.Y.: Doubleday, 1965); Thomas McPherson, *Political Obligation* (London: Routledge & Kegan Paul, 1967); H. A. Prichard, "Green: Political Obligation," in Prichard's *Moral Obligation*, ed. J. O. Urmson (Oxford: Oxford University Press, 1968); and T. D. Weldon, *The Vocabulary of Politics* (London: Penguin, 1953) for the argument that the problem of political obligation is impossible to solve because there can be no general theory of political obligation. For a defense of the "traditional" position, see Carole Pateman, "Political Obligation and Conceptual Analysis," *Political Studies*, 21 (June, 1973), 199–218.

[2] This is McPherson's judgment. He says (*Political Obligation*, pp. 84–85) that "we may well feel justified in dispensing with the concept of political obligation" for this reason: "it is so general a notion as to do little or no useful work, and if interpreted in terms specific enough to be useful it is . . . no longer the satisfyingly general thing that [theorists] wanted in the first place."

In recent years, however, several philosophers have argued that such usage overextends "obligation" by conflating it with "ought."[3] The arguments vary, but the main objection to the extended use of "obligation" is simply that the two concepts have different functions—this, after all, is why we have two concepts rather than one. As Joel Feinberg points out, one of the tasks of "ought" is "to prescribe or give advice. When the word 'ought' occurs in a sentence which gives advice, we can call it the 'ought of final judgment, all things considered.' "[4] But "obligation," which has functions of its own, is not prescriptive in this sense: "If I tell you what your obligations are, I do not necessarily give you advice of any kind; I simply report that you stand in certain relations to other people, relations of commitment and trust."[5]

An example may help to clarify this distinction. Consider the following propositions:

(a) Jones has an obligation to pay Smith $5.
(b) Jones ought to pay Smith $5.

Let us assume that Jones is not disposed to accept these claims. In the case of (a), Jones may respond in two ways: he may deny the existence of the obligation; or he may admit to the obligation but hold that he ought not discharge it. This second response is legitimate even if one believes that *ceteris paribus* we ought to fulfill our obligations, for the ought, as Feinberg says, is "the 'ought of final judgment, all things considered.' " *Advising* Jones of his obligation does not necessarily involve *advising* him to *do* or forbear from doing anything. This is not true of (b). Jones may respond to (b) by asking, "But ought I *really* give Smith $5?", of course, but this questions the soundness of the advice (on either moral or prudential grounds), not the fact that advice has been given.

The core of the distinction between "obligation" and "ought" is to be found in the grounds for accepting or rejecting obligation and ought claims. The fact that there are different grounds for each concept underscores the importance of resisting the temptation to conflate them. In the case of obligation, two conditions must be met in order to warrant an obligation claim. First, when someone confronted with such a claim demands to know why he is obligated, we point to some "previous committing action" of his.[6] This is what happens, for instance, when we say, "You have an obligation to pay Smith $5 because you promised to do so." To show *why* or *that* someone has an obligation, we show *how* he came to have it. Not all instances of previous committing action are as unproblematical as promises, certainly, but the point remains: one becomes obligated by committing himself to pursue or to desist from pursuing a course of action. This is borne out by the way we speak about our obligations: we "undertake," "assume," "acquire," and "incur" them, on the one hand, and "meet," "fulfill," and "discharge" them, on the other.

The second condition which obligation claims must meet has to do with a second party. "Obligation" is a relational concept, and the party under the obligation is always obligated to someone or some group. Consequently, whenever we charge someone with having an obligation, we must be able to answer the question, "An obligation *to whom?*" This is clearly true of such obligating devices as contracts, promises, marriage vows, etc. One who has an obligation *owes* something or is *indebted* to another party. We justify obligation claims, then, by demonstrating that (1) x has committed himself to do or forbear from doing something and that (2) he has committed himself to y.

Ought claims, however, may be warranted without an appeal to either of these features of obligations. This is true whether the claim is moral—"You ought to be honest"—or prudential—"You ought to eat your spinach." In neither of these cases do we require either a previous committing action—"What have I done that makes you think that I ought to be honest (or eat my spinach)?"—or a second party—"With whom ought I be honest? To whom do I owe it to eat my spinach?" Instead, ought claims are justified by an appeal to principles or desires or consequences. I ought to be honest because it is simply right to tell the truth or because better consequences follow from honesty than from dishonesty? I ought to eat my spinach because it is good for me, because it is in my interest to do so. Here again the testimony of language indicates the distinction between "obligation" and "ought": We are obligated to do *a* because we "incurred," "undertook," etc., the obligation, but we ought to do (or refrain from doing) *b* because it is "right" (or "wrong") or "good" (or "bad") to do so. To justify an obligation claim we point to a *commitment;* to justify an ought claim we refer to the *content* of the claim.[7]

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[3] See H. L. A. Hart, "Are There any Natural Rights?", *Philosophical Review*, 64 (April, 1955), and "Legal and Moral Obligation," in *Essays in Moral Philosophy*, ed. A. I. Melden (Seattle: University of Washington Press, 1958); also Joel Feinberg, "Supererogation and Rules," *Ethics*, 71 (July, 1961), 276–288; David Gauthier, *Practical Reasoning* (Oxford: Oxford University Press, 1963), chap. 12; E. J. Lemmon, "Moral Dilemmas," *Philosophical Review*, 71 (April, 1962), 139-158; and Rolf Sartorius, "Utilitarianism and Obligation," *Journal of Philosophy*, 66 (February, 1969), 67–81. An extended use of "obligation" is defended by R. B. Brandt, "The Concepts of Obligation and Duty," *Mind*, 73 (1964), 374–393, and Kurt Baier, "Moral Obligation," *American Philosophical Quarterly*, 3 (July, 1966), 210-226.

[4] Feinberg, "Supererogation and Rules," p. 278.

[5] *Ibid.*, p. 278. This does not mean that "obligation" is in no sense prescriptive; it is consistent with the belief that *ceteris paribus* one ought to fulfill one's obligations. For an analysis of "political obligation" which regards obligation as a "conduct-guiding" concept, see Richard Flathman, *Political Obligation* (New York: Atheneum, 1972), esp. pp. 29–30 and 34.

[6] The phrase is E. J. Lemmon's in "Moral Dilemmas," p. 141.

This position on the relationship of "obligation" and "ought," I should note, is not uncontested. R. B. Brandt, for one, has argued that "obligation" may be used correctly even where there has been neither a previous committing action nor a second party to whom the obligation is owed.[8] Brandt distinguishes a "paradigm" from an "extended" use of "obligation." The "paradigm" use—when "there is no better word for the occasion and there are no better occasions for the word"[9]—corresponds with the account of obligation I have set out. The "extended" use, which involves what I have called the conflation of "obligation" and "ought," is exemplified in such statements as, " 'Jane Addams thought that people are *under an obligation* to affirm their own vision [sic] of truth' " and " 'students are *morally obligated* to report infractions of college rules which they observe'."[10] Since the "extended" use of "obligation" is intelligible, Brandt reasons, the question is, "What must we strike from the paradigm use of 'obligation' in order to arrive at elements common to correct uses?"[11] Brandt's answer is that the two features which I have maintained are essential to "obligation" must be eliminated: "First, we must strike out the notion that obligation is an obligation *to* someone. . . . Second, we must strike out the idea that the bond derives from some prior act or event, such as an agreement or the acceptance of a benefaction."[12]

Brandt's reasons for "striking out" these features are remarkably weak. He dismisses the requirement that obligation is an obligation *to someone* by producing the following examples where "it seems hardly sensible to attempt to identify any individual to whom obligation is owed":

> Citizens have an obligation to observe the laws of their country;
>
> Citizens in a democratic country have an obligation to vote;
>
> Mentally gifted people are under an obligation to develop their capacities.[13]

Similarly, his justification for denying the need for a previous committing action is that "the obligations to give help to one in dire need or to make some contribution to charitable enterprises are sufficient reason for elimination of this restriction."[14] But these examples are not enough to establish Brandt's position. No one disputes that these are intelligible and not altogether uncommon uses of "obligation"; the question is whether these are appropriate uses of the word. To use "obligation" when "ought" is called for is inappropriate precisely because it leads to confusion in two areas: confusion about what is being claimed, on one hand, and about what needs to be done to warrant that claim, on the other. Brandt himself acknowledges this—but fails to recognize its significance—when he says that the "extended" use of "obligation" is "harmful only in blurring our sense of how language is ordinarily used."[15]

"Obligation" and "ought" are certainly closely related concepts; but the relationship will become insignificant if their separate identities are not preserved. The fact that I am under an obligation to do *a* is a reason for saying that I ought to do *a,* but the converse is not true. The "ought of final judgment" may override an obligation, but again the converse does not hold: there is no such thing as an "obligation of final judgment." When we ask, "What am I obligated to do?" we are not also asking, "What is it that I ought to do?" The two concepts have different functions, and the "extended" use of "obligation" only confuses what is already complex.

(2) *Obligation and coercion.* In his essay on "Legal and Moral Obligation," H. L. A. Hart

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[7] This distinction is drawn from Hart, "Legal and Moral Obligation," p. 100 ff. "Promises," Hart says, "have pre-eminently the feature I have called independence of content: the obligation springs not from the nature of the promised action but from the use of the procedure by the appropriate person in the appropriate circumstances" (p. 102).

[8] "The Concepts of Obligation and Duty."

[9] *Ibid.,* p. 385.

[10] *Ibid.,* p. 376 (emphasis in the original).

[11] *Ibid.,* p. 390.

[12] *Ibid.,* p. 390 (emphasis in the original).

[13] *Ibid.,* p. 379. It should be noted that even in these cases we can still point to someone or some group to whom the obligation may be owed—e.g., "Citizens are obligated to their fellow citizens to pay taxes."

[14] *Ibid.,* p. 390.

[15] *Ibid.,* p. 391.

identifies the following as features of obligation: "(1) dependence on the actual practice of a social group, (2) possible independence of content, and (3) coercion."[16] The first two features have to do with previous committing action. The second, as I have already noted, deals with the commitment/content distinction, while the first is concerned with specifying what is to count as a committing action. The third feature, however, introduces another kind of problem, that of the connection between obligation and coercion.

Hart points out that whenever we fail to meet our obligations we are subject to certain pressures or sanctions. If I fail to pay my income tax, for example, I may be fined or jailed; if I break a promise, I expose myself to a variety of moral pressures—censure, ostracism, etc. Not all cases of failed obligation result in such pressure being brought to bear, of course, but the invocation of sanctions is always possible in these circumstances.

This connection between obligation and coercion has been the source of a good deal of confusion in legal and political philosophy. "If we have an obligation to do something," as Hart says, "there is some sense in which we are bound to do it, and where we are bound there is some sense in which we are compelled to do it."[17] The question is, how, or in what sense, are we bound? Some writers—notably Hobbes and Austin (as Hart demonstrates)[18]—have failed to distinguish between different senses of being bound and have argued, as a result, that obligation originates in coercion. They have taken the relationship of obligation to coercion, in other words, to mean that we are obligated because we are bound and not that we are bound because we are obligated.

The extremity of this position is apparent when we consider that from this perspective, keeping a promise and paying ransom to a kidnapper are equally obligatory actions. What happens here, as Hart has shown, is that the notions of "having an obligation" and "being obliged" are conflated.[19] If I am accosted by a gunman, to borrow Hart's example, I may *be obliged* to hand my money over to him, but I surely do not *have an obligation* to do so. The difference between the two concepts becomes even clearer when we look at another situation. Smith, my creditor, may threaten to take legal action against me if I do not pay him promptly. In this case I may *be obliged* to pay Smith, but I am only *obliged* because I *have an obligation* to pay him: the sanction can be invoked only because the obligation exists to validate it. But when obligation is reduced to coercion, the distinction between gunmen and creditors vanishes.

The connection between obligation and coercion can best be understood by observing when the possibility of coercion can and does arise in disputes concerning our obligations. We do not attempt to coerce those who fulfill their obligations to us—there is no reason to do so. The prospect of coercion is only invoked when someone fails or gives evidence of failing to discharge, or even recognize, an obligation; and in many of these instances there is no actual coercion because excuses are offered and accepted. Coercion is justified,[20] however, when the obligated party simply refuses to acknowledge his obligation, as the following dialogue illustrates:

> Jones: Why should I give you $5?
> Smith: Why? Because you borrowed $5 from me and promised to pay me back.
> Jones: So? Why should I keep my promise?
> Smith: Because a promise is a promise! And if you don't give me my money I'll tell everybody that you can't be trusted!

Coercion, as related to obligation, is a response to one whose abuse threatens the practices through which we undertake obligations and conduct our lives. The existence of a promise is, *ceteris paribus*, sufficient reason for keeping the promise. One who does not acknowledge this and who seeks to use the institution of promising to his own advantage makes himself liable to coercion. Coercion acts to guarantee, so to speak, that those who would voluntarily fulfill their obligations are not sacrificed to those who would not.[21] Obligation is secured by coercion, but it is not rooted in it.

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[16] Hart, "Legal and Moral Obligation," p. 100.

[17] *Ibid.*, p. 95.

[18] *Ibid.*, pp. 95–99. See also Hart's extended critique of the "coercive theory of law" in *The Concept of Law* (Oxford: Oxford University Press, 1961), esp. chaps. 2 and 4 and pp.79–89. I am "much obliged" to Hart for what follows.

[19] *Ibid.*, p. 96, and *The Concept of Law*, pp.79–89; also Gauthier, *Practical Reasoning*, pp. 174-176.

[20] I am not suggesting that all forms and all degrees of coercion are always justified. The sanction must fit the abuse, as it were.

[21] This borrows from Hart, who (in a slightly different context) says: " 'Sanctions' are . . . required not as the normal motive for obedience, but as a guarantee that those who would voluntarily obey shall not be sacrificed to those who would not. To obey, without this, would be to risk going to the wall. Given this standing danger, what reason demands is *voluntary* co-operation in a *coercive* system." *The Concept of Law*, p. 193 (emphasis in original).

The concept of obligation, then, involves these elements: a commitment to someone (or some group) to do (or not do) something which one may justifiably be coerced into doing or punished for failing to do. "Obligation" is not coextensive with "ought"; and obligations are supported by, but not derived from, the threat of coercion. With this understanding of "obligation" it becomes evident that the question which supposedly expresses the problem of political obligation, "Why should I obey the law?", is not at all a proper statement of the problem. This is because the question is put in terms of "ought" rather than "obligation." In turn, this means that "Why should I obey the law?" fails to come to grips with obligation in two different ways. First, it may be that I am *obligated* to obey the law even though, all things considered, I *ought* to disobey it. The fact that I have in some way committed myself to obedience may not, in a particular case, constitute a (morally) sufficient reason to obey. Second, "Why should I obey the law?" may be answered with considerations of expediency or prudence. Thus some have argued that we ought to obey the law because we will benefit by doing so or we will suffer if we do not. This is an entirely reasonable answer; but this is also the reason for rejecting "Why should I obey the law?": the question can be answered in terms which fail to justify obligation claims.

What we need to do is to recognize two distinct kinds of questions: questions of political obligation, on the one hand—"Am I obligated to obey the law?"—and more general, and perhaps more important, questions of political obedience, on the other—"Why should I obey the law?"[22]

The Problem of Political Obligation

My second thesis is that the problem of political obligation can be solved—at least in principle. This has been an article of faith with many political philosophers, but in the years since World War II it has become the subject of dispute. Several commentators, in fact, have dismissed the problem of political obligation as a pseudo-problem which cannot be solved and need not be posed in the first place. Since those who hold this view explicitly deny my second thesis, it seems appropriate to turn to an examination of their position.

According to those who would dissolve the problem of political obligation, political obligation is best understood as what has come to be called an institutional obligation, i.e., an obligation which is presupposed by the rules of an institution and incurred by those who participate in its practices. Anyone, then, who participates in the practices of a political society incurs an obligation to obey the laws of that society, just as anyone who joins in a game of basketball undertakes an obligation to obey the rules of the game.

The chief significance of this institutional model for the present discussion is that it tends to moot the question of obedience. Hans Kelsen, for instance, deals with "Why should I obey the law?" in this fashion:

> We ought to obey the decisions of a judge or administrator, ultimately, because we ought to obey the constitution. If we ask why we ought to obey the norms of the existing constitution, we may be referred to an older constitution that has been replaced in a constitutional way by the existing constitution; and in this way we arrive finally at the historically first constitution. To the question why we ought to obey its provisions a science of positive law can only answer: the norm that we ought to obey the provisions of the historically first constitution *must be presupposed as a hypothesis if* the coercive order established on its basis and actually obeyed and applied by those whose behavior it regulates is to be considered as a valid order binding upon these individuals; *if* the relations among these individuals are to be interpreted as legal duties, legal rights, and legal responsibilities, and not as mere power relations; and *if* it shall be possible to distinguish between what is legally right and legally wrong and especially between legitimate and illegitimate use of force. This is the basic norm of a positive legal order, the ultimate reason for its validity. . . .[23]

Kelsen's argument amounts to something like this: "If we want a legal system, with the safeguards and advantages law provides, we must realize that we will have to pay a certain price. And that price is that we must consider ourselves obligated to obey the law." In this fashion the "basic norm" presupposes an obligation to obey the law as the *conditio sine qua non* of legal systems.

Kelsen's position is similar to that taken by T. D. Weldon in *The Vocabulary of Politics.* Weldon answers "Why should I obey the law?" in this manner:

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[22] There are other objections to "Why should I obey the law?" which remain, I take it, even if we consider this to be a question of obedience rather than obligation—e.g., the single question hides a cluster of distinct questions. See Hanna Pitkin's valuable essay "Obligation and Consent—I," *American Political Science Review,* 59 (December, 1965), 990–999,esp. p. 990–991.

[23] Hans Kelsen, "Why Should the Law be Obeyed?" in his *What Is Justice?* (Berkeley and Los Angeles: University of California Press, 1960), p. 262 (emphasis added).

> Suppose . . . the objector goes on to say "Even if it is the law, I don't see why I should obey it." The only further comment possible is "Well, this is Great Britain, isn't it?"
>
> The position is exactly parallel to that of the cricketer who asks "Why should I obey the umpire? What right has he to give me out?" One can answer only by expounding the rules of cricket, the position of the M.C.C., and so on. Beyond that there is nothing to be done except to say "This is a game of cricket, isn't it?"[24]

Like the obligation to obey the rules of cricket, the obligation to obey the law is presupposed by the rules of the game. And there is some sense to this notion—after all, why play a game if one does not intend to follow the rules? Indeed, it may be said that one who fails to follow the rules does not even play the game.

The advantages of the institutional (or game) model are clear. The model allows us to trace the obligation to a committing action, i.e., the act (or acts) of participating in the practices of the institution. It also provides an answer to the question, to whom is the political obligation owed? The obligation is owed to the other players, i.e., to one's fellow citizens. Thus, as one commentator puts it, "the game philosopher has found a way of relating political obligation to the individual's role as a citizen-subject rather than leaving it hanging as an external, accidental feature, say, of his birth."[25]

But there is also a crucial disadvantage. From the point of view of the game model it is senseless to ask either "Why should I obey the law?" or "Am I obligated to obey the law?" This tendency to deny that there can be any kind of general answer to the questions of obedience and obligation is best exemplified by Margaret Macdonald. In "The Language of Political Theory" Macdonald declares that "to ask why I should obey any laws is to ask whether there might be a political society without political obligations, which is absurd. For we mean by political society, groups of people organized according to rules enforced by some of their number."[26] Macdonald, like Kelsen and Weldon, takes the obligatory nature of laws for granted. A political society consists of groups of individuals living together according to rules; this is what "political society" *means.* It is therefore absurd to ask for a general justification of the obligation to obey the law. Our attention is better directed to particular questions of obedience and obligation—"Am I obligated to obey *this* law in *these* circumstances?"—and not to general questions which cannot be answered.

But Macdonald and others who accept the institutional model dismiss the general question of political obligation too hastily. I say this for two reasons. First, contrary to what Weldon says, the position of one who questions his obligation to obey the law is not "exactly parallel" to that of one who questions his obligation to follow the rules of the game. While it is sometimes helpful to look upon political life as a game, the analogy must not be taken too far. When we play games we participate voluntarily; we accept and undertake an obligation to obey the rules. But in most cases we do not choose to participate in a political society or legal system—this is usually a matter of the accident of birth.[27] Therefore, if we do have an obligation to obey the laws of the political society into which we are born, this obligation cannot be accounted for by the game analogy alone.

The second reason is that the institutional model attaches far too much significance to the conceptual link between "obligation" and "political society." There is a bit of sleight-of-hand in the arguments of Macdonald and in those of Thomas McPherson, who claims that

> any general question of the form "Why should we accept obligations?" is misconceived. "Why should I (a member) accept the rules of the club?" is an absurd question. Accepting the rules is part of what it *means* to be a member. Similarly, "Why should I obey the government?" is an absurd question. We have not understood what it *means* to be a member of political society if we suppose that political obligation is something that we might not have had and that therefore needs to be justified.[28]

Macdonald and McPherson may well be correct in arguing that anyone who belongs to a political society is necessarily obligated to obey the laws of that society, but this begs a crucial question: what is a political society? And: how is belonging signified and membership marked? The implication of those who adopt the institutional model is that only the Crusoes are *not* members of political societies. This simply dodges one of the most troublesome issues of political philosophy, *viz.*, the problem of legitimacy. If we recognize the powers-that-be in Xenotania as the legitimate government of Xenotania, then the general questions of obedience and obligation are senseless; but how do we know that their rule is indeed legitimate? The powers-that-be may issue directives and enforce obedience, but this does not distinguish them, as St. Augustine pointed out, from a band of robbers.

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[24] T. D. Weldon, *The Vocabulary of Politics,* p. 57. I owe this reference to Alan Gewirth, "Obligation: Political, Legal, Moral," in *Nomos XII: Political and Legal Obligation,* ed. J. Roland Pennock and John Chapman (New York: Atherton Press, 1970).

[25] John Ladd, "Legal and Moral Obligation," in Pennock and Chapman, pp. 21-22.

[26] Macdonald, "The Language of Political Theory," p. 192.

[27] Weldon disputes the significance of this point. "It is argued," he says, "that if I do not like my cricket club or my trade union or even my Church, I can resign from it and join a different one. But I cannot escape from my State. There seems to me to be very little in this. . . . Resigning from any institution involves some inconvenience. . . . But to say that one cannot escape from one's State is simply untrue. Normally emigration is possible, though it is sometimes so difficult as to be practically out of the question. But there is always suicide, and it is not in these days so very uncommon." *The Vocabulary of Politics,* p. 48.

[28] Thomas McPherson, *Political Obligation,* p. 64.

Responses to these questions are unlikely to be satisfactory, for political society—like art, religion, science, and democracy—is an *essentially contested concept.*[29] That is, we can neither define the concept in terms of necessary and sufficient criteria nor stipulate a generally acceptable definition. The reason for this is that political society is a concept which *informs* and *constitutes* our action and behavior. "Political society" can be, and is, used in a descriptive sense, but since the use of the concept both affects and reflects what we believe and what we do, the criteria for its use are in dispute. I may consider Xenotania to be a political society, for example, but someone else may consider it to be an unfortunate land ruled by naked coercion; and I may consider myself obligated to obey the laws of Xenotania, while another denies that the dictates of those in power are, properly speaking, *laws* at all.

It is important to notice that the dispute here is not like that between a batter, who judges a pitch to be a ball, and an umpire, who calls it a strike. The batter and umpire accept the same criteria for distinguishing balls from strikes, but they disagree about whether this particular pitch meets the criteria for a strike. In the case of political society, the contest centers directly on the criteria: what are we to take "political society" to mean? The concept is essentially contestable because the debate about when the concept may be properly used is inescapably normative.

When we strip the apparent neutrality from "political society" (or "legal system," or "state," or even "government"), we find that we have returned, in a sense, to the problem of the relationship between obligation and coercion. There are two choices: either we distinguish political societies from societies where naked power reigns or we deny that a member of a political society is necessarily obligated to obey the laws. Otherwise, we are once again in the untenable position of deriving obligation from coercion. Whichever of the two alternatives one accepts, the ultimate question remains the same: what are the characteristics of a regime to which I may be obligated? What remains, in other words, is the general question, "Am I obligated to obey the laws of *this* regime?"

This approach to the problem of political obligation resembles what Hanna Pitkin has called "the doctrine of hypothetical consent." "For a legitimate government," as Pitkin says, "a true authority, one whose subjects are obligated to obey it, emerges as being one to which they *ought to consent,* quite apart from whether they have done so.... Legitimate government is government which *deserves* consent."[30] The mere fact that one's life is governed in important respects by others is not enough to establish an obligation to obey the governors. The government must also be worthy of obedience—it must be a just state. Anyone, therefore, who attempts to develop a theory of political obligation must at the same time present a conception of a just state, and anyone who wishes to develop a theory of justice will also present a conception of a state which ought to be obeyed. This connection between justice, obligation, and obedience is explicit in the work of John Rawls and implicit in that of Hobbes, Locke, Rousseau, Green, and others.[31]

The notion of "hypothetical consent" is not sufficient, however, as a response to the question of political obligation. It may be that we *ought* to obey any government which deserves our consent, but this is *not* to say that we are *obligated* to such a government. To "hypothetical consent" we must also add participation. The reason for adding this requirement is simple but significant. Political obligation and citizenship are correlative: one can only have a *political* obligation to a state of which he is a citizen. There may be numerous political societies which deserve obedience, but we are not politically obligated to all of them. It is also possible, conversely, that someone may be sufficiently displeased with even a just society to leave it; and when one ceases to be a member of a society, when he renounces his citizenship, his political obligation ends as well.[32]

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[29] The notion of "essentially contested concepts" is developed by W. B. Gallie, *Philosophy and the Historical Understanding* (New York: Schocken, 1966), chap. 8. See also Alisdair MacIntyre, "The Essential Contestability of Some Social Concepts," *Ethics,* 84 (October, 1973), 1–9.

[30] Pitkin, "Obligation and Consent–I," p. 999 (emphasis in the original).

[31] Peter Singer seems to be working along these lines in *Democracy and Disobedience* (Oxford: Oxford University Press, 1973), where he argues that there are reasons for obeying the laws of democratic states which do not hold for nondemocracies. There are, according to Singer, two reasons for this claim to obedience: "firstly, that one ought to accept a decision-procedure which represented a fair compromise between competing claims to power. 'Accept' here involves both participating in and abiding by the results of the decision-procedure. Secondly, . . . that participation in a decision-procedure, when others are participating in good faith, creates a *prima facie* obligation to accept the results of the procedure" (p. 59).

By thus combining the emphasis on participation (from the institutional model) with the "nature of the government" or "hypothetical consent" doctrine, we arrive at the two conditions necessary and jointly sufficient to validate a political obligation claim. We are politically obligated to (obey the laws of) a state when (1) we are citizens of that state and (2) the state deserves our obedience. This means that the question of political obligation, "Am I obligated to obey the laws of this regime?", remains an open question: we must always determine whether the state is deserving of our obedience. There is no social contract which, once signed, obligates us for all time.

A political obligation, on this view, is a systemic obligation; that is, the obligation is not to obey any particular law of the polity, but to obey all its laws. This obligation, of course, is not absolute. It may be that in some circumstances we ought to disobey a law despite our political obligation. Nevertheless, a political obligation provides a *prima facie* reason for obedience which is absent in a state to which we have no political obligation. In this sense political obligations are like promises: if there are no overriding considerations, they ought to be honored.

But what if the situation is turned around? What if the society is not ruled justly? Do we then have no obligation to obey *any* of the commands of the rulers? Or what if the state is just, but we are simply visitors, not citizen-subjects; are we then not obligated to obey *any* laws of the state we are visiting?

This challenge to the conception of political obligation as a systemic obligation reverses the charge often brought against Locke. Locke extends the notion of tacit consent to the point where

> every man that hath any possessions, or enjoyment of any part of the dominions of any government, doth thereby give his tacit consent, and is so far forth obliged to obedience to the laws of that government during such enjoyment as anyone under it; whether this his possession be of land to him and his heirs for ever, or a lodging only for a week; or whether it be barely travelling freely on a highway; and in effect it reaches as far as the very being of any one within the territories of that government.[33]

One of the problems with this argument is that Locke leaps from an obligation to obey particular laws to a general obligation to obey all the laws of a country—as if one could be obligated to perform *all* actions of a certain class *because* he is obligated to perform *some* actions of that class. But does this mean that, on the other hand, we can have no obligations to obey particular laws unless we have a general political obligation? Do we, as one philosopher has said,

> want to say that a system like that of Nazi Germany, since obviously immoral, was therefore not a legal system? It is hard to see any temptations in such a view. For this would entail such absurdities as that no binding contracts were ever made in Nazi Germany, that all children born under Nazi "marriages" are bastards, and so forth.[34]

A brief examination shows that this argument does not hold. If we grant that Nazi Germany was an unjust regime, then, given my conception of political obligation, the Germans under the Nazi regime had no political obligation. This does *not* entail, however, that they were never obligated to obey *any* "laws" enforced by that regime. As long as two parties willingly entered into a contract, that contract was binding. Similarly, anyone today who drives a car undertakes an obligation to obey the traffic regulations no matter what the character of the regime which enforces those regulations. The point is that there are some rules which we are obligated to obey because we have undertaken the obligation, as we do when we sign a contract. These are what Hart calls "power conferring" rules. Laws of this sort empower private persons to do certain things, but, unlike "duty imposing" laws, they do not require performance.[35] The laws governing marriage, for instance, do not require us to marry, they simply confer the power to do so. Those living in Nazi Germany, therefore, may have had neither a political obligation nor specific obligations to serve in the army or pay taxes, but there certainly were occasions on which they were obligated to obey the law. Unless there are extraordinary circumstances, the immorality of a regime does not justify, for example, reckless driving. We may be *legally* obligated, in other words, even when we are not *politically* obligated.

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[32] One remains liable, of course, for obligations incurred before he renounced his citizenship; so that if one has a political obligation to a state which requires an income tax, he may not legitimately avoid payment by renouncing his citizenship just before the tax is due.

[33] *The Second Treatise of Government,* ed. J. W. Gough (Oxford: Blackwell's, 1966), §119, p. 61.

[34] Jeffrie Murphy, "Allegiance and Lawful Government," *Ethics,* 79 (October, 1968), 56–69, at 57. This quotation is somewhat out of context—Murphy is arguing against natural law theorists who claim that a legal system cannot be immoral—but I think it fits the present discussion well enough.

[35] *The Concept of Law,* pp. 26–48.

Conclusion

The problem of political obligation can be solved. If I am a citizen of a just state, then I have a political obligation to—and a reason for obeying the laws of—that state. But this is a modest solution, one that is hardly likely to resolve all the practical and theoretical difficulties usually associated with the notion of political obligation. To determine that one has a political obligation, in the first place, is to provide a reason for obeying the laws of the state; but it is not necessarily a conclusive reason. If I satisfy myself that I am (not) *obligated* to obey the law, I may still have to satisfy myself that I *ought* (not) to obey the law.

This solution, moreover, is purely formal. Before it can serve as a guide to action, it must be given content: conceptions of citizenship and just state must be supplied. This means, in turn, that it is necessary either to develop a full political theory or to work within the framework of an elaborated tradition of political discourse.[36] In the end, therefore, the solution does not guarantee that there is only one correct answer in any particular situation in which the question of political obligation arises.

This is not to say that the distinctions I have drawn are worthless. Much of the ambiguity in theories of political obligation can be traced to the failure to distinguish questions of obligation from questions of obedience—Hobbes is a good example. If we keep this distinction in mind, we can better understand, and avoid, the confusion which has plagued discussions of political obligation. And even if we cannot reach final agreement on troublesome questions, we may nevertheless benefit from knowing how and why we disagree.

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[36] That is, different conceptions of citizenship and just state are associated with different traditions in political theory. On this point, see Michael Walzer's essay, "The Problem of Citizenship," in his *Obligations: Essays on Disobedience, War, and Citizenship* (New York: Simon & Schuster, 1971). In *The Logic of the Social Contract* (Ph.D. dissertation, University of Minnesota, 1976) I argue that the social contract can be used to provide satisfactory conceptions of citizenship and the just state.

Hegel as a Social Scientist*

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The interpretive approach to social science is once more undergoing a revival. One sees evidence of this in the kinds of journals being founded, in the development of caucuses for a "new political science" and an "insurgent sociology," and generally in an attitude of renewed interest toward certain founders of the interpretive approach such as Alfred Schutz. By interpretive social science I here refer to what has loosely been termed the idealist approach.[1] This tradition is not a school of social thought unified by an ideological or practical outlook[2] but is, rather, a general attitude of scholarship, based upon respected and eminent statements of philosophy, which stands as a significant alternative to the positivist or behavioral mode of inquiry. It specifically invokes the judgments of Kant, Hegel and Husserl, and proclaims as its finest craftsmen such theoreticians as Schutz, Max Weber, and G. H. Mead. One could include in this tradition such schools (and semi-schools) of social science as symbolic interactionism, phenomenological sociology, transactionalism, ethnomethodology, and the so-called critical theory. It is in effect a loose federation of sympathetic approaches united by a generally nonpositivist or interpretive stance,[3] an attitude that has developed some of the most important concepts, and probably some of the best books, of contemporary social thought. This, then, is a serious approach to problems of social inquiry that ought to be reckoned with by all scholarly students of society. What is distressing, therefore, and somewhat perplexing in view of the recent revival, is that interest in and knowledge of such a tradition is still not especially widespread. Practicing social scientists, and especially political scientists, often seem unaware, or at best unconcerned, that there is another important way of doing social research.[4]

This paper is specifically about Hegel as a philosopher of social science and, in fact, as a social scientist. The main purpose will be to relate Hegel's work to the nonpositivist tradition of social inquiry just introduced. Such an effort will not try to label Hegel as the true source of interpretive social science, or to draw specific causal relationships between Hegel's writings and more modern works. The history of ideas is certainly a worthwhile topic for study. But specific statements of "who-said-it-first" or "who-owes-what-to-whom" are not really intended.[5] Rather, this paper is designed merely to illuminate certain elements of Hegel's thought, and to sketch some aspects of his influence. More specifically, by alluding to the nonpositivist tradition of social inquiry, I will attempt to identify Hegel's role in the propagation of certain philosophic principles essential to the development of that tradition, to demonstrate in what ways Hegel himself can be understood as an early sociologist and, finally, to indicate those aspects of Hegel's thought that might be of most use to contemporary philosophers of social and political inquiry.

Some recent scholarship has touched upon the social scientific aspects of Hegel's thought. This literature, however, tends to focus on Hegel's understanding of the relationship between that which is transcendent or meta-

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*I am grateful to John L. Stanley for his many valuable criticisms and suggestions; and to the anonymous reviewers for their comments.

[1] I will henceforth refrain from using the term idealist which is, I think, too strongly suggestive of Kantian formalism. While idealist conceptions of social inquiry probably should be included in the "nonpositivist" tradition, the more proper emphasis is on the interpretive kinds of knowledge resulting from various sorts of human action. Part of my effort will be to show that Hegel was not simply an idealist.

[2] A practical or policy-oriented social science is all that David Easton was able to find in the "post-behavioral" movement. See David Easton, "The New Revolution in Political Science," *American Political Science Review,* 63 (December, 1969), 1051–1061.

[3] Among the few attempts to synthesize these disparate approaches, see Robert W. Friedrichs, *A Sociology of Sociology* (New York: The Free Press of Glencoe, 1970); and Alvin Gouldner, *The Coming Crisis of Western Sociology* (New York: Avon Books, 1970). For an important critique see Karl R. Popper, *The Poverty of Historicism* (New York: Harper Torchbooks, 1964), chaps. 1 and 3.

[4] This point is made by John Gunnell, "Deduction, Explanation and Social Scientific Inquiry," *American Political Science Review,* 63 (December, 1969), at p. 1233.

[5] Such phrases are inspired by Andrew Hacker's "Capital and Carbuncles: The 'Great Books' Reappraised," *American Political Science Review,* 48 (September, 1954), pp. 775–786.

physical and that which is social or cultural. Several writers, such as Z. A. Pelczynski, have argued that the two realms are really distinct and that Hegel's social thought can be understood without reference to his metaphysics[6]; others, for example, Raymond Plant, find rather a coherent and unified system in which the social and the metaphysical are interwoven and mutually supportive[7]; and still others, most notably Shlomo Avineri, seem to contend that Hegel created a paradox in this regard which he never adequately resolved.[8] Insofar as the issue is a critical one, the present paper may be said to accept implicitly the second position, *viz.*, that Hegel's approach to the social world is an integral, and in fact fundamental, aspect of his philosophy. My more specific concern, however, is to trace systematically Hegel's social theory to the central, epistemological elements of his general philosophy, and to outline the implications of that theory for contemporary social scientists.

As an occasional admirer of the interpretive social sciences, I feel that clarification of Hegelian principles in these regards can be most valuable, especially in view of the recent revival of interest in Hegel's work. One caveat, though, is in order. I have felt compelled to spend some time discussing contemporary behavioralism and its methodological alternatives, and I trust that more sophisticated readers will bear with me through these sections.

The Critique of Positivism

I will begin with a word of terminological clarification. In the social sciences, positivism and behavioralism are not rigidly applicable terms. The most diverse studies using wide ranges of method can correctly be called positivist or behavioral. Sample surveys, investigations using census data, computer simulations, even certain kinds of case-studies—all can fit the positivist-behavioral category. Moreover, the terms are not precisely transferable from one discipline to another. Thus, sociological positivism tends to focus on the larger, "organic" structures of society; while political positivism is concerned, most basically, with the individual as a unit of analysis.[9] There is, however, an important sense in which these terms are meaningful and, in fact, crucial. Truman writes of behavioralism that it "aims at stating all phenomena . . . in terms of the observed and observable behavior of man."[10] Dahl adds that behavioralism "is an attempt to improve our understanding of politics by seeking to explain the empirical aspects of political life by means of methods, theories and criteria of proof that are acceptable according to the canons, conventions, and assumptions of modern empirical science."[11] Positivism and behavioralism (and also empiricism) are, therefore, increasingly identified with one another in two regards—the kinds of data deemed acceptable and the method of explanation considered appropriate. The emphasis is scientific, that is, on the collection of facts, usually in quantified form, and the verification of hypotheses. Given this identification, much of what is commonly thought of as behavioral is not, in fact positivist. Rather, the immensely significant works of such prominent behavioralists as Robert E. Lane, Edward Banfield and S. M. Lipset,[12] to name a few, involve less formal types of data collection and explanation than a strict positivist would perhaps allow.

The trends in social science, however, continue to reflect a growing willingness to quantify, objectify, and hence, to reify data regarding social and political behavior. In political science, the discipline with which I am most familiar, major new research efforts tend to involve various types of mathematical reduction. One thinks immediately of recent research on political participation and socialization, political culture, legislative roll-call analysis, judicial decision making, public policy analysis, and, perhaps most significant, formal models of politics involving theories of gaming, simulation and economics. In urban politics, long an area dominated by interpretive case-studies, recent interest has focused on policy determinants, on factors of urban ecology, and on models of decision making dealing with metropolitan public economies. All such efforts seek a kind of facticity that is discrete and unambiguous, validate their hypotheses through positivist methods of inference, and build upon these results explanations of unwarranted scope and subtlety. The most sensitive critics of this approach do not reject it out of hand but, rather, deny its adequacy as a method of hypothesis testing. Such critics might argue, for example, that statistical analysis ought not be considered the means of validating social science hypotheses; rather, it ought to be the generator of hypotheses, which can then be tested through a more personal knowledge of the particular processes involved. In these regards, then, "nonpositivism"—as used in this paper—is not to be considered a method that ignores facts but, rather, an approach that (1) accepts a wider range of "facts" and that (2) rejects therefore the strictly scientific criteria of explanation.

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[6] Z. A. Pelczynski, "Introductory Essay," in G. W. F. Hegel, *Hegel's Political Writings,* trans. T. M. Knox (London: Oxford University Press, 1964), pp. 136–137. For a discussion of Hegel's narrowly social and political works—i.e., those of his "prephilosophic" period—see H. S. Harris, *Hegel's Development* (Oxford: The Clarendon Press, 1972).

[7] Raymond Plant, *Hegel* (London: George Allen & Unwin, 1973). See also, John Plamenatz, "History as the Realization of Freedom," in *Hegel's Political Philosophy,* ed. Z. A. Pelczynski (Cambridge: Cambridge University Press, 1971).

As will be shown, some of the arguments contained in the present paper are considered by Plant, though in a somewhat different context. This is especially so when he discusses similarities between Hegel and Wittgenstein (pp. 202–203). Plant, however, only sketches these similarities in the most general of terms, for his main purposes lie elsewhere, viz., in Hegel's political or ideological motives. My effort, on the other hand, is to determine, pointedly and systematically, the philosophic sources of Hegel's social science, considered as a method, and to relate this social science not merely to Wittgenstein but to an entire tradition of social theory.

[8] This conclusion is at least implicit in Shlomo Avineri, "Consciousness and History: *List der Vernunft* in Hegel and Marx," in *New Studies in Hegel's Philosophy,* ed. Warren Steinkraus (New York: Holt, Rinehart and Winston, 1971), especially pp. 108–115.

[9] Robert Dahl, "The Behavioral Approach in Political Science: Epitaph for a Monument to a Successful Protest," *American Political Science Review,* 55 (December, 1961), 763–772.

[10] *Ibid.*, pp. 765–766.

[11] *Ibid.*

[12] See, for example, Robert E. Lane, *Political Ideology* (New York: The Free Press of Glencoe, 1962); Edward Banfield, *Political Influence* (New York: Free Press of Glencoe, 1961); S. M. Lipset, Martin Trow, and James Coleman, *Union Democracy* (Glencoe, Illinois: The Free Press, 1956).

The origin of the word "positivism," at least as an academic concept, is normally connected with the development of French sociology, especially with St. Simon and Comte. Hegel, however, certainly dealt with positivism, and his usage may be considered a fairly early example of its modern connotations. Hegel's conception of, and critical attitude toward, positivist inquiry is to be found in several areas of his work. In his discussions of law, for example, Hegel rejected those theories which limit themselves to the "brute facts," to such "contingent" elements as the letter of the law, its external or efficient causes, and its apparent harmony with popular will.[13] Such surface characteristics, Hegel maintained, render an excessively simple picture and, consequently, do not reveal the essential reality of law which is something much more involved and far less unambiguous.[14] Similarly, when dealing with historical analysis, Hegel deplored works which relied solely upon the manifest or "accidental" nature of history.[15] The products or manifestations of historical change in themselves explain very little; they are, in fact, "opaque to reason." In general then, Hegel argued that the underlying reality of manifest phenomena must be the more fundamental focus for inquiry.[16] In opposition to positivist theories, the kinds of facts Hegel deemed adequate would be *conceptual* or interpreted facts, that is, determinations based on understanding the "concepts" or "notions" that lay behind positive phenomena. As will be shown below, it was precisely these kinds of data that were central to his own social scientific analysis.[17]

The most rudimentary elements of this critique concern the excessively superficial nature of positive facts. The argument is not, however, merely one of superficiality versus depth, or simplicity versus subtlety. Rather, Hegel had fairly specific ideas regarding the constitution of that deeper, obscure region. What positivist theories are incapable of dealing with, he felt, are the dynamic and active—and, hence, truly *relational*—aspects of the social world. These aspects are described in Hegelian philosophy by the series of conceptions associated with the dialectical nature of history.[18] Insofar as he was concerned with more than systematic or taxonomic description, Hegel relied on his notion of dialectics to develop a social theory aimed at explaining the dynamic elements of the social world, including change, cause, and human creativity. As is well known, the foundation of this perspective is the comprehension of the negative aspect of human being in all its forms. Negativity, generally referring to Mind or human subjectivity, to that which transforms given reality by denying its completeness, is thus a primary datum or form of data for Hegel's social theory.[19] This element is, almost by definition, closed to the strict positivist. Unlike positive facts, negative "facts"—comprising the opposite of positivity—are not capable of being observed. They are not apparent or discrete, they do not present themselves. Hegel's specific method for analyzing negativity, "conceptual thought," will again be discussed below in a more appropriate context. For now, it can be asserted that his approach seeks to improve and expand upon the detached posture of the empirical or positivist observer.

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[13] G. W. F. Hegel, *The Philosophy of Right,* trans. T. M. Knox (New York: Oxford University Press, 1971), pp. 15–18 and 136.

[14] The relevance of Burke here as a theorist of positive law is suggested by Z. A. Pelczynski, "Introductory Essay," in G. W. F. Hegel, *Hegel's Political Writings,* ed. Z. A. Pelczynski, pp. 37–39.

[15] See M. B. Foster, *The Political Philosophies of Plato and Hegel* (New York: Russell & Russell, 1965).

[16] G. W. F. Hegel, *The Phenomenology of Mind,* trans. J. B. Baillie (New York: Harper & Row, 1967), p. 100. For an excellent discussion of this general point, see Plant, *Hegel,* pp. 90–92.

[17] Hegel, *The Phenomenology of Mind,* pp. 70–71, 95, and 129. See also Hegel, *The Philosophy of Right,* pages 14–15 and 225.

[18] For a provocative discussion of Hegelian dialectics see Alexander Kojeve, *Introduction to the Reading of Hegel*(New York: Basic Books, 1969), pp. 44–60.

[19] Hegel, *The Phenomenology of Mind,* pp.80–01. I should emphasize that the words "mind" and "spirit",

Hegel thus criticized positivism not merely for dealing with social facts in an inadequate or superficial way, but more importantly for ignoring a very basic class of social facts, those relating to the negative aspect of being. Further, as is I think implicit in the foregoing discussion, Hegel was concerned to reject not only the positivist method of data selection, but also the positivist mode of explanation. For the positivist, explanation is necessarily limited to the inference of relationship, inference based largely on the observed coincidence of positive facts. Most primitively, if social fact X is observed to be always followed by social fact Y in the presence of social fact Z, then one may infer a causal or other type of sociological law as the explanation.[20] But because of the empiricist refusal to look at anything other than positive phenomena, the explanation of any social regularity can only be inferred. Hegel's writing often seems to show more concern with this consequence than with any other aspect of the positivist approach.[21]

As should now be apparent, this discussion is both epistemological and methodological and leads, in effect, to a program for scientific-philosophic research. That is, according to Hegel, the ultimate focus of social inquiry ought to be those distinctively human attributes—for example, thought and will—that constitute the realm of negativity. These elements are not properly subject to positivist investigation. The very focus of positivism, therefore, renders it an unsatisfactory approach. As will be seen, the philosophic or interpretive attitude of Hegel, on the other hand, relies upon a different conception of facticity and upon a less equivocal mode of explanation and, thereby, attempts to focus on the pivotal question of "mind." This program is clearly evident in Hegel's constructive work. Suffice it to say for now that the foundation of Hegelian social philosophy—contained in *The Phenomenology of Mind*—is primarily an historical study of the modes and structures of human consciousness.

Hegel's critique and program thus far described, comprising a general orientation toward the social world, is so similar to the current nonpositivist outlook that the identification of a single tradition seems most reasonable.[22] The contemporary critique of modern behavioralism is aimed precisely at behavioralists' reliance upon the dual positivist principles of data selection and explanation. The problem is thus considered to lie (as Hegel had affirmed) in the attempt to piece together from discrete, self-evident data some subtle truth about the nature of a complex social process. Seduced, so the critique goes, by the apparent success of the natural sciences, and pushed on by the desire to be scientific in their own right, social theorists have sought to transfer the techniques of the hypothetico-empirical method, including, and especially, quantitative techniques, to the study of human society. This would, in turn, lead to the development of a body of objective, scientific data and to the further discovery of social laws. In this way the dynamics of society could be understood as are, for example, the laws of physics.[23]

According to the nonpositivists, the consequences of this posture are severely limiting. To view men "scientifically" is to treat them as objects which, in turn, is to ignore the fullness of their humanity. That is, the natural science or positive approach precludes direct examination of the truly distinctive human characteristics—thought and consciousness. Man, the critique continues, can only be understood if his entire self is accounted for. This requires, among other things, that the investigator be aware of and informed by the fact that he is of the same species as his subjects. The natural scientist can with ease detach himself from the objects of his inquiry. But the social scientist, a human being, has the capacity to understand his subjects in a much more complete and empathetic way. Scholarship, the critique concludes, demands the exploitation of

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as used in this paper, should be read as translations of *Geist.*

[20] Perhaps the best contemporary discussion of the positivist theory of causality is Paul F. Lazarsfeld, "Interpretation of Statistical Relations as a Research Operation," in *The Language of Social Research,* ed. P. F. Lazarsfeld and Morris Rosenberg (New York: The Free Press, 1955), especially pp. 121–125.

[21] Hegel, *The Phenomenology of Mind,* p. 95.

[22] For a different view, which links Hegel with a more positivist brand of sociology, see Pelczynski, *Hegel's Political Philosophy,* pp. 238–241.

[23] Among the classic formulations of this critique are Wilhelm Dilthey, *Pattern and Meaning in History* (New York: Harper & Row, 1961), especially chaps. III and IV; and Karl Mannheim, *Ideology and Utopia* (New York: Harvest Books, undated), pp. 290–291.

this capacity, and the consequent discovery of facts that go beyond discrete, quantifiable data.[24]

The essence of the contemporary critique thus emphasizes the inadequacy of natural science methods to the study of society. Men are not mere objects, the critique maintains, but are purposive beings, capable of thought and conscious of themselves and their setting. To base one's understanding of these thought processes solely on their manifestations is to underestimate the very real possibility that such manifestations are "opaque to reason." That is, behavior may not at all mean what the detached observer thinks it means. Analysis of human beings therefore requires something beyond the scientific detachment of behavioralism. For, again, the distinctively human attributes—purposiveness, thought, consciousness—are not positive phenomena but reside in a realm beyond the sensory powers.

The relevance of Hegel for the contemporary debate may, in one sense, seem rather slight. Hegel's discussion of positivity is designed primarily to clear a path toward arriving at a true ethic and a correct metaphysic. The contemporary nonpositivists, meanwhile, are concerned with the somewhat less ambitious matters of scholarly investigation and method. In another sense, though, the relationship is profound. For what is at issue in each case is a matter of epistemology. Both Hegel and the nonpositivists view positive epistemology as being inadequate to the task of properly understanding social man. In each case, the relevance of what is apparent and manifest is not denied. What *is* denied, however, is the ability of positivism or behavioralism to even approach a complete understanding of the human condition. Hegel and the nonpositivists agree that human consciousness is the fundamental and distinguishing characteristic of social man. And, further, they agree on this central point: that understanding of this deeper realm cannot depend solely on the inferences derived from the analysis of self-evident, manifest data. This tradition then seeks not to understand consciousness by observing behavior, but to understand behavior by grappling first with the nature of human thought.

The Nonpositivist Alternative

The most appropriate way to outline the alternative proposed by the nonpositivists is to begin by following their search for epistemological principles. This search sought to discover premises in the history of ideas that denied the philosophic bases of behavioralism. The rejection of positivism thus entailed a rejection of empiricist philosophy, and the nonpositivists have consequently come to view the history of empiricism with suspicion.[25] More specifically, the idea of the human mind as a *tabula rasa,* equipped with sensors capable of perceiving the reality of a separate and concrete world, is regarded as the source of the positivist viewpoint. From the British empiricism of Locke and Hume to the logical positivism of the twentieth century, certain fundamental positions have remained essentially unchanged: the concrete world is a self-contained reality; in the knowing process, subject and object are separate and autonomous; any attempt at knowledge other than that based on the observation of manifest reality is either impossible or meaningless; the goals of inquiry are prediction and verification. This epistemology leaves little room for any methodology but positivism. It says, in effect, that the role of the mind in the knowing process is passive. Objective reality exists independently, and the mind, via the senses, merely records images of that reality. A good mind, then, is little more than a faithful recorder. Such a standpoint limits the field of systematic inquiry to that which is scientifically observable.

systematic inquiry to that which is scientifically observable.

It is quite natural that the great nonempiricist philosophies of the nineteenth century would become the inspiration for a nonpositivist social science. Conventionally, the resurrection of nonempiricist epistemology is traced to Kant's *Critique of Pure Reason.* The fundamental change wrought by Kant was to establish the active role of the human mind in the knowing process. That is, Kant found in the mind not a blank canvas but a series of categories with which observable phenomena could be endowed with meaning. Meaningful reality, in effect, is not to be considered autonomous from human thought. The mind, by virtue of processes that exist prior to the external world, constitutes the nature of that world's reality.

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[24] Many contemporary writers have argued along these lines. Among the best sources are Peter Winch, *The Idea of a Social Science* (London: Routledge and Kegan Paul, 1960), chap. 1; John Gunnell, "Deduction, Explanation, and Social Scientific Inquiry"; Eugene Miller, "Positivism, Historicism, and Political Inquiry," *American Political Science Review,* 66 (September, 1972), 796-817; and Hwa Yol Jung, "A Phenomenological Critique of the Behavioral Persuasion in Politics," paper presented at the 1971 Annual Meeting of the American Political Science Association. Another interesting argument is found in Eric Voegelin, *The New Science of Politics*(Chicago: University of Chicago Press, 1952). Voegelin of course is arguing from a somewhat different perspective. But his analyses of the inadequacy of natural science methods (p. 4) and of the role of human creativity (p. 27) are strikingly suggestive of the tradition here discussed.

[25] See Winch, *The Idea of a Social Science,* pp. 7–24.

These well-known principles of idealism were seriously amended by the Hegelian system. Most basically, Hegel expanded the scope of possible knowledge to include what Kant had called the noumena.[26] In this respect, however, the Hegelian epistemology is best understood as a crucial extension and radicalization of Kant's principle of constitution. On this point George Lichtheim has written: "In extending the Kantian approach from the realm of nature to history, Hegel affirmed the governing principle of idealist faith: matter is organized by mind."[27] By placing emphasis on the "hidden" side of things, as discussed above, Hegel was inevitably led to view knowledge as the process of an active mind or, even further, of an active being. He extended Kant's principles by adding to knowledge an historical aspect; and he radicalized these principles by making knowledge the function of all activity.[28] In so doing, Hegel increased the scope of possible knowledge to include noumenal as well as phenomenal elements. The metaphysic (but also the ontology, history and sociology) of *The Phenomenology of Mind* is based on the principle of action, negation, or process.[29]

For Hegel,

> . . . The living substance . . . is that being which is truly subject . . . is truly realized and actual solely in the process of positing itself, or in mediating with its own self. its transactions from one state or position to its opposite. As subject it is pure and simple negativity, . . . True reality is merely this process of reinstating self-identity, of reflection into its own self in and from its other. . . .[30]

This process of negation begets not merely destruction but a higher level of self-consciousness, a term which in the Hegelian philosophy means knowledge. The process Hegel describes is as much as anything else an epistemology. Each successive level of consciousness means that a larger piece of reality is understood, makes sense, and is recognized as real and rational.

That the process of knowing is inextricably bound up with metaphysical reality is made quite explicit. In a fundamental passage of the "Preface" to the *Phenomenology* Hegel wrote:

> Spirit is alone Reality. . . (I)t must be presented to itself as an object, but at the same time straightaway annul and transcend this objective form; it must be its own object in which it finds itself reflected. So far as its spiritual content is produced by its own activity, it is only we who know spirit to be for itself, to be objective to itself; but in so far as spirit knows itself to be for itself, then this self-production, the pure notion, is the sphere and element in which its objectification takes effect, and where it gets its existential form. In this way it is in its existence aware of itself as an object in which its own self is reflected. Mind, which, when thus developed, knows itself to be mind, is science. Science is its realization, and the kingdom it sets up for itself is its own native element.
>
> A self having knowledge purely of itself in the absolute antithesis of itself . . . is knowledge in universal form.[31]

Hegel has here asserted that the essential metaphysical categories—action and process, subject and substance, self-positing and self-production—are utterly dependent upon the essential epistemological categories. Spirit that "knows itself to be for itself" is, thus, engaged in "self-production." When it becomes "conscious of itself as spirit," it then truly becomes spirit in its "existential form." Spirit thus exists only when it comes to "annul and transcend" itself, a process of consciousness, awareness and knowledge. Mind—the highest realm of spirit, "the pure ether," and hence the only reality—is also, "when thus developed," science. The Hegelian conception of being always involves the element of self-creation and re-creation through self-awareness or self-knowledge. Spirit, which is the totality, truly exists insofar as it knows itself. The human mind, itself a basic aspect of spirit, knows spirit, hence itself, and hence all knowable things, only through the distinctive forms of *activity* associated with self-annulment, self-transcendence, and—what is synonymous with these—self-creation.

This process of knowledge, for Hegel, is properly included in all truly human activity. Hence, knowledge can never be purely passive or detached but, rather, like life itself, demands an element of action, confrontation, and immersion. The process through which man properly knows, Hegel calls "conceptual thought" which "strenuously penetrates to the very depths of . . . meaning."[32] This means that conceptual thought is, perhaps alone, capable of comprehending that element of the world which explains the dynamics of spirit, viz., the truth of negativity, understood as the transforming and "overcoming" aspect of being. For Hegel, such "truth finds the medium of its existence in notions of conceptions alone. . .",[33] the pursuit of which constitutes science. Science involves, to a greater extent, diremption: "the action of separating the elements is the exercise of the force of Understanding, the most astonishing and greatest of all powers, or rather, the absolute power."[34] But to do this, to arrive at those concepts and notions that constitute knowledge, means to participate in the object under consideration. True scientific knowledge

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[26] See Josef Maier, *On Hegel's Critique of Kant* (New York: Columbia University Press, 1939).

[27] George Lichtheim, *The Concept of Ideology* (New York: Vintage Books, 1967), p. 13.

[28] A brief discussion of these points, though with a different focus, can be found in Alisdair McIntyre, *Herbert Marcuse: An Exposition and a Polemic* (New York: Viking Press, 1970), pp. 22–25.

[29] See Richard Bernstein, *Praxis and Action* (Philadelphia: University of Pennsylvania Press, 1971), part 1.

[30] G. W. F. Hegel, *The Phenomenology of Mind*, page 80.

[31] *Ibid.*, p. 86.

> . . . demands abandonment to the very life of the object, or, which means the same thing, claims to have before it the inner necessity controlling the object, and to express this only. Steeping itself in its object, it forgets to take [the] general survey. . . .[35]

Since the object of knowledge is spirit, which is the totality, all properly human actions—creation, mediation, transformation—are fundamentally epistemological. True knowledge comes through action and immersion.

In the *Phenomenology,* Hegel's critique of positivist doctrine is most explicit. His description of the proper knowing process is pointedly contrasted to that which merely consists of "casual philosophical reflections, referring to objects here and there, to processes and thoughts of the undeveloped mind as chance may direct. . . ."; and which tries to "establish the truth by miscellaneous ratiocinations, inferences, and consequences drawn from circumscribed thoughts."[36] The argument fundamentally rejects any "form of knowledge external to its material."[37]

The principles of action and creation, then, are the bases both of Hegelian metaphysics and of Hegelian epistemology. Knowledge requires the knower to involve himself truly with his material, and thereby to discover the I in the Thou. Conceptual thought involves, most fundamentally, a process whereby the fact of experience is itself experienced.[38] In this sense, "truth is thus the bacchanalian revel, where not a member is sober; . . ."[39]

Upon this interpretation of Hegel, the proper object of knowledge is, at least to a considerable extent, social existence. The human world, which man can truly experience, is not merely the reflection of a transcendent and controlling realm, but is rather the very substance and energy of Spirit. It is only by virtue of man's unique capacity for thought and interaction that Spirit is as it is. One thus comes to understand the transcendent realm by first understanding, through conceptual thought, the world of human or cultural affairs. This general point is made emphatically by Raymond Plant:

> Because he saw the problems of social, personal and religious bifurcation as the central social and political difficulties of his age, it follows that in so far as he considered that philosophy, when taken as a whole, could provide solutions to these problems it can fairly be said that the *whole* of his philosophy, however abstract, is pervaded by this insistent and central social concern.[40]

The influence of this orientation upon succeeding philosophies is indisputable. While the positivist line remained an attractive and forceful method for social thinkers, perhaps the most original philosophers of the nineteenth and twentieth centuries seemed to adopt some variant of the constitutive or creative principle. A history of modern epistemology is certainly not possible here.[41] But the importance of the nonpositive attitude can be quickly and effectively noted. Thus, for example, Marx's formulations regarding theory and practice show a rejection of empiricist detachment in favor of an active subject knowing his object through a variant of the Hegelian negation.[42] Thus also can be noted the intuitive understanding of Bergson, and the aesthetic metaphysics of the young Nietzsche, who celebrated knowledge as a kind of passionate drama between the knower and the known.[43] The pragmatic epistemology also emphasizes, though in a different way, the active role of the subject; and this pragmatism interestingly meets Bergson and Marx in the work of Sorel, whose notion of myth is among the clearest examples of knowledge as a human creation.[44] Perhaps most important of all is the philosophy of Husserl and its influence upon modern existentialism. Husserl's principle of intentionality indicates that the meaning of an object is "intended" by the subject. That is, the knower lends reality to the object and in this way eventually creates a meaningful environment, a "life-world." This concept of intentionality is illustrative of the main elements of constitutive epistemology. For Husserl, the prior reality of the object is not denied. Things, in all likelihood, do exist before we "intend" them. But since knowledge of this prior existence is beyond our scope, the problem is suspended or "bracketed" out. Husserl's assertion is that objects have *reality for us* only through the meanings we attach to them. This reality, the reality intended by the human mind, is clearly the only reality of any consequence. Again, though, as Quentin Lauer points out, our intentions must be constantly revalidated by endless resort to phenomenological reductions.[45] Thus, phenomenology—and nearly all of the constitutive theories—asserts that, as regards the possibility of human knowledge, subject and object are not truly independent of one another. The subject does not merely record images evoked by the object but, rather, acts upon that object in some way and creates for it an appropriate real meaning. We can know, therefore, only to the extent that we can create.

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[32] Hegel, *Phenomenology of Mind,* p. 70. See also Plant, *Hegel,* p. 126.

[33] *Phenomenology,* p. 61.

[34] *Ibid.,* p. 93.

[35] *Ibid.,* p. 112.

[36] *Ibid.,* p. 95.

[37] *Ibid.,* p. 106.

[38] *Ibid.,* p. 96.

[39] *Ibid.,* p. 105.

[40] Plant, *Hegel,* p. 88. See also, Shlomo Avineri, *Hegel's Theory of the Modern State* (Cambridge: Cambridge University Press, 1972), pp. 63–65 and 87.

[41] A major discussion of this aspect of the history of thought is found in Karl Lowith, *From Hegel to Nietzsche* (New York: Holt, Rinehart and Winston, 1964). A more recent work is Bernstein, *Praxis and Action.*

[42] Of course, finding an epistemology in Marx is not that simple. I am here emphasizing the "early" Marx in which the influence of German philosophy upon his thought is beyond question. See "Theses on Feuerbach," in *Marx and Engels: Basic Writings on Politics and Philosophy,* ed. Lewis Feuer (Garden City: Anchor Books, 1959), pp. 243–246.

This epistemology is implicit in all contemporary interpretive sociology. One explicit formulation of such a viewpoint is to be found in an analysis by Harold Garfinkel of the theories of Alfred Schutz. This paper is worth quoting at length. According to Garfinkel, Schutz adopts a "congruence theory" of such knowledge in which "the two terms, *perceived object* and *concrete object* are synonomous and interchangeable. . . . Rather than there being a world of concrete objects which a theory cuts this way and that, (Schutz's) view holds that the cake is constituted in the very act of cutting." This is opposed to what Garfinkel calls the "correspondence theory," as adopted by (for example) Talcott Parsons, in which

> the function of any schema of categories is to render some sort of approximation of what is actually out there. The view is that through successive states of improvement, the logico-empirical methods, based as they are on universal and unchanging primary categories of apprehension, gives to the properly qualified observer a view of the real world that is independent of the historical conditions of the observer's circumstances as far as the accuracy of reproduction of actual things is concerned.[46]

This latter theory is clearly positivist, especially in its emphasis on subjective-objective separation, on knowledge as the sensory recording of images corresponding to "real" things, and on the universal or ahistorical aspect of the knowing procedure.

In contrast, the "congruence" and other nonpositivist theories view knowledge as an intimate, creative, historical and social activity. This latter, the social aspect, is crucial, for it signals the fundamental element of interpretive sociology. Nearly all of the nonpositivist philosophies cited above, with the exception of Kant's, contain an implicit social element. Husserl's "life-world," Marx's class-consciousness, Sorel's myth of the general strike, even the integral experience of Bergson—each concept is of a social nature.[47] The empirical-positivist tradition finds knowledge to be the fruit of individual minds. The subject-object relationship requires no social element and, consequently, may be immune to historical change.[48] Nonpositivist epistemology, on the other hand, strongly implies a socio-historical aspect to the process of knowing. This implication was first made the focus of inquiry by early developers of the sociology of knowledge, especially Dilthey, Weber, and Mannheim.[49] These writers, of course, emphasized the relativity (or relationality)[50] of knowledge and focused their attention upon the larger postulates of society. Ideologies, or the accepted doctrines of society, were to be regarded as products of culture and historical circumstances. One understands thought processes, then, by examining the social foundations—the substructure—of knowledge.

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[43] See Friedrich Nietzsche, *The Birth of Tragedy and the Genealogy of Morals* (Garden City: Doubleday, 1956), pp. 23 and 126–127.

[44] See Georges Sorel, *Reflections on Violence* (New York: Collier Books, 1972), pp. 120–125; also "The Utility of Pragmatism," in *From Georges Sorel,* trans. John Stanley (New York: Oxford University Press, 1976).

[45] Quentin Lauer, *Phenomenology: Its Genesis and Prospect* (New York: Harper & Row, 1965), pp. 46–64.

[46] Harold Garfinkel, "A Comparison of Decisions Made on Four Pre-Theoretical Problems by Talcott Parsons and Alfred Schutz," distributed by the Department of Social Sciences, University of California, Irvine, 1972.

[47] An even more specific example is John Dewey, who wrote: "Knowledge is a function of association and communication." *The Public and its Problems* (Chicago: The Swallow Press, undated), p. 158.

[48] Perhaps a fundamental exception is to be found in the ordinary-language approach to philosophy. The early arguments of Hobbes's *Leviathan* are pertinent in this sense. More recent are the *Philosophical Investigations* of Wittgenstein. The relationship of these ideas to the present subject matter is discussed in Randall Ihara, "Phenomenology and Political Science" (paper presented at the 1971 Annual Meeting of the American Political Science Association). See also, Eugene Miller, "Positivism, Historicism, and Political Inquiry."

Although strongly influenced by certain of these principles derived from the sociology of knowledge, the nonpositivists have radicalized those very principles by returning to philosophic tradition. Sociologists of knowledge were concerned primarily with the most general kinds of knowledge. They sought to explain beliefs, religious doctrines, and political ideologies. But current writers have attempted to rediscover more basic questions. Most epistemologies, of whatever nature, deal not with the largest elements of belief but with the smallest. Knowledge as an everyday phenomenon is the heart of epistemology, and this is what has been rediscovered by the interpretive sociologists. The concern, then, is not with ideology or religion, but with the everyday constitution of reality.

Epistemology and Social Science

In making this emphasis, interpretive sociologists have developed the basis for an alternative approach to social science. The usual social variables—class, status, power, religion[51]—are now viewed by the interpretivists not as objective structures that shape human life but primarily as the products of the collective social will. They are human creations and can be properly understood only by understanding the ways in which they are created, the ways in which they are vested with meaning. Thus, the social construction of social reality becomes the focus of inquiry.[52] Herein can be understood, then, Weber's emphasis on the subjective meaning of social forms,[53] or Mead's concepts of symbol, self, and other. Equally, the "negotiations" of Garfinkel and Cicourel,[54] the "life-world" of Schutz,[55] the "transactions" of Henry Kariel,[56] all are based on a common interpretation of reality as a social construct. In each case, the task of the sociologist is to understand social forms by looking at the ways in which those forms are constituted. And in each case, the emphasis is on the creative activity of human will.

It may appear that the argument has just leapt from a question of epistemology to a program for sociology. But in fact, the entire nonpositivist tradition makes the knowing process the basis for statements of ethics, social philosophy and even metaphysics. What is characteristically emphasized is the creativity of the human mind, posited as active will in a social setting. The two main products of this will, knowledge and reality, are in fact one and the same thing. That is, the knower comes to know his object by constituting or creating its very reality. Without this kind of subjective activity, the object in effect would cease to exist; it would cease to have any real meaning. Mannheim writes: "Isolated and discrete facts must be comprehended in the ever-present but constantly changing configurations of experience in which they are actually lived. Only in such a context do they acquire meaning."[57] Thus, a theory of knowledge and a theory of society can be derived from a single principle.

But this argument logically leads to a further central intuition: if the human principle of action is the source of all knowledge and the source of all social reality, and if indeed knowledge and reality are one and the same thing, then social forms are in effect the codification or symbolic representation of knowledge. Stated otherwise, society is or represents nothing more than a set of common meanings, especially regarding human relationships, meanings wrought and constantly being revised through the genius of human action and will, but also providing a context for such activity in the future. Thus, for example, "social class" is best understood not simply as a quantifiably grounded concept, nor even as a description of life-style priorities. Though such aspects should not be dispensed with, a full understanding requires insight into the socially generated meaning of these kinds of criteria. Social classes certainly exist in the United States as well as in England. Outward manifestations of class in each country, including economic and life-style factors, may well be similar. But for various reasons, among them the American tradition of upward mobility, the meanings of social class are not consistent. Americans, that is, have an understanding of social inequality that differs from the English conception. In each case, the meaning of social class is, at once, a way of determining the societal role of inequality and a definition of that very aspect of social reality. The source of this meaning, or the process through which it is generated, thus becomes the focus of social research.

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[49] See Lichtheim, *The Concept of Ideology*, chap. 1.

[50] Relationalism is Mannheim's word, and he explicitly opposes it to relativism. See Mannheim, *Ideology and Utopia*, pp. 78–79.

[51] For a discussion of such variables see Robert A. Nisbet, *The Sociological Tradition* (New York: Basic Books, 1966).

[52] This phrase is borrowed from the book by Peter Berger and Thomas Luckmann, *The Social Construction of Reality* (Garden City: Doubleday, 1966), one of the best discussions of the kind of social science presented here.

[53] This is, to be sure, an excessively facile way of dealing with Weber. Despite a great deal of exposition by social theorists, I think his methodological position remains unclear. Nonetheless, Weber felt that social scientists can "accomplish something which is never attainable in the natural sciences, namely, the subjective understanding of the actions of individuals in interaction. . . . This additional achievement of explanation by interpretative understanding, as distinguished from external observation is of course attained only at a price—the more hypothetical and fragmentary character of its results. Nevertheless, subjective understanding is the specific characteristic of sociological knowledge" (Max Weber, *The Theory of Social and Economic Organization*, ed. Talcott Parsons [New York: The Free Press, 1969], pp. 103–104).

[54] See Aaron Cicourel, "Basic and Normative Rules in the Negotiation of Status and Role," in *Recent Sociology No. 2*, ed. Hans Peter Dreitzel (New York: The Macmillan Company, 1970), pp. 4–45.

[55] See Aron Gurwitsch, "Problems of the Life-World," in *Phenomenology and Social Reality*, ed. Maurice Natanson (The Hague: Martinus Nijhoff, 1970).

[56] See Henry Kariel, *Saving Appearances: The Reestablishment of Political Science* (Belmont, Calif.: Duxbury Press, 1972), chaps. 6 and 7.

[57] Mannheim, *Ideology and Utopia*, p. 60.

One further implication here is that the social inquirer, in order to understand his subject, cannot maintain the traditionally detached posture of scientific observation. Rather, he, like anyone else, must in some way become intimate with his subject in order to know it properly. He must therefore adopt a nonpositivist stance usually involving any of a variety of techniques and methods, including (for example) participant observation, in-depth interviewing, certain case-study techniques, and the historical-philosophical methods implied in such terms as hermeneutics, dialectics and *Verstehen*.

Hegel's sociological analyses were historical in nature. Thus, immediate contact with the objects of inquiry was not possible. However, Hegel's use of interpretive or conceptual reasoning is striking. The application of his own philosophic principles to the study of civilizations was based on a kind of understanding that anticipated the interpretive methods of Dilthey and his followers. All such theorists, including, and especially, Hegel, emphasized (or are emphasizing) a method of social research and analysis that may be called philosophic. Hegel's sociological method, thus, is essentially an interpretive or philosophic method.[58]

Substantively, Hegel's social science is concerned with the concept of will. The focus of all Hegelian practical philosophy is the creativity of human will, usually in a social setting. Hegel's dialectic of history is driven by this active will which negates or mediates that which is and, in so doing, creates something new. This basic concept informs the entire Hegelian system, including the sociology. Hegel's social analyses, then, are focused on the principle of human creativity, from which can be derived the notion that social forms are produced by active social will—that is, by mind. Thus, Hegel's system leads him to describe the social construction of social reality. I will use one of the best examples of Hegelian sociology, the discussion of ancient Greece, to illustrate this assertion.

The Interpretation of Greece

According to Hegelian philosophy, the world is to be understood as an historical entity. As such, it is dynamic and evolutionary; its various states are merely moments. Conditions and relationships are but temporary manifestations of the historical principle and, in fact, contain within them the very basis of change. Historical movement is described by the threefold dialectic of history, of which the middle term—antithesis—is perhaps the most important. The dynamic force of the historical world is the negating, mediating, antithetical element. *Geist* is best understood as describing that element, the distinctively human aspect of things which is the prime mover. The world changes because of the activity of human spirit, of mind.

This most central dynamic element arises first in Greece. The Oriental world, with which Hegel began his philosophy of history, was a stagnant world, containing merely the seed of humanity. It is only with Greece, the period of man's "adolescence," that the true nature of humanity is evidenced. For Hegel, the Greek epoch is the first spiritual epoch, the first mediating—hence the first historic—epoch. Clearly, said Hegel, it is in Greece that man puts his initial stamp upon nature, "transforms it into mere objective form of its (spirit's) own existence."[59] But further, Hellenic man really sets history in motion by pitting spirit against spirit. Among the causal mechanisms found in this regard are geographic, demographic and historic factors. That is, the plurality of Greece permits the confrontation of different human beings, a confrontation that overcomes itself and grows or evolves—historically—into new struggles. In Hegel's more philosophical terms this aspect is called the Desire for Desire, involving that kind of conflict between will, or minds, that distinguishes man from the lower orders.[60] In writing about history, the very same theory is stated thus: "The only real antithesis that Spirit can have is itself spiritual: viz., its inherent heterogeneity, through which alone it acquires the power of realizing itself as Spirit."[61] Man, though incomplete,[62] first beomes truly human in the Greek world; this by virtue of his new-found dimension of creativity, of transforming action. Greece embodies, and is identified with, this distinctively human dimension; it is the "region of spirit." Most generally, the Greek epoch is the mediation of the Oriental thesis.

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[58] For a discussion of the relationship between philosophy and science see Hegel, *The Phenomenology of Mind*, pages 86–92.

[59] Hegel, *The Philosophy of History*, trans. J. Sibree (New York: Dover Publications, 1956), p. 238.

In this regard, the idea of *Geist* can be properly understood. The apparent futility of translating this word is well known. The proper aim, therefore, is to adequately render its sense. Perhaps the most facile translation of *Geist* is spirit. But the term "spirit" though often very appropriate, fails to describe sufficiently the uniquely human aspect of *Geist*. Hegel's concept of the historical world centers on the primacy of creative, active man. The source of man's creative ability, and that which distinguishes him from other forms of being, is the faculty of mind and its capacity to be conscious of its environment, of other minds, and of itself. Mind, then, is the dynamic force of history, that which unfolds through consciousness and finally evolves, at the end, into a consciousness of all worldly relationships. Thus, *Geist* is best understood as mind. In the Oriental epoch, the human mind lay dormant. Such changes and transformations as did occur resulted not from spiritual mediation but from the clash of natural principles. The truly human aspect was not yet a factor.[63]

It is in Greece that mind is at last activated, in the form of individual subjectivity and thought; mind, or *Geist,* is set free and the result is that true history is begun. For Hegel, this was the essence of the Greek epoch: "This stamps the Greek character as that of *Individuality conditioned by Beauty,* which is produced by Spirit, transforming the merely Natural into an expression of its own being."[64] It was in this sense that Hegel found the basis of Greek unity. The plurality of the Hellenic world demanded a new kind of unity, an artificial or manmade bond. The Oriental world was united by natural or divine principles. For example, regarding the social organization of India, Hegel wrote: "The different castes are indeed, fixed; but in view of the religious doctrine that established them, they wear the aspect of *natural* distinctions."[65] Similarly, he saw China as the "realm of theocratic despotism," and Persia as the government of "law . . . still regarded as a dictum of mere nature."[66] In contrast, the unity of Greece transcends the natural and discovers, rather, a spiritual or distinctively human solution. The initial step of history is taken, in which man begins to liberate himself from natural necessity. In Greece, for essentially the first time, man found no prior basis for social organization. His response was to create one. Thus, Hegel described the Greek world as a "condition in which individuals take their own ground, and are not, from the very beginning, patriarchally united by a bond of *Nature,* but realize a union through some other medium—through Law and Custom having the sanction of Spirit."[67] The society of Greece was created by man. It can be best understood, then, in terms of the process of creation. Hegel's sociology is the attempt to understand society in terms of those states of mind that inform the social (or human) construction of social reality.

The sociological analysis of Greece emphasizes three social forms. First is the cultural form. Greek culture is characterized by the subjective demonstration of the beautiful. There are several specific manifestations of this social element, all of which emphasize the beauty of the individual. One is ornamentation. A fundamental aspect of Greek society, said Hegel, concerns the custom of enhancing one's physical aspect. Much of Greek social life involves the transformation of nature toward this end. One such type of transformation involves simply the fashioning of ornaments. Thus Hegel wrote: "Ornament by its very nature is destined only to beautify something other than itself, viz. the human body, which is man's immediate environment, and which, in common with Nature at large, he has to transform."[68] Greek social and cultural activity are strongly concerned with this demonstration of personal beauty: "Among the Greeks, then, we find this boundless impulse of individuals to *display themselves,* and to find their enjoyment in so doing."[69] This is, importantly, not a merely sensuous involvement with nature, but a celebration of human powers of aesthetic creativity. For, the Greeks are "too powerfully excited, too much bent upon developing their individuality, absolutely to adore Nature. . ."[70] Greek culture, then, takes the form of such social activities aimed at "self-assertion," and "the effort (of the Greeks) to dignify themselves."[71] This is further indicated by one of the more celebrated types of Greek social activity, the practice of engaging in physical contests. Games are, in Greece, examples of this same concern with subjective beauty. Though originally private events, "games and aesthetic displays . . . became an affair of the nation, and were fixed for certain times at appointed places. Besides the Olympic games in the sacred district of Elis, there were also held the Isthmian, the Rhythian, the Nemean, at other places."[72] These kinds of activity, representing fundamental aspects of Greek society, are properly understood only in terms of the type of consciousness that informs them. That is, the freedom of spirit or mind won by the Greeks, in which the human power of creation is activated, provides the clue to the true *meaning* of such aesthetic displays. In sport, wrote Hegel, "Nature is wrought into Spirit, and although in these contests the subject has not advanced to the highest grade of serious thought, yet in this exercise of his physical powers, man shows his Freedom, viz., that he has transformed his body into an organ of Spirit."[73]

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[60] See Hegel, *The Phenomenology of Mind,* pp. 224–240.

[61] Hegel, *The Philosophy of History,* p. 226.

[62] *Ibid.,* p. 238.

[63] *Ibid.,* p. 113.

[64] *Ibid.,* p. 238.

[65] *Ibid.,* p. 113.

[66] *Ibid.,* p. 114.

[67] *Ibid.,* p. 225–226.

[68] *Ibid.,* p. 241.

These cultural forms thus represent the subjective expression of Greek consciousness. They describe very human activities, day-to-day patterns of behavior influenced by socially constructed norms. In this regard, however, there is an ephemeral or mortal aspect to the "subjective work of art" that is not entirely characteristic of the Greek world. That is, Hegel found in Greece a coequal tendency, an instinct toward immortalization.

This did not precisely involve immortalizing the self. Self-immortalization, a seemingly appropriate corollary to the subjective realm, becomes truly applicable, said Hegel, only in the modern era. Indeed, one of the cornerstones of Hegel's "Germanic world" would be the divinization of man.[74] The Greek world does not exhibit this tendency. But there is in Greece a parallel element, a perhaps more primitive but nonetheless analogous attempt to objectify, hence to immortalize, spirit. This "objective work of art" finds its manifestation in Greek religion, and constitutes the proper explanation of the latter. The Greek divinities are, according to Hegel, spiritual divinities. They represent not nature, but aspects of human spirit or mind. More specifically, their form is not abstract or natural, but concrete and human. They have at their command the elements of nature, but are not themselves natural. In this regard, Hegel saw great significance in the War of Gods, and "the overthrow of the Titans by the race of Zeus."[75] Thus, for example, "Oceanus, as such, is only the element of nature which his name denotes. (On the other hand) Poseidon has still the wildness of that element in his character; but he is also an ethical personage. . . ."[76] Greek divinities are thus created as spiritual entities, "people" of a higher, immortal order. Hegel said that they "are to be regarded as individualities—not abstractions. . . . The gods are personalities, concrete individualities . . . special characters."[77] This fundamental aspect of the Greek gods provides the clue to understanding Greek religion. That they are vested with a predominantly spiritual element means that Greek religion is most basically the attempt to objectify the Greek development of spirit. Previous religions, those of the Oriental world, worshipped nature. Greek culture, on the other hand, creates a religion in which spiritual or human characteristics are immortalized. The more specific objects of worship, in fact, are concrete embodiments of the human form; "The Greek god . . . exists for his worshippers perenially *in the manifestation*—only in marble, in metal or wood, or as figured by the imagination."[78] Again, Greek religion is a less complete spiritual religion than Christianity. This is because spirit in the Greek epoch is at a less advanced stage than in the modern world. But the import of the analysis for our purposes is again to indicate Hegel's sociological approach. His explanation of Greek religion is that it is a human creation and, therefore, that the distinctive character of Greek consciousness results in a particular kind of divinity. The Greek spirit, as a partially free, creative and active spirit, produces a religion in which that very phase of *Geist* is the object of worship. In this way, Greek religion becomes sociologically comprehensible. Its specific pecularities and limitations can be understood as being derived from the nature of Greek spirit; and the meaning it has for its worshippers can be found in the assertion that the very act of worship is an act of creation and that every creator is, himself, an agent of spirit.

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[69] Hegel, *The Philosophy of History,* p. 242.

[70] *Ibid.*

[71] *Ibid.*

[72] *Ibid.*, p. 243.

[73] *Ibid.*

[74] See Hegel, *The Phenomenology of Mind,* pp. 758–764. See also, Robert Tucker, *Philosophy and Myth in Karl Marx* (Cambridge: Cambridge University Press, 1961); also Lowith, *From Hegel to Nietzsche,* pp. 47–50.

[75] Hegel, *The Philosophy of History,* p. 244.

[76] *Ibid.*, p. 245.

[77] *Ibid.*, p. 246.

[78] *Ibid.*, p. 249.

Thus, Greek religious life, like the cultural life, is to be understood in terms of the nature of spirit in Greece. The development of a certain level of spiritual creativity makes possible the constitution of social structures that, precisely as a consequence of this, are fundamentally spiritual. The two types of structures discussed so far, the objective and subjective, are largely distinct examples of this process. But the Greek world, said Hegel, is a socially unified world. These distinct elements are, he argued, given coherence and unity by virtue of a third, more essential social form.

This third social entity is the "Political Work of Art." Politics is the culmination of the Greek epoch, the synthesis of its various strains. Hegel's understanding of Greek politics is expressed at the outset with great clarity: "The State unites the two phases just considered, viz., the Subjective and Objective Work of Art. In the State, Spirit is not a mere Object, like the deities, nor, on the other hand, is it subjectively developed to a beautiful physique. It is here a living, universal Spirit, but which is at the same time the self-conscious Spirit of the individuals composing the community."[79] This quotation is revealing of the true place of Hegel's sociology. The last sentence contains two clauses. The first clause describes the Greek state as "a living, universal Spirit." Herein is manifested the most widely known aspect of Hegelian thought, the metaphysical. The common understanding of Hegel's concept of *Geist* is as a transcendent element, encompassing everything, marching through history, sweeping up mere mortals in its journey. While this is a valid interpretation, it is frequently forgotten that mere mortals are indeed the agents of and embodiments of spirit. It is by virtue of the capacities of human beings that spirit exists at all. For spirit, once again, is the distinctively human aspect of the world in which all men necessarily participate.

This basic principle informs Hegelian sociology. The second clause, above, describes the Greek state as "at the same time the self-conscious Spirit of the individuals composing the community." The role of individuals in forging social institutions is thus the focus of sociological inquiry. This assertion is not generally recognized. For example, in their well-known discussion of Max Weber, Hans Gerth and C. Wright Mills only see in Hegel (and also in Ranke and Dilthey) "the attempt to 'interpret' the individual person, intention, act or style of work by seeing it as a 'document', 'manifestation', or an 'expression' of a larger morphological unit that underlies particular data."[80] While this assertion is essentially correct in what it says, it ignores the other edge of the blade. That is, humans do not merely manifest spiritual reality. They in fact *comprise* it and, through their various mental capacities, give it life and energy. Thus, for example, the laws and customs that characterize the Greek polity should not be understood as necessary manifestations of some metaphysical or theological order. Rather, they are human constructs. Their meaning is to be ascertained by examining the point of view that informs their creation. This particular point of view means an approach to the world that is, by definition, unique. But its solutions to social problems are not pre-ordained; the specific formulations of Solon's codes, for example, are not strictly determined by the state of Greek consciousness. Rather, this state of consciousness means only that a certain *kind* of solution will be found; and, further, that those specific social structures that are developed can only be understood in terms of this process. That is, the analysis of Greek social forms requires reference to the spiritual element that informs their creation.

We now have sufficient materials to characterize the social science of Hegel. There are three essential components of his approach. The first may be generally called the historical aspect. Hegel's sociological analyses invariably commence by placing the subject matter in its unique historical context. This involves material as well as ideal elements. Geographic, economic, and demographic factors are given as much emphasis as are previous states of minds. (In this regard, the characterization of Hegel as an "idealist" is usually excessively simplistic.) These historical elements are crucial. Upon them are based Hegel's central attempt to reconstruct the development of consciousness.

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[79] Hegel, *The Philosophy of History*, p. 250.

[80] Hans Gerth and C. Wright Mills, *From Max Weber: Essays in Sociology* (New York: Oxford University, 1946), p. 56.

Thus, for example, the particular spirit of Greece is made possible by previous developments of consciousness in the Orient. But Greek consciousness is equally, and perhaps more directly, attributable to (for example) the migratory patterns and physical characteristics of ancient Attica.

The second component of Hegelian social science, then, involves the analysis of spirit or mind. The reconstruction of consciousness results in detailed descriptions of the elementary character of this aspect. Though informed by historical circumstances, these descriptions involve the ability of the investigator to find the "I in the Thou." Hegel, in effect, places himself in the Greek experience—in the diversity of Greek culture—and thus develops an intuition of the Greek state of mind.

This intuition is, in effect, justified by the analysis of social structures. These structures, comprising the third element in Hegel's social science, are assumed to be the products of spirit or mind. Their true character, then, is to be understood in those terms. That is, the peculiar aspects of these forms are to be interpreted by examining the states of mind that inform their creation. Clearly, the validity of such a procedure depends upon the credibility of the analysis. Thus, Hegel's explanation of Greek social forms in terms of spirit is powerful largely because it seems to make sense. It is coherent and not at all far-fetched. It certainly is something to be approached critically, for we ought not to be easily persuaded. But, unless and until some other contrary thesis proves more compelling, the analysis can be considered a significant addition to our understanding. And who can argue that Hegel's discussion of ancient Greece does not constitute a fundamental insight into the essence of that period?

It may be charged here that the three elements just discussed really constitute, in effect, a primitively positivist or "scientific" approach. That is, the historical element and the social element may be regarded as the overt manifestations from which is derived or inferred the middle element, consciousness. This charge is true only in so far as all social theory is considered scientific. Every serious investigator of society—whether philosopher, sociologist, historian or journalist—must marshall facts in support of his theses. This accumulation of facts, however, does not in itself constitute a truly positivist outlook in the manner described above. Again, two factors are relevant here: the kinds of facts considered acceptable and the type of justification deemed appropriate. Positivist social science—certainly since Durkheim—demands facts which are essentially unambiguous. Thus, the casting of a single vote for the Democratic party may be regarded simply as that vote. There may be considerable debate about why the vote was cast; but the vote itself—as a simple, discrete fact—is beyond (or beneath) interpretation. Behavioral or positivist social science seeks this kind of fact. Spatial patterns, group membership, SES, budgetary expenditures—all are examples of acceptable scientific data, distilled to the point where their truth is theoretically beyond question. Hegel's social theory, on the other hand, uses facts that are rarely so self-evidently factual. Thus, for example, Hegel found in Greece the existence of oracles which he considered part of the historical and cultural foundation upon which Greek consciousness developed.[81] But this fact, the fact that oracles existed, is not treated as a discrete, self-explanatory datum. Rather, the fact used by Hegel is clearly an interpreted fact. The oracles are not simply oracles, but certain kinds of oracles. Their very facticity contains crucial qualitative or conceptual attributes that the observer sought to understand. This he could not do merely by observing; such understanding required interpretation or "conceptual thought." Thus, the "data" of Hegel are in no way self-evident or quantifiable. The ideological, historical, cultural, aesthetic, and even natural facts he used are part and parcel of his interpretive approach. They are not solely manifest or positive; they contain elements that are accessible—not opaque—to reason.

Equally, the method of explanation used by Hegel is not positivist. The nature of Greek consciousness or spirit is not merely inferred from the "facts." It is, rather, reconstructed by the inquirer, by Hegel, from the kind of empathetic standpoint described by Dilthey. In effect, it could be argued that Hegel really adopted the posture of his subjects, examined the influence of previous states of mind, as transmitted through migratory history, and the physical factors, as exemplified by the very terrain of Greece, and conceived from these an attitude toward nature and an attitude toward other men that, together, sum up the "elementary character of the Spirit of the Greeks." This, then, contains little of the hypothetico-deductive model of positivism, with its emphasis on intersubjective testability and precision of measurement.[82] In this regard, it may seem that Hegel's method lacks the proper rigor of science. To this suggestion there are two possible rejoinders. First, the scholarship and thoroughness of Hegel's researches evidence the kind of breadth of knowledge, discipline and spirited rigor that is perhaps only rarely matched. Second, the scientific method is in itself no guarantee of rigor, as some of today's social science research indicates.

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[81] Hegel, *The Philosophy of History,* pages 235–237.

[82] See Herbert Feigl, "The Scientific Outlook: Naturalism *and* Humanism," in *Readings in the Philosophy of Science,* ed. Herbert Feigl and May Brodbeck (New York: Appleton-Century-Crofts, 1953), especially pp. 10–14.

Conclusion

It should now be apparent that Hegelian social science is properly in accord with the basic principles of Hegelian philosophy. Such principles as action, consciousness, and creation are basic to Hegel's epistemology and metaphysic, and also form between them the essential foci of his social scientific approach. Perhaps because of its profound theoretical foundations, this method has I think an unusual quality of timelessness and durability, an assertion that is in part justified by the continuing authority of several of Hegel's specific analyses. This durability makes the Hegelian method one that, at very least, ought to be reckoned with. By now listing certain key principles of that method I can both resummarize the essence of Hegelian social science and outline its potential relevance for contemporary theorists.

The first elementary principle is that the social world is, by definition, a human world. Society is not merely an entity that develops an independent life of its own, but is rather a collection of human wills. While the whole may in some way exceed the sum of its parts, the facts of human action, consciousness, and creation are never irrelevant. Thus, a purely functional-imperativist notion, such as that of Parsons, might according to the Hegelian conception actually ignore the crucial elements of social life.

From this principle comes a second, that the relevant social facts must be full-dimensioned facts. That is, the social researcher must go beyond those unambiguous data that present themselves, and seek a level of facticity that demands a degree of sympathetic interpretation. Thus, the positivist methods of data collection and inferential explanation are to be considered inadequate. The researcher ought rather to confront his world in a total way; he must "lose himself in the object" and obtain thereby a conceptual understanding of the appropriate complex social phenomena.

Such phenomena must, however, be of more than one kind. The third principle of Hegelian social science is the rejection of single-cause theories. Part of the researcher's intuition must involve a capacity to detect a wide variety of determinants. Hegel is often identified as advocating an idealist theory, as opposed to Marx's material or economic approach (an interpretation perhaps most attributable to Marx himself). As has been indicated, however, Hegel's sociological explanations draw upon a wide range of facts, both ideal and material. According to this approach, social forms are complex and multidimensional, dependent therefore on a multiplicity of factors.

The synthesis of these three principles results in the major theoretical insight of Hegelian sociology. By positing the importance of the human element in responding to and transforming a variety of ideal and material contexts of fact, Hegel views society as a human construct. It is an artifact that itself becomes a context, or a moment to be negated; a tool, that is, with which man can find order in the diversity of relationships. Man's relationship with nature and with other men is thus given momentary coherence by virtue of the social construction of social reality. This fourth principle of Hegel's sociology involves, inherently, a fifth. The social structures and forms are best understood as codifications of human experience. The various conditions of human life—spiritual and material—are given coherence and a degree of permanence in the patterning of social activity. This principle informs the specific social scientific analysis of Hegel. His method defines the structures of a particular society as the meaningful embodiments of human consciousness.

The pivotal role of consciousness makes an understanding of consciousness essential to any sociological analysis. The sixth and last principle of Hegelian society is that the elements of mind—thought, will, affect, reason—must ultimately become the researcher's main concern. Specific and particular social structures are only comprehensible in terms of their spiritual context. Moreover, true understanding of this realm must involve direct immersion in it. Again, positivist inference is inadequate to the task. The researcher must truly know his subject's state of mind, and must be able to identify with the point of view therein. It is most basically in this sense that nonpositivist methods of research become indispensable.

Several of these basic principles are reflected in the current "anti-behavioral revolution" which is occurring once again in the social sciences. As has been indicated, this movement involves a reaction to the purely positivist principles—an intellectual tendency that has again arisen to charm not only those who are perhaps born dissenters but also, more importantly, those who are skeptical about the possibility of developing a physics of society. Significantly, most contemporary nonpositivists are quite aware of their participation in what has here been described as an intellectual tradition. My main purpose has simply been to indicate the role of Hegel in this tradition and, by examining certain of his basic principles, to clarify further the elementary character of the interpretive approach to social analysis.

A Method of Scaling with Applications to the 1968 and 1972 Presidential Elections

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Introduction

The analysis of electoral behavior has seen a radical shift in emphasis over the last two decades. Empirically, there has been a major resurgence of interest in analysis of the relationship between issues and electoral behavior.[1] At approximately the same time, the theoretical literature has seen the development of the spatial model of party competition.[2] Based on a rational choice view of politics, this model perceives elections as a strategic contest between candidates who compete for votes by adopting positions in a multidimensional issue space. The further advancement of both the approaches has been hindered by the inability to obtain good empirically based measurements of the positions of the candidates and citizens in a common issue space.

Some recent literature along this line has attempted to estimate candidate and citizen positions by using only individual level *preference* data among candidates. There is a substantial body of psychological literature, culminating in the development of multidimensional proximity scaling methods,[3] which, on the basis of single-peakedness assumptions of individual preferences, provides means for estimating these positions. Despite the attractions of these methods, this approach has some serious drawbacks. Strong assumptions must be made about the nature of individual preferences, and only a relatively small number of all-inclusive and relatively uninterpretable dimensions can be recovered. In addition, by depending on preference rather than perceptual data, these methods end up *assuming* that voters have single-peaked preferences and vote for the candidate closest to them, rather than being able to *test* such assertions.

For the above reasons, we feel that operationalizations of tests of the spatial model have to be based on *perceptual,* as opposed to *preference* data. One method of collecting such data has been the straightforward procedure of simply asking respondents to place themselves as well as the candidates on a common issue continuum. These types of data have been collected, for example, in the 1968 and 1972 SRC election surveys in the form of "seven point scales." Here each respondent is asked to identify the positions of the major candidates and parties on a preselected set of issues. He identifies these perceptions, as well as his own "ideal point," by placing them somewhere on an equal interval scale running from 1 to 7 in which the two endpoints are identified.[4]

Unfortunately, much of the analysis of this type of data indicates that, in general, there is substantial disagreement between different individuals' perceptions of candidates, so it is not clear how to use such data to obtain representations of candidates and voters in a common space. Further, a natural interpretation of such data, and an interpretation that has been drawn by some political scientists is that voters simply don't have the necessary information to evaluate and intelligently vote their preferences in an election, as is assumed in the spatial theories.

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[1] See John H. Kessel, "Comment: The Issues in Issue Voting," *American Political Science Review,* 66 (June, 1972), 459–465, for an extensive bibliography of the literature.

[2] See Otto A. Davis, Melvin J. Hinich, and Peter C. Ordeshook, "An Expository Development of a Mathematical Model of the Electoral Process," *American Political Science Review,* 64 (June, 1970), 426–448, for a review of this literature.

[3] See Paul E. Green and Frank J. Carmone, *Multidimensional Scaling and Related Techniques in Marketing Analysis* (Boston: Allyn and Bacon, Inc., 1970) for a review of this literature. Applications of these methods to the 1968 and 1972 presidential elections can be found in Jerrold G. Rusk and Herbert F. Weisberg, "Perceptions of Presidential Candidates: Implications for Electoral Change," *Midwest Journal of Political Science,* 16 (August, 1972), 338–410, Herbert F. Weisberg and Jerrold G. Rusk, "Dimensions of Candidate Evaluation," *American Political Science Review,* 64 (December, 1970), 1167–1185, and Gary A. Mauser, "A Structural Approach to Predicting Patterns of Electoral Substitution," in *Multidimensional Scaling: Theory and Applications in the Behavioral Sciences, Vol. II, Applications,* ed. Roger N. Shepard et al. (New York: Seminar Press, 1972), pp. 245–287.

[4] These particular data have been analyzed by Benjamin I. Page and Richard A. Brody, "Policy Voting and the Electoral Process: The Vietnam War Issue," *American Political Science Review,* 66 (September 1972), 979–995, and by John H. Aldrich, "Some Results about the 1968 Election Based on the Theory of the Spatial Model of Party Competition" (paper delivered at the 1973 Annual Meetings of the American Political Science Association).

In this paper, we suggest an alternative interpretation of the above data, and argue that at least part of the confusion which has been attributed to the voter may be attributable purely to methodological difficulties inherent in collecting this type of perceptual data. We propose a model of the possible generation of such data which can be used to eliminate the errors attributable to these methodological difficulties. The same model serves as a scaling procedure which can be used to scale candidates and voters in a common issue space. This yields interval level data on candidate and voter positions which can be used to address various propositions from the spatial modeling literature.

The model we develop assumes that candidates occupy fixed positions in an issue space and that the individual perceptual data arises from this via a two-step process, the first step consisting of "true" error in perception, and the second step consisting of distortion introduced in the actual survey situation. We then derive a least squares solution for the true parameters of this model. The solution turns out to be essentially a principal components solution for the candidate parameters together with a regression estimate of the citizen parameters. We go on to evaluate, by Monte Carlo methods, the statistical properties of these estimators, in the type of situation to which they will be applied. Finally, we use the method to analyze the candidate and citizen positions on two issues in the 1968 and 1972 presidential elections.

The Problem

Before proceeding with the formal development of the model, we shall illustrate, in greater detail, the types of methodological difficulties that can be expected to arise in the analysis of individual level perceptual data of the sort described above. In particular, we consider an example of the type of data that might arise from the "seven-point" SRC scale on Vietnam.

In the example of Table 1, we have illustrated the possible perceptions of three voters. It is evident that although these three voters differ greatly in their placement of the candidates on the Vietnam scale, they seem to agree pretty well on the underlying scale on which the candidates lie. An alternative explanation for the lack of agreement of the voters on the placement of the candidates is that the voters are simply exhibiting different reactions to the response task. Convincing arguments can be made that this may be occurring at least to some extent. Thus, even though the endpoints of the issue scales are identified, these identifiers are rather vague, and their responses are subject to interpretation. Different voters may be anchoring the scales according to their own interpretation of these endpoints. The fact that voters are also asked to locate their own ideal points on the scale can only serve to accentuate this tendency, for a voter who is himself a hawk is likely to interpret the endpoints of the Vietnam scale in order to accommodate his own ideal point, thus pushing his perceptions of the candidates farther to the left than a dove would. In addition, and associated with the ambiguity of the endpoints, is the problem that different voters may well interpret the intervals on the scale differently. Again, it is reasonable to suspect, for example, that an extreme hawk might see less difference between Nixon and Humphrey than a moderate would. Finally, the forced categorization tends to have additional undesirable effects: Not only does one lose information by forcing voters to ignore small differences, but also voters tend to place their perceptions of candidates, as well as their placement of their own ideal points, more frequently in the "prominent" categories (i.e., 1, 4, and 7) rather than in the "off" categories (i.e., 2, 3, 5, and 6). This tendency leads to curious results when one attempts to analyze data. If one uses the raw data to observe the distribution of ideal points, for example, he or she observes what one of us has likened elsewhere to a "circus tent" effect, obtaining a distribution with modes at the prominent categories. Again, this gravitation of the respondent towards the prominent categories is usually interpreted as meaning that one is asking too much of the respondent—that the voter cannot make such fine distinctions—and one then proceeds to collapse the off categories, losing further information. Here, also, an alternative explanation might be that the gravitation towards the prominent categories is due to the ambiguity of the scale. Individuals use the prominent categories as "natural anchoring" points, but each individual gives his own interpretation to the prominent points.

If the above is an accurate account of the generation of the data, then data of this type would seem to contain contaminating information, in addition to the information they carry about the true candidate positions. For it is possible that there might be complete agreement in the perceptions of the candidates, but that because of different interpretations of the scale, we might be led to believe that there was

Table 1. Some Hypothetical Voters' Scores on Seven-Point Vietnam Scale

	"Dove"	1	2	3	4	5	6	7	"Hawk"
Voter 1		H,J,N			W			S	
Voter 2		H	J		N,S			W	
Voter 3		S		H	J,N			W	

Key:
H = Humphrey
J = Johnson
N = Nixon
W = Wallace
S = Self (i.e., Voter's ideal point)

little or no agreement. In fact, one would not expect that *all* of the variation in perceptions of the candidates would be accounted for by the above type of contamination, but one would like to be able to sort out what portion of the variance is due to actual variations in perceptions and what is due to variations in response to the scale. It is this question that we try to answer in the next section. We attempt there to factor out the variations due to differential response to the response task by placing all individuals in a common space such that their perceptions are most in agreement with the common perception of the candidates.

Formal Development of the Model

We assume that the candidates occupy true positions on an issue continuum, and that the information that the citizen gives us on his perception of the candidates is derived from this true position in a two step process. In the first stage, we assume that there is a random disturbance in the citizen's perception of the candidate. This error in perception could arise for several reasons. For example, it may occur because the candidate is unintentionally ambiguous about his position. It may occur because voters only obtain partial information from secondary sources who distort that information in the passing. It may also arise because the voters themselves selectively perceive and distort the information they receive so that it is consistent with their prior information. Whatever the cause, we assume that the first stage, which results in the voters' perceptual space, consists in the voter observing the true space, subject to this error.

The second stage consists of the voter taking what is in his head, i.e., his perceptions, and reporting them to the interviewer. Here, we assume, since there is no common metric for placing the candidates on a scale, that the positions where the citizen reports that he sees the candidates may be an arbitrary linear transformation of his perception of the space.

More formally, we develop the following model: We assume that there are J candidates who occupy the positions $Y_1, Y_2, \ldots, Y_J$ on a one-dimensional continuum, i.e., $Y_j \epsilon R$ for $1 \leqslant j \leqslant J$. Since this scale can only be specified up to a linear transformation, we assume that it is normalized with unit sum of squares, i.e.,

$$\sum_{j=1}^{J} Y_j = 0 \text{ and } \sum_{j=1}^{J} Y_j^2 = 1.$$

Further, we assume that there are n citizens, each of whom has a perception of each candidate. The i^{th} citizen's perception of the j^{th} candidate is denoted Y_{ij}, and we assume that this is distributed randomly around the true candidate position, as illustrated in Figure 1, for four candidates. Thus, for the first stage, we assume that individual perceptions are generated as follows:

$$Y_{ij} = Y_j + u_{ij} \tag{1}$$

for $1 \leqslant i \leqslant n$, $1 \leqslant j \leqslant J$,

where u_{ij} is a random variable which satisfies the usual Gauss Markov assumptions, i.e.,

$$\begin{aligned} &E\,(u_{ij}) = 0 \text{ for all } i, j \\ &E\,(u_{ij})^2 = \sigma^2 \text{ for all } i, j. \\ &E\,(u_{ij}\,u_{kl}) = 0 \text{ for all } i, j, k, l \text{ with} \\ &\quad \text{either } i \neq k \text{ or } j \neq l.^5 \end{aligned} \tag{2}$$

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[5] The scaling model we develop here rests on Gauss Markov type assumptions. There is good reason to question some of these assumptions, and hence attempt to extend the basic results to cover some of these potential violations. One assumption is that of "homoscedasticity" or constant error variance for each respondent and each candidate/stimulus. It may be more reasonable to expect some respondents to have greater perceptual error than others, and some stimuli to have lesser ambiguity in their positioning on issues than others. Another assumption is that of "no covariance" among an individual's perception of different candidates. It may be more realistic to assume that a respondent might identify certain candidates so that a change in perceived position of one candidate carries over to other candidates. A more general model incorporating these objections, would assume that there is a known stochastic variance/covariance matrix for each respondent:

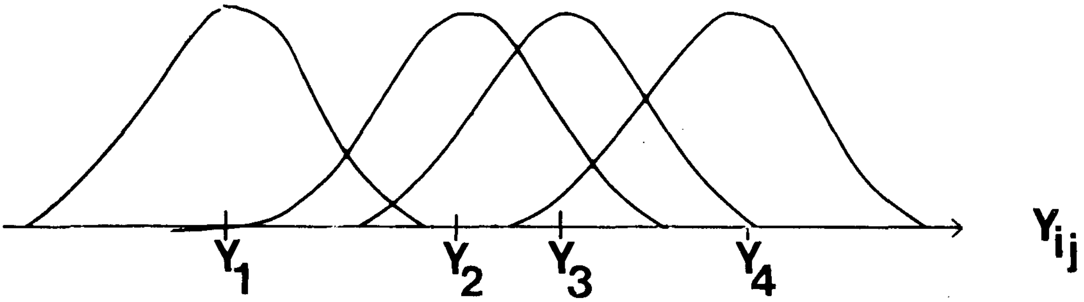

Figure 1. Distribution of Perceptions, Y_{ij}

The second stage of the data generation consists in the reporting of the perceptual space. Here we assume that our observed data consists of a reported position for each candidate, for each voter, and that this consists of some linear transformation of the voter's perceptual space. I.e., we let X_{ij} represent the position where citizen i reports that he sees candidate j, and it is assumed that for each voter, there are scalars, $c_i, w_i \epsilon R$ such that

$$c_i + w_iX_{ij} = Y_{ij} = Y_j + u_{ij} \tag{3}$$

for $1 \leqslant i \leqslant N$, $1 \leqslant j \leqslant J$, or equivalently,

$$X_{ij} = \frac{1}{w_i}(Y_{ij} - c_i) = \frac{1}{w_i}(Y_j - c_i) + \frac{u_{ij}}{w_i} \tag{4}$$

Note that the above transformation allows us to account for different anchoring of the scale as well as for different interpretation of the intervals. It does not account for the ordinal nature of the data.

Now, the only data we actually observe is the X_{ij} matrix of reported positions, and from this, we want to recover the true parameters, Y_j, c_i, and w_i for $1 \leqslant i \leqslant n$, $1 \leqslant j \leqslant J$. Before proceeding, we establish some matrix notation to make the calculations less burdensome. We set

$$Y = \begin{bmatrix} Y_1 \\ Y_2 \\ . \\ . \\ Y_J \end{bmatrix}, X_i = \begin{bmatrix} 1 & X_{il} \\ 1 & . \\ . & . \\ . & \\ 1 & X_{iJ} \end{bmatrix}, \beta_i = \begin{bmatrix} c_i \\ w_i \end{bmatrix}.$$

And, using $\hat{Y}_j$, $\hat{c}_i$, $\hat{w}_i$, etc., to denote our estimates of the true parameters, we set

$$\hat{Y} = \begin{bmatrix} \hat{Y}_1 \\ . \\ . \\ \hat{Y}_J \end{bmatrix}, \hat{\beta}_i = \begin{bmatrix} \hat{c}_i \\ \hat{w}_i \end{bmatrix}.$$

Then, the perceived candidate positions for citizen i are

$$X_i\hat{\beta}_i = \begin{bmatrix} \hat{c}_i + \hat{w}_iX_{i1} \\ . \\ . \\ . \\ \hat{c}_i + \hat{w}_iX_{iJ} \end{bmatrix}.$$

The procedure that we will use will be to choose $\hat{Y}$ and $\hat{\beta}_i$, for $1 \leqslant i \leqslant n$ in such a way as to get the best fit, in a least squares sense, between the estimated candidate positions and the citizen's perceptions of them. To do this, we define the vector of estimated residuals, for individual i, as

$$e_i = X_i\hat{\beta}_i - \hat{Y}. \tag{5}$$

Then, the sum of the squared residuals for a particular voter is

$$e_i'e_i = (X_i\hat{\beta}_i - \hat{Y})' (X_i\hat{\beta}_i - \hat{Y}), \tag{6}$$

and the total sum of squared residuals is

$$SS(\hat{\beta}_i, \ldots, \hat{\beta}_n, \hat{Y}) = \sum_1^n e_i' e_i. \tag{7}$$

We want to minimize (7) subject to the constraints that

$$\sum_{j=1}^{J} \hat{Y}_j = 0 \text{ and } \sum_{j=1}^{J} \hat{Y}_j^2 = 1. \tag{8}$$

so, if we let C be a J x 1 matrix of ones, i.e.,

$$E(u_i'u_j) = \begin{bmatrix} \sigma_{1i}^2 \; (\sigma_{1i}\sigma_{2i}) \ldots (\sigma_{1i}\sigma_{Ji}) \\ . \\ . \\ . \\ . \\ (\sigma_{Ji}\sigma_{1i}) \; (\sigma_{Ji}\sigma_{2i}) \ldots . \sigma_{Ji}^2 \end{bmatrix}$$

While we have formulated a model of the above sort, we have not yet been able to carry through the mathematical derivations of the estimators for this case.

$$C = \begin{bmatrix} 1 \\ \cdot \\ \cdot \\ \cdot \\ \cdot \\ 1 \end{bmatrix}$$

the constraints in (8) become

$$C'\hat{Y} = 0 \quad \text{and} \quad \hat{Y}'\hat{Y} = 1. \tag{9}$$

We set the above up as a Lagrangean multiplier problem, getting

$$L(\hat{\beta}_i, \hat{Y}, \lambda_1, \lambda_2) = \sum_{i=1}^{n} e_i'e_i + 2\lambda_1 C'\hat{Y} + \lambda_2(\hat{Y}'\hat{Y} - 1) = \sum_{i=1}^{n} (\hat{Y} - X_i\hat{\beta}_i)'(\hat{Y} - X_i\hat{\beta}_i) + 2\lambda_1 C'\hat{Y} + \lambda_2(\hat{Y}'\hat{Y} - 1). \tag{10}$$

Differentiating and setting equal to 0, we get the $2n + J + 2$ equations:

$$\frac{\partial L}{\partial \hat{\beta}_i} = -2X_i'\hat{Y} + 2X_i'X_i\hat{\beta}_i = 0, \text{ for } 1 \leqslant i \leqslant n \tag{11}$$

$$\frac{\partial L}{\partial \hat{Y}} = -2\sum_{i=1}^{n} X_i\hat{\beta}_i + 2n\hat{Y} + 2C\lambda_1 + 2\lambda_2\hat{Y} = 0 \tag{12}$$

$$\frac{\partial L}{\partial \lambda_1} = C'\hat{Y} = 0 \tag{13}$$

$$\frac{\partial L}{\partial \lambda_2} = \hat{Y}'\hat{Y} - 1 = 0. \tag{14}$$

Solving (11) for $\hat{\beta}_i$, we get

$$\hat{\beta}_i = (X_i'X_i)^{-1}X_i'\hat{Y}, \tag{15}$$

so that the individual transformation consists of the least-squares regression of the reported on the actual (unknown) positions of the candidates. Now, substituting in (12), gives,

$$\sum_{i=1}^{n} [X_i(X_i'X_i)^{-1}X_i']\hat{Y} - n\hat{Y} - C\lambda_1 - \hat{Y}\lambda_2 = 0. \tag{16}$$

Setting

$$A = [\sum_{i=1}^{n} X_i(X_i'X_i)^{-1}X_i'], \tag{17}$$

and substituting in (16) we can reduce equations (11) through (14), getting

$$(A - nI)\hat{Y} - C'\lambda_1 - \hat{Y}\lambda_2 = 0 \tag{18}$$

$$C'\hat{Y} = 0 \tag{19}$$

$$\hat{Y}'\hat{Y} = 1. \tag{20}$$

Multiplying (18) by C', we get

$$C'(A - nI)\hat{Y} - J\lambda_1 - (C'\hat{Y})\lambda_2 = 0. \tag{21}$$

But now it is straightforward, by expansion of X_i, to show that, for any $1 \leqslant i \leqslant n$

$$C'[X_i(X_i'X_i)^{-1}X_i'] = C',$$

so that

$$C'A = C'[\Sigma X_i(X_i'X_i)^{-1}X_i'] = nC'$$

and

$$C'(A - nI) = 0. \tag{22}$$

And from (19), it follows that $C'\hat{Y} = 0$, so (21) yields $J\lambda_1 = 0$, or

$$\lambda_1 = 0. \tag{23}$$

Now (18) becomes

$$(A - nI)\hat{Y} = \lambda_2\hat{Y}. \tag{24}$$

But this simply says that $\hat{Y}$ is a characteristic vector of the matrix $(A - nI)$, which gives us our solution. To determine which characteristic vector to choose, we note that $-\lambda_2$, the negative of the characteristic root, represents the sum of the squared errors associated with the characteristic vector $\hat{Y}$. To see this, we multiply (18) by $\hat{Y}$, getting

$$\hat{Y}'(A - nI)\hat{Y} - \hat{Y}'C\lambda_1 - \hat{Y}'\hat{Y}\lambda_2 = 0,$$

or, applying (19) and (20),

$$\lambda_2 = \hat{Y}'(A - nI)\hat{Y}. \tag{25}$$

But, setting

$$A_i = X_i(X_i'X_i)^{-1}X_i',$$

one can easily show that $(I - A_i)$ is a symmetric, idempotent matrix, and

$$\begin{aligned} \sum_{i=1}^{n} e_i'e_i &= \sum_{i=1}^{n} (\hat{Y} - X_i\hat{\beta}_i)'(\hat{Y} - X_i\hat{\beta}_i) \\ &= \sum_{i=1}^{n} (\hat{Y} - X_i(X_i'X_i)^{-1}X_i'\hat{Y})'(\hat{Y} - X_i(X_i'X_i)^{-1}X_i'\hat{Y}) \\ &= \sum_{i=1}^{n} \hat{Y}'(I - A_i)'(I - A_i)\hat{Y} \\ &= \sum_{i=1}^{n} \hat{Y}'(I - A_i)\hat{Y} = \hat{Y}'(nI - A)\hat{Y} \\ &= -\hat{Y}'(A - nI)\hat{Y}. \end{aligned}$$

So from (25),

$$-\lambda_2 = \sum_{i=1}^{n} e_i'e_i. \tag{26}$$

Thus, our solution, $\hat{Y}$, is the characteristic vector of the matrix $(A - nI)$ with the highest (negative) nonzero characteristic root. Having obtained a solution for the candidate positions, we can, of course, go back to (15), to obtain the parameters of the individual transformation by performing the least squares regression of the individual's reported positions on the estimated positions of the candidates.

With regard to the estimates of the individual perceptions, we note that $\hat{Y}_i = X_i\hat{\beta}_i$ is an estimate of the i^{th} voter's perceptions of the candidate positions. Ideally, we would hope that the average perception of a candidate's position would correspond to the estimate of his position. In vector notation, we would want

$$\frac{\Sigma\hat{Y}_i}{n} = \hat{Y}.$$

In fact, this is not the case. Rather, because of a "regression towards the mean" on individual's candidate perceptions, we get

$$\frac{\Sigma\hat{Y}_i}{n+\lambda_2} = \hat{Y} \tag{27}$$

To see this, we note that

$$\begin{aligned}\Sigma\hat{Y}_i &= \Sigma X_i\hat{\beta}_i\\ &= \Sigma X_i(X_i'X_i)^{-1}X_i'\hat{Y}\\ &= A\hat{Y}\\ &= (n+\lambda_2)\hat{Y},\end{aligned} \tag{28}$$

from which (27) follows.

Because of this relation, in our empirical applications, we will actually present the voter's perceptions of the candidate in terms of the expanded transformation of equation (27), which differs from the least squares estimators by a factor of $\frac{n}{n+\lambda_2}$. This has the effect of normalizing the solution with respect to the mean perceptions of the candidates, and makes possible more direct comparisons with the unscaled data.

Note that from (26), it follows that the expression

$$\frac{-\lambda_2}{nJ} = \frac{\sum_{i=1}^{n} e_i'e_i}{nJ} \tag{29}$$

represents the average squared deviations of the observed from the true candidate positions, and we can use this as an estimate of σ^2. Formally, we set

$$\hat{\sigma}^2 = \frac{-\lambda_2}{nJ}. \tag{30}$$

Since the estimated scale positions of the candidates are normalized to have unit sum of squares, $\hat{\sigma}^2$ can also be used as a measure of the "goodness of fit" of the model. Actually, we will see later that $\hat{\sigma}^2$ is generally a biased estimator of σ^2 providing a substantial underestimate of σ^2. This can be partially corrected for by computing the sum of the squared error in the expanded perceptual space described above. This results in the formula

$$\hat{\sigma}^2 = \frac{-\lambda_2}{nJ}\left(\frac{n}{n+\lambda_2}\right)^2 = -\frac{n\lambda_2}{J(n+\lambda_2)^2} \tag{31}$$

which is the formula we will actually use. Even with this adjustment there is substantial bias left, as we shall see. We have not yet been able to correct for this bias, however, and use σ^2 as defined above for the present, realizing that it must be interpreted cautiously.

Another point of caution regards the estimate of the individual transformations in (15). Note that no constraints are placed on $\hat{\beta}_i$. In particular, no constraint is placed on $\hat{w}_i$, so it is assumed that $\hat{w}_i$ could be negative for some voters. In applications to real data, this means that voters who perceive the candidates in a "mirror image" space will be estimated as having good fits to the true model, but with negative weights. A voter who sees things backwards then contributes to a better fit to the "true" space, and this accounts for some of the underestimation of σ^2 mentioned above.

Given that the endpoints of the scales are identified in the empirical data to which we actually apply the techniques, it is not clear that one would want to treat such voters as we have above. It would be more reasonable, perhaps, to assume that the parity of the scale is given, and that any misperception of this parity is due to error in perception. In terms of the model, this would correspond to an additional set of constraints, i.e., that $\hat{w}_i > 0$ for all voters. We have not done this for several reasons. First, the additional mathematical complexities which are introduced by this modification are substantial. Second, although the problem of negative weights is serious if one only has a small number of candidates, it should be less so as the number of candidates increases. Thus, with large numbers of candidates, the probability of obtaining a mirror-image set of observations purely by chance becomes smaller. Finally, the procedure we have developed above at least has the virtue of identifying the voters with negative weights so that one can treat them separately if need be.

All of the above analysis has dealt with scaling of the candidate positions. We have not yet discussed the treatment of individual ideal points, but this procedure is straightforward. To obtain the individual's ideal point in the common space, we merely subject it to the same transformation that his perceptions of the candidates are subjected to. Thus, if X_{i0} represents the i^{th} individual's placement of his ideal point, then

$$\hat{Y}_{i0} = \hat{c}_i + \hat{w}_i X_{i0} \tag{32}$$

is our estimate of his ideal point in the common space.

Before proceeding, it will be worthwhile to point out some similarities between the specification of the model that we have developed in this section and the usual factor analytic model. For these purposes, we set

$$X = \begin{bmatrix} X_{11} \ldots X_{1J} \\ \cdot \\ \cdot \\ \cdot \\ X_{n1} \ldots X_{nJ} \end{bmatrix}, A = \begin{bmatrix} a_1 \\ \cdot \\ \cdot \\ \cdot \\ a_n \end{bmatrix}$$

$$F = \begin{bmatrix} F_1 \ldots F_n \end{bmatrix}$$

$$D = \begin{bmatrix} d_1 & & 0 \\ & \ddots & \\ 0 & & d_n \end{bmatrix}, \text{and } U = \begin{bmatrix} U_{11} \ldots U_{1J} \\ \cdot \\ \cdot \\ \cdot \\ U_{n1} \ldots U_{nJ} \end{bmatrix} \tag{33}$$

Here, X represents the matrix of observed data, where the X_{ij} are defined above. F represents a common factor of scale positions (similar to Y above), A is a vector of individual transformations, D a diagonal matrix of scalars, and U a matrix of errors.[6] Then the usual one-factor model can be written

$$X = AF + DU \tag{34}$$

or

$$X_{ij} = a_i F_j + d_i u_{ij},$$

which can be compared to (4) to note the similarities. The differences are that in (34), only stretching and shrinking of the original space is allowed. More important, however, the factor model generally treats F as a random variable rather than as a parameter to be estimated.[7] Hence, although one can obtain estimates of the factor scores, one does not obtain sampling distributions of these estimates. Since the candidate positions are of primary interest, we are particularly concerned about the accuracy with which they are recovered, and we would want a model that treats them as parameters rather than random variates.

In addition to the differences in the specification of the model, one should note that if the factor-analytic formulation above is used, computational problems arise in applying usual factor-analytic procedures for obtaining a solution because the usual roles of the observation and the variable are reversed. Thus a variable, under this representation, is a respondent, while an observation is the vector of individual perceptions of a particular candidate. Because of this reversal of roles, one would end up factor analyzing a matrix of, say, four observations and 1000 variables, leading to unmanageable correlation matrices.

Despite the differences between the two formulations, it can be shown that the solution we have derived above for the candidate parameters is mathematically equivalent to extracting the first principal component of the correlation matrix XX'.

Monte Carlo Results

The last section has derived a least-squares solution for our scaling problem. In order to assess the adequacy of the solution, and its performance in a given situation, however, we should know something about the statistical properties of the estimators. Thus, unless we know the theoretical sampling distributions of the estimators, it is difficult to know how much confidence to place in the results. Since our estimators have been derived in the rather complex manner described above, analytical determination of their sampling distributions is exceptionally difficult. Although we cannot obtain mathematical derivations of these distributions, we can obtain an indication of their properties by conducting Monte Carlo type experiments.

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[6] See Harry H. Harman, *Modern Factor Analysis* (Chicago: The University of Chicago Press, 1967), for a more complete development of the factor-analytic model.

[7] See e.g., D. N. Lawley and A. E. Maxwell, *Factor Analysis as a Statistical Method* (London: Butterworths, 1963) for a discussion of this point.

The purpose of a Monte Carlo experiment is to generate artificial data according to a specified probabilistic model. In our case, we can specify true parameters and a stochastic term of known size and then generate data according to the model of the previous section to see how well the technique recovers these parameters. Typically, we choose, as known parameters, ones that will be as realistic as possible. To do so, we have used real data estimates to serve as parameters, and in particular, we have used those determined from the 1968 Vietnam scale data to be discussed below. Therefore, we hope to investigate the adequacy of the technique in the sort of situation we will be faced with in the real data. The true positions of the stimuli were chosen to correspond to the estimated positions of Johnson, Humphrey, Nixon, and Wallace, respectively, and the variance, σ^2, of the perceptions around the true positions was assumed to be equal to the largest variance in the estimated solution. Thus, we have $\sigma = .388$, or $\sigma^2 = .1505$. (This value of σ is actually somewhat different from the results reported below, because the Monte Carlo experiment was based on a preliminary estimation of the 1968 data.) Finally, the individual parameters, c_i and w_i, were chosen by taking a random sample of the 1968 respondents, and using their estimated values, $\hat{c}_i$ and $\hat{w}_i$ for the Monte Carlo experiment. Using these "true" parameters, we generated 25 samples with an n of 100 each, according to the model described in the previous section.

The results of the estimation of the parameters is reported in Table 2, and we are led to conclude that all the parameters describing the candidate positions are recovered exceptionally well. Figure 2 illustrates the distribution of perceptions in the assumed true model, while Figure 3 illustrates the sampling distributions of the estimated candidate positions. We see that even with a substantial amount of misperception in the original data, the technique recovers the candidate positions very well. The average correlation between the 25 estimated candidate vectors and the true candidate vector was .9977, with the average estimate of each candidate being nearly identical to the true position of the candidate. The mean estimate for each candidate is well within one standard deviation of the true position; thus if there is any bias in these estimators, it is insignificant in relation to their standard error.

The estimators for the variance in perception show a slightly different story. Here we get an overall estimate of σ of .2845, a significant underestimate of the true parameter, $\sigma = .388$. The estimates of the error in perceptions around each candidate are similar in magnitude and in their negative bias. This negative bias reveals one of the potential drawbacks of the technique, i.e., since the least-squares procedure attributes as much of the error as possible to variations in reactions to the scale, the procedure cannot recognize someone who actually perceives all candidates (say) to the left of their true positions. Such a voter is seen as perceiving the space correctly; and consequently, we underestimate to a certain extent the variance in perceptions. Similarly, as discussed in the previous section, voters with negative weights contribute to underestimation of σ. Both of these sorts of underestimation should become less severe as the number of candidates increases.

Turning to the individual parameters, we now must view the experiment as 100 individual parameter pairs, each being estimated by 25 observations, or estimates. Rather than presenting the estimated sampling distribution of each of these 200 estimators, we present, in Table 3, some summary statistics of these sampling distributions across all 100 voters. The first set of figures gives the distribution of the true parameters, so that we have a base against which to evaluate the estimators. The next two statistics give an indication of the average bias of the estimators. First is a measure of the average (signed) bias, and the fact that these are so close to zero indicates that there is no consistent bias in one direction or the other across the 100 estimators. The second statistic is the root mean-square of the bias over all 100 estimators. This gives a better indication of the average magnitude of the bias. The third figure gives the average standard error of the 100 estimators. These indicate that the bias is generally insignificant in comparison with the standard error of the estimators. Unlike the candidate estimates, however, the standard error is fairly large. To get an idea of the amount of error that is represented in the estimators of the c_i and w_i, one can compare these figures (the average standard error figures) with the standard deviations of the distributions of the true c_i and w_i. The average standard error figures are on the order of half the size of these standard deviations. This means that although these estimators may perform well on the average, in any given sample there can be a substantial amount of error in the estimation of a particular voter's transformation parameters.

It is difficult to assess the seriousness of the error in the individual estimates when they are expressed in the above form. This is because we are not really interested in the transformation parameters themselves, but are interested in them so that we can determine how accurate is the recovery of arbitrary points on the individu-

Summary of Monte Carlo Experiment

Distribution of Estimated Parameters
(In 25 samples, n = 100)

Table 2. Distribution of Candidate Estimators

		Candidate Parameters		Citizen Perceptions	
	True Parameters	Mean	Standard Deviation	Mean σ_j	Standard Deviation σ_j
Y_1	–.321	–.320	.040	.289	.021
Y_2	–.424	–.422	.035	.290	.020
Y_3	–.096	–.096	.036	.304	.013
Y_4	.841	.840	.010	.253	.021
σ	.388	.285	.013		

Average correlation of Y_j with $\hat{Y}_j$ = .9977 (over 25 samples)
Average correlation of Y_{ij} with $\hat{Y}_j$ = .868
Correlation of Y_j with $\hat{Y}_j$ = .999997.

Table 3. Distribution of Voter Transformation Parameters and Ideal Points

		c_i	w_i	Y_{i0}
True Distribution	Mean	–.1544	.368	–.209
	Standard Deviation	(2.242)	(.525)	1.757
Estimators	Average Bias	–.0897	.0217	–.189
		(.356)	(.083)	(1.376)
	RMS Bias	.1338	.0072	
	Average Standard	1.080	.2511	.486
		(.865)	(.279)	(.422)

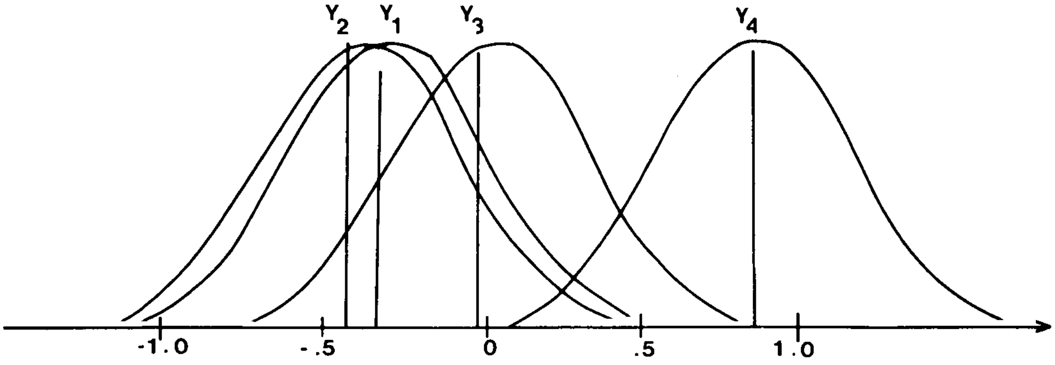

Figure 2. Assumed True Model (Distribution of Perceptions)

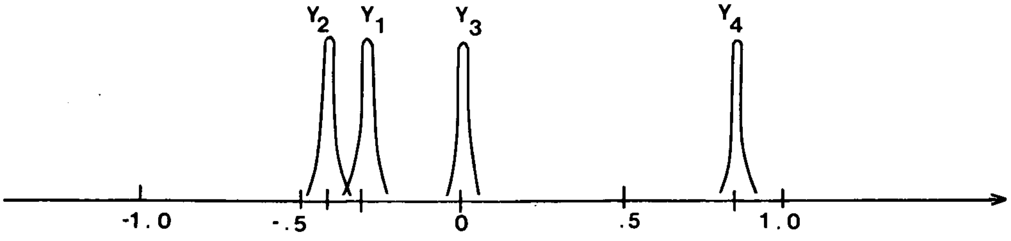

Figure 3. Estimated Model (Sampling Distribution of Candidate Estimators)

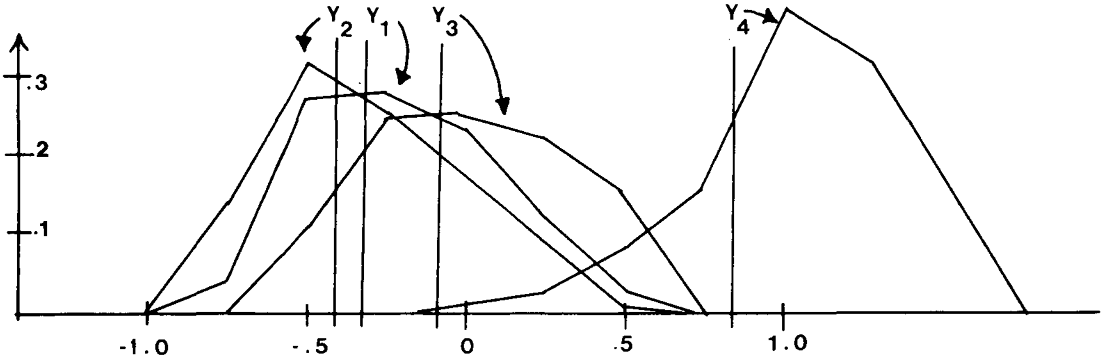

Figure 4. Estimated Model (Recovered Distribution of Perceptions)

al's perceptual scale. In particular, in our applications below, we will be interested in how well we can recover the individual's ideal point. To investigate this problem we assume that each of the 100 voters has an ideal point Y_{i0}, which consists of a point in his perceptual space. Unlike his perceptions of the candidates, this is not subject to error. We then want to discover how well, for each of the 100 voters, we recover the ideal point over the course of the 25 samples. As above, we should end up with 100 estimators of 100 true parameters (in this case the Y_{i0}), each estimator based on a sample size of 25.

Just as the c_i and w_i were taken as the estimated values from an application to the 1968 Vietnam scale, the Y_{i0} were determined by setting $Y_{i0} = c_i + w_i X_{i0}$, where X_{i0} is the reported ideal point of the i^{th} voter and c_i and w_i his estimated coefficients in the 1968 application. This computation should result in a distribution of true values much like that which we would expect to find in the actual applications. For each of the 100 voters, then, we get an estimator, $Y_{i0} = c_i + w_i X_{i0}$, for which we have 25 observations, and it is this estimator that interests us. In Table 3, we have presented information on the distributions of these estimators. We note that the average standard error of these estimators is .486, which is substantial, but comparing it to the distribution of the true ideal points, with a standard deviation of 1.757, we conclude that we can tell at least in what general area of the distribution the citizen's ideal point falls. Another way of looking at this result is to note that a 95 per cent confidence interval for an individual ideal point will be about one-half of a standard deviation either way of the estimated value. Although the amount of error may appear to be substantial, each of the individual ideal point estimates is based on a regression on four data points, and one might consider it surprising that the error is not larger than it is.

These estimates do seem to reflect a systematic bias. Points close to the mean of the true distribution are recovered with relatively greater accuracy than are those at the extremes, as reflected by the correlation of .135 between the standard error and the true value squared, and by the correlation of −.447 between the bias and the true value squared. Thus, there is a larger bias toward the mean and a larger standard error in the estimates of extremist ideal points, leading to a greater total mean squared error for these voters. This "regression toward the mean" suggests that a good portion of the above error in the individual estimators may be accounted for by the extremists. From our point of view, i.e., for predicting voting behavior, we are more interested in getting good estimates of moderate ideal points than of extremists, for it is the moderate whose vote is most difficult to predict. Thus, the bias here works in our favor, giving us better estimators where we need them. As a final note, the "regression toward the mean" implies that extremists will be estimated as somewhat less extreme than they actually are. Undoubtedly this same phenomenon accounts for the skewing of the distributions of the perceptions of a candidate, as observed in Figure 4 above.

This section is rather brief, since the important findings are best presented in tabular form. This brevity does not mean that the results obtained are unimportant. On the contrary, they strongly indicate the scaling approach developed in the previous section will yield reasonably accurate estimates of the unknowns, provided that the model underlying the technique fairly describes the data being scaled. The estimators of the candidate parameters are remarkably accurate, the only deficiency being in an underestimation of the variance in individual perceptions. The recovery of the individual parameters is considerably less accurate, but there is very little bias in these estimates, and one can expect that in large samples, the method will recover these parameters well on the average. With these results in mind, we can proceed to apply the technique to some real data.

Empirical Results—Introduction

The scaling procedure we have developed provides a solution to a general scaling problem. In this section we apply this technique to some electoral data, the type of problem that catalyzed our interest in the scaling procedure in the first place. The basic data will be the two seven point issue scales concerning urban unrest and Vietnam that were asked in the SRC's 1968 election survey and again in 1972. Following a description of the data, we will explain the candidate position estimates and the remaining variance of individual perceptions of these issues in the two very different elections. We will use the 1968 results to define a two-dimensional issue space in which we can locate both candidates and citizens. This space can be used to predict voting behavior and to demonstrate the improvement in such predictions from using the scaling results instead of the unscaled seven-point issue data. Unfortunately, the urban unrest scale was given to only a random half sample in the 1972 survey. Therefore, we are not able to relate the 1972 scaling results to the vote. Finally, we briefly report the scaling estimates for the 10 seven point issue scales that were asked of the whole sample in 1972.

The respondents sampled in 1968 were asked to locate themselves and four "candidates," Humphrey, Nixon, Wallace, and Johnson on the two dimensions. We limit our attention to those respondents who placed all four candidates and themselves on both scales and who reported their voting behavior. Further we remove any citizen who placed all candidates on the same point on an issue, because these individual parameter estimates are undefined. For these "no-variance" people, the effect would be the same if we assigned them w_i parameter values of 0, since all candidates were seen to take the same position on that issue. These restrictions leave us with an n of 885, or about 64 per cent of the 1384 respondents asked the questions.[8] Citizens were included in the 1972 scalings if they placed themselves and all five "candidates"—McGovern, Nixon, Wallace and the two political parties—on the seven-point scale and saw at least some variance in the candidates' positions on that issue. The sample sizes are 1045 for the Vietnam scale and 519 for urban unrest. While the term "candidates" is broadly defined in these examples, it is clear that the "candidate" stimuli are all relevant to the elections and to these particular issue dimensions.

Candidate Position Estimates

The scaling estimates of the candidates' positions, the $\hat{Y}_j$, are presented in Table 4 along with the standard deviation of the citizens' perceptions of each candidate, the $\hat{Y}_{ij}$. Note that the mean of the distribution of $\hat{Y}_{ij}$ is equal to $\hat{Y}_j$. Included as well are the mean and standard deviation of the estimated distributions of citizens' ideal points (denoted by "I" in the Table). The reader should keep in mind that it is not meaningful to compare candidate positions among dimensions, even for the 1968 estimates which are based on the same sample of citizens. The "unit of measurement" of each dimension has been arbitrarily set to have a mean of zero and unity sum of squares of candidate positions, reflecting our assumption

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[8] The issue scales were asked on the postelection wave of the 1968 survey. Only 1384 individuals responded to this wave, down from an original N of 1557 in the initial, self-weighting, cross-sectional sample. Fifty citizens saw no differences between the candidates on Vietnam, while 14 (including 7 of the original 50) were no variance respondents on urban unrest.

Table 4. Scaled Estimates of Candidate Positions on Vietnam and Urban Unrest

1968	**LBJ**	**HHH**	**RMN**	**GCW**	**I**	
Vietnam	–.321	–.424	–.096	.841	–.238	
(Standard deviation)	(.208)	(.302)	(.401)	(.403)	(1.033)	
Urban Unrest	–.394	–.402	–.003	817	–.131	
	(.239)	(.249)	(.327)	(.276)	(.645)	
1972	**RMN**	**McG**	**GCW**	**Dem.**	**Rep.**	**I**
Vietnam	.361	–.705	.376	–.355	.326	.046
	(.290)	(.338)	(.430)	(.327)	(.277)	(.694)
Urban Unrest	.180	–.602	.670	–.374	.126	.243
	(.348)	(.446)	(.500)	(.363)	(.329)	(1.164)

that the dimensions are unique only up to a positive linear transformation. We will be able to make some cross-dimensional comparisons when we consider the individual's ideal point distributions.

The scaling results indicate that Wallace was distinctly the most conservative candidate on these two important issues in 1968. While Humphrey was the most liberal on urban unrest and dovish on Vietnam, the distance between him and the remaining two candidates is much less than that separating Wallace and Nixon.

In 1972, Wallace is once again the most right/hawkish candidate. In this election, however, Wallace and Nixon are estimated to be very similarly hawkish, while a relatively large distance remains between these two candidates on urban unrest. Balancing Wallace on the right and more, McGovern appears to be very liberal on both dimensions, relative to the positions of the other candidates. The Democratic party appears to be relatively liberal on the two issues. Nonetheless, there is a discernible gap between this party and its nominee, the party appearing more moderate. The same is not true of the competing party. The Republican party is very close to, but somewhat more liberal than, the President on both dimensions. Perhaps the most notable characteristic of these placements is the consistency of at least the ordinal properties of the four scales (we will find some examples where the ordinality is violated for the other 1972 dimensions). This ordinal consistency supports the notion that there might be a single dimension underlying the two issues in each election. The interval placements, however, are not completely consistent with this view (consider the relative placements of Nixon and Wallace in 1972).

Perceptual Variation

We will return to the consideration of the candidate point-estimations later, after we have investigated the distributions of perceptions and citizens' ideal points. The overall variance to perceptions in the scaled data, $\hat{\sigma}^2$, was defined in equation (31) of Section 3, and is reported in Table 5. These figures can be used as an indication of the overall "goodness of fit" of the model and data. An alternative way of looking at the "goodness of fit" is to provide a benchmark basis of comparison to indicate the amount of reduction of the variance of the scaled over the unscaled data. To make this comparison, we normalized the seven-point scale data so that the average perceived candidate position on the seven-point scales has the same mean of zero and sum of squares of one as the scaled estimates of the candidates' positions. This restandardization of the seven point scale data leads to an average variance to perceptions that can be compared with $\hat{\sigma}^2$. The ratio of $\hat{\sigma}^2$ to the average variance just described gives an indication of the reduction of variance of perceptions accomplished by the scaling technique. These figures, also found in Table 5, indicate substantial reductions in variance for all four dimensions. These range from about 31 per cent of the variance in the original data for the 1972 Vietnam scale to only 7 per cent for the 1968 Vietnam issue. Actually, in light of our Monte Carlo results, $\hat{\sigma}^2$ is probably an underestimate of the true stochastic component to perceptions. But even allowing for considerable bias in these estimators, it is clear that the scaling has effected a considerable reduction of the variance in perceptions. Our original suspicions seem to be confirmed: a substantial portion of the observed variance in perceptions seems to be due simply to different reactions to the interview response task, and not all variance is due to error in perceptions.

Figure 5 and Figure 6 graph the individual distributions of perceptions of each candidate, (the $\hat{Y}_{ij}$'s), on the four dimensions.[9] The first observation that strikes one is the general similarity of these distributions to those derived

Table 5. Goodness-of-Fit Measures for Scaling Solutions

	$\hat{\sigma}^2$	Average Var. 7 Point Scale Data (0,1)	Ratio of $\hat{\sigma}^2$/Average Var.	% Negative Weights
1968				
Urban Unrest	.075	.285	.264	7.5%
Vietnam	.127	1.913	.066	21.3
1972				
Urban Unrest	.162	.607	.267	7.8
Vietnam	.113	.362	.313	14.5

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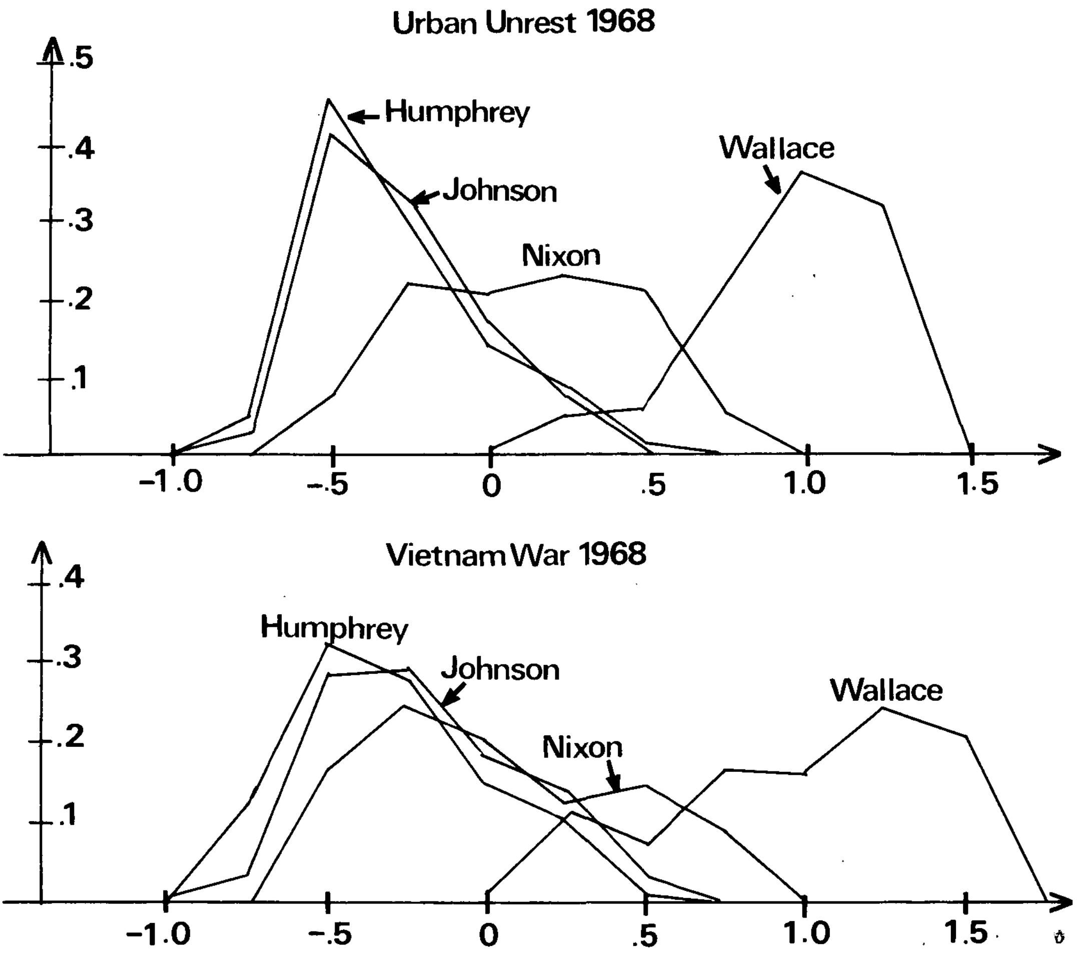
[9] These graphs were drawn to scale by determining the individual frequency distributions of Y_j. The scaling dimension was divided into 16 categories, each spanning a range of .25, and the proportion of Y_j in each category determined. This is the same procedure as used in the Monte Carlo experiment.


Urban Unrest 1968

Vietnam War 1968

Figure 5. Distribution of Scaled Perceptions of Candidates in 1968

from the Monte Carlo experiment. In particular, there seems to be a similar skewing of the distribution of the more extreme candidates, indicative perhaps of a "regression toward the mean" phenomenon. Second, there seems to be a greater clarity of the perceptual distributions for the urban unrest dimension in 1968 and Vietnam in 1972. This relative clarity seems most reasonable in the contexts of the two elections. In contrast, the urban unrest dimension in 1972 in particular is rather jumbled at best. In fact, the two extreme candidates, McGovern and Wallace, have perceptual distributions with long tails toward the center of the candidate distribution and beyond, which overlap with each other and seem rather clearly not unimodal. Large portions of the sample, however, make a distinction between these two extreme candidates—a distinction that had been only slightly clearer in the 1968 version of this issue. While the perceptions of Wallace are rather distinct on Vietnam in 1968, the other three distributions are nearly identical, and even the Wallace distribution has considerable overlap with the other three. If, as Page and Brody (1972) have argued, Humphrey and Nixon (and they argue perhaps even Wallace) were purposely vague on this issue, they certainly succeeded in confusing their Vietnam policy proposals with each other's in the minds of the electorate. This finding contrasts with the relative clarity with which the Y_{ij} distributions of Nixon and McGovern are distinguished in 1972 on the Vietnam issue. The much larger overlap of the perceptual distributions of Nixon and Wallace on both issues (but especially Vietnam) in 1972 than in 1968 is noteworthy. We will shortly indicate whether this change was due to Wallace's being perceived as less conservative in 1972, or to Nixon's being viewed as more conservative. Finally, the central location and relatively large dispersal of the

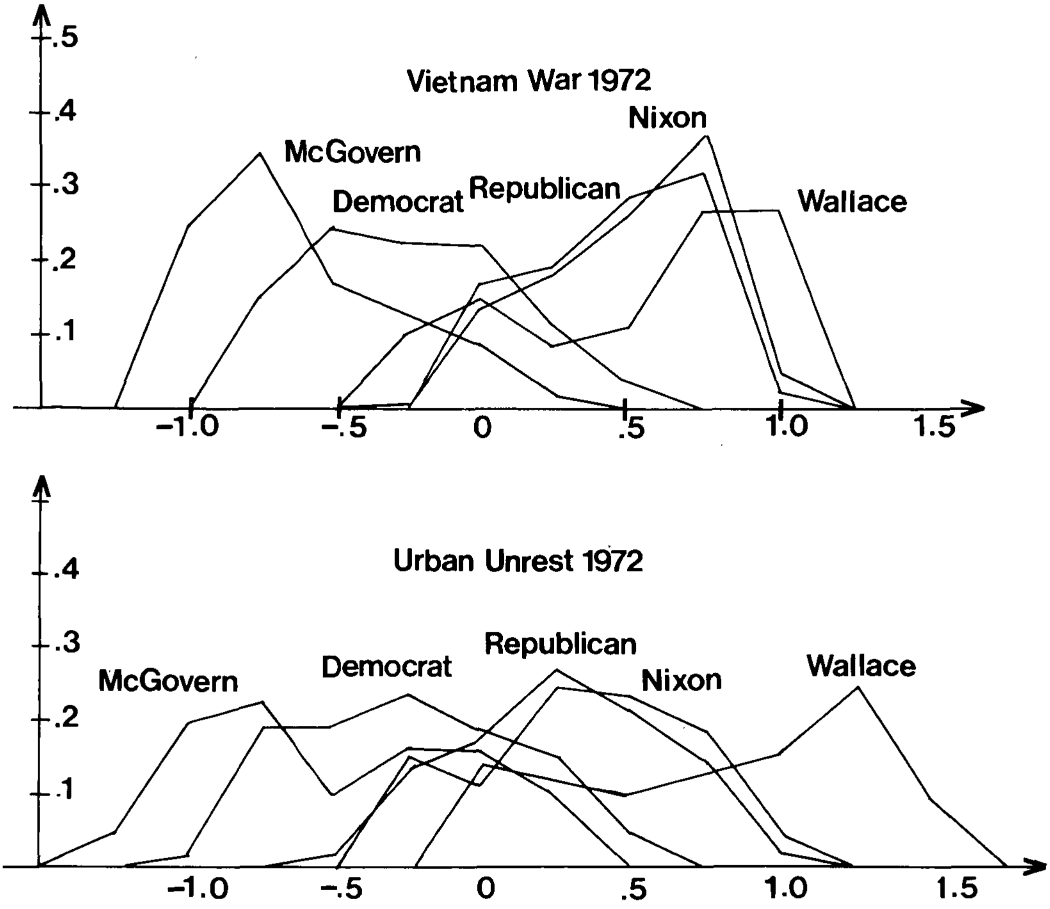


Figure 6. Distribution of Scaled Perceptions of Candidates, 1972

distributions of perceptions of Nixon in 1968 support the argument of calculated ambiguity on his part.

The entire set of candidate position estimates seems quite reasonable and helps to confirm the use of the scaling procedure for all four issues. The more refined analysis, particularly of Y_{ij}, further strengthens the argument for the "reasonableness" of the scaling estimations in our opinion. All indications, especially the distribution of perceptions, seem to point toward a greater clarity of candidate positions, and quite possibly a set of scaling estimates of higher quality, on the issues of urban unrest in 1968 and Vietnam in 1972 than on the other two dimensions. Our reading of the two elections leads us to believe that these two issues were discussed at length in the elections and that the major candidates took clear and distinguishable positions on them. This was not so obviously the case for Vietnam in 1968, when candidates were less clear in their statements, and for urban unrest in 1972, when the extent of discussion of the issue was lower and the immediacy of the riots present in 1968 was four years removed by 1972. These expectations are clearly supported by the scaling estimated distributions.

The argument of the higher quality of the scaling estimations of the issues of urban unrest in 1968 and Vietnam in 1972 receives additional support from the proportion of negative "weighting" parameters, w_i, estimated. Recall that these negative weights indicate that the citizen's perceptions can best be "fit" with the candidate position estimates by a *negative* linear transformation. In 1968, a quite small number, less than 8 per cent are estimated for the urban unrest issue (see Table 5), as is also the case for the Vietnam issue dimension in 1972. However, 14.5 per cent are estimated for urban unrest in 1972 and an unfortunately large 21.3 per cent are estimated for the 1968 Vietnam dimension. Taking the later case for example, to a fairly large extent, citizens who responded by perceiving themselves as hawks have become transformed to doves and vice versa. Perhaps the most typical case would be a hawkish respondent who claimed that Wallace is the most dovish candidate, while Humphrey or Johnson was most hawkish. Such an individual is likely to have a negative w_i and to have perceptions that correlate highly (but negatively) with the estimated candidate positions. The effect of the scaling estimations on such an individual is to treat him as if he were a dove, placing his estimated ideal point closest to Humphrey and Johnson. Thus, while it reverses his stated position, it places him closest to those candidates he claims to perceive as being closest to him.

Our interest in the individual parameter estimates, c_i and w_i, is primarily in their use in estimating the citizen's ideal point location on the set of common dimensions. The mean and variance of the distribution of ideal points for each dimension is found in Table 4. While we will be shortly putting the two 1968 distributions together to determine the distribution of citizens in the two dimensional issue space and relating their and the candidates' positions to the citizens' voting behavior, we can also use the aggregate distributions to make some cross-dimensional comparisons. The results here can only be considered tentative, especially for the 1972 comparisons which are based on nonidentical sets of respondents. With this in mind, however, we can use the variance of ideal point distributions on each dimension to determine the relative dispersion of candidate positions on them. In particular, the estimated dimensions are unique only up to a positive linear transformation. At present, the "unit of measurement" is determined by setting the distribution of candidate positions at the "(0,1)" standardization. Alternatively, we can linearly transform each dimension with respect to the distribution of ideal points so that all four have the same mean (say zero) and variance (in this case, we set all dimensions by the ideal point distribution of *urban unrest* in 1968). Therefore, all ideal point variances are set equal to .416. The new transformation of candidate position estimates would then represent the position of the candidate relative to the distribution of citizen's ideal points. Assuming that the distribution of ideal points remains constant from 1968 to 1972, we can then not only make cross dimensional comparisons within a given year but also look at the movement of candidates between elections.

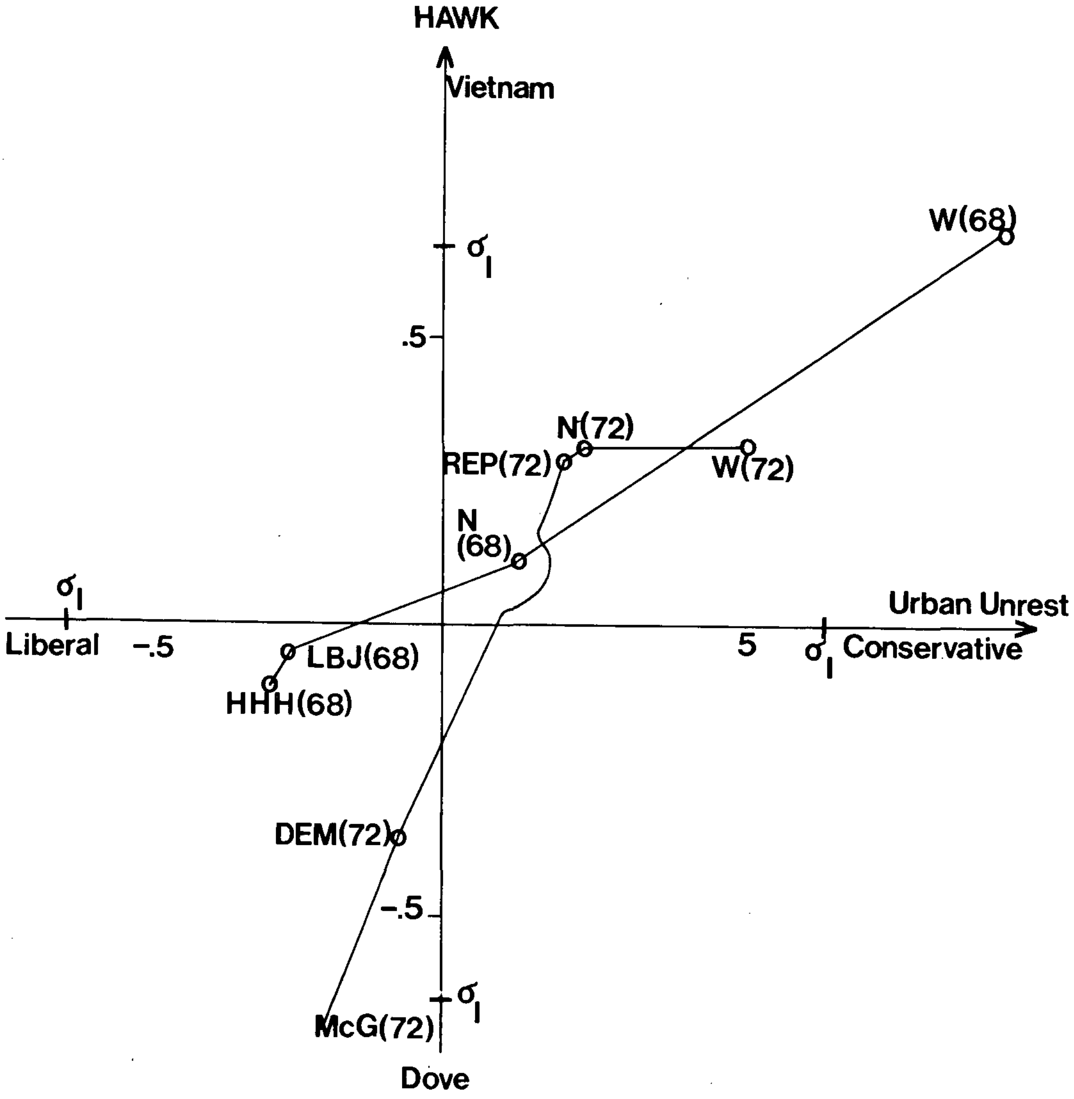
The results of the above normalization, displayed in Figure 7, tend to agree with the journalistic interpretation of the candidate positions. In 1968, Humphrey, Johnson, and Nixon were very similar and close to the average citizen on both dimensions, but especially so on Vietnam. Wallace, however, was particularly extreme on urban unrest and only somewhat less so on Vietnam. In 1972, McGovern was closer to the average citizen on urban unrest than the Democratic candidates in 1968. McGovern, however, was quite extreme on Vietnam, and the Democratic party was also more extreme than its nominees were in 1968. Nixon is estimated to have been more conservative than the average citizen in 1968, but in 1972, he is estimated to have been even more so. More dramatic movement was displayed by Wallace who appeared to have moved toward the center on both dimensions in 1972. In 1968, he was located beyond one (marginal) standard deviation of the distribution of citizens from their mean on both dimensions. By 1972, he was estimated to be well within one deviation.

Candidates appear to have been more widely dispersed on urban unrest in 1968, especially the three major party figures. By 1972, the dispersal was much greater on Vietnam. Further, while all candidates appeared to be approximately distributed along a straight line in 1968, such a "unidimensional" distribution of candidates was much less adequate in 1972.

These over-time comparisons do lend support to the scaling methodology employed. Issues that appear to us to reflect important concerns on which relatively clear positions are taken lead to better scaling estimations. Moreover, the scaling estimates conform reasonably well with a priori expectations based on (not impartially) observing the two elections.

Two-Dimensional Distribution of Ideal Points—1968

The final stage in the analysis concerns the location of the citizen's ideal point in the common space. We have already examined the ideal-point distribution for each individual dimension. In this section, we will look at the distribution of ideal points for the two 1968 dimensions in greater detail. Beyond our concern with the extent of negative weight parameters estimated, w_i, our interest in the individual coefficients is primarily directed at ideal point placement, which results from solving equation (32). Figure 8 provides a scatterplot of the citizens' ideal-point locations in the common space for 1968. It is clear that most positions are estimated to be rather centrally located. Recalling the Monte Carlo results concerning the ideal point estimates, we found that ideal points were recovered rather well over all, and that this was particularly true for less extreme estimations. Thus, this sort of distribution in the real data is likely to be somewhat more precise than a distribution with a larger number of extreme cases.



[a] σ_I indicates one (marginal) standard deviation of the ideal-point distribution from its mean of zero.

Figure 7. Candidate Positions on Urban Unrest and Vietnam in 1968 and 1972 with Dimensions Set so that Ideal Point Distributions Have Mean of Zero and Variance Equal to .416 (Based on Urban Unrest 1968)

The scaling estimated distribution of ideal points differs quite extensively from the distribution obtained using the raw data. In the seven-point scale data, the individual is constrained to be located at one of the (7x7) = 49 points of ordered pairs of positions on the two dimensions, leading to a distribution of ideal points which looks like a "circus tent" As in the scaling estimated distribution, there is some concentration of citizens towards the center, the global mode being located at the point (4,4), where one would place the large center pole of a circus tent. There were subsidiary modes, however, for each dimension at the end points of 1 and 7. This resulted in local modes at the four corners and at the "center edges" of such pairs of positions as (1,7) and (4,7), thus heightening the tent-like appearance of the distribution. In the transformed space, the "lumpiness" has been considerably smoothed—the distribution looking, if anything more nearly unimodal. While the distribution is not exactly unimodal or symmetric, it is greatly altered from its unscaled counterpart.

Predicting the 1968 Vote

The above estimations of the candidate positions exhibit a substantial amount of sub-

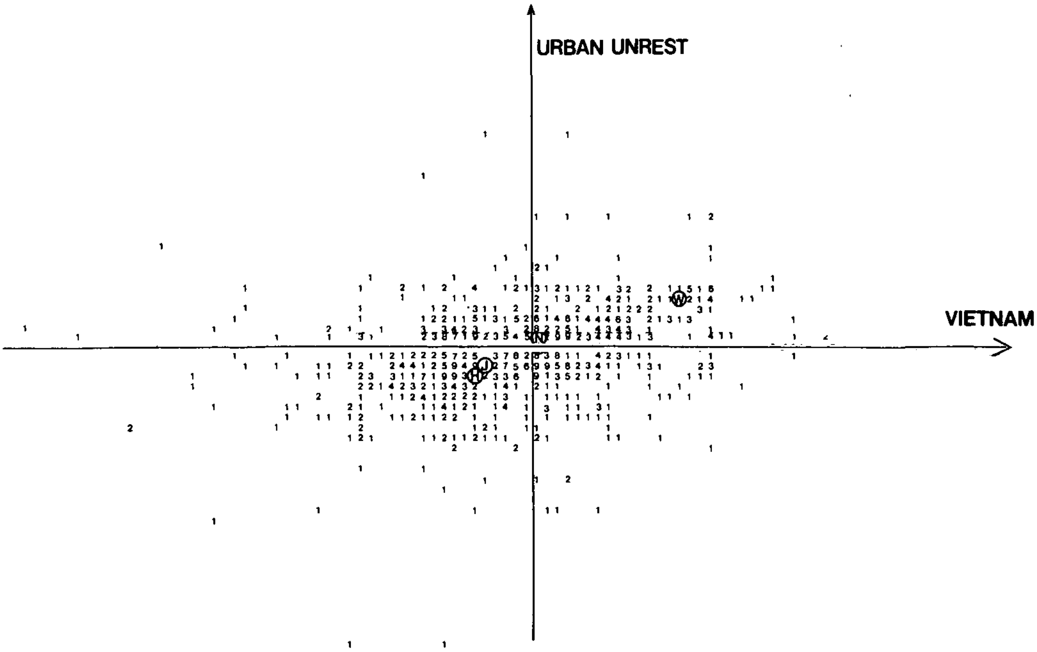


Figure 8. Two Dimensional Ideal Point Distribution, 1968

stantive reasonableness. Further, we have seen that the voter may be less confused over the candidate positions than would appear at first glance. These conclusions suggest that the assumptions of the spatial theories may constitute a reasonable model of voter behavior. The argument would be strengthened, however, if we could demonstrate that the scaling placements also lead to better predictions of electoral behavior.

To test this question, we make a simple prediction of the vote. In both the scaled and unscaled cases, the predicted vote is determined by computing the two-dimensional Euclidean distance between the voter's ideal point and the candidate position. The candidate position in the unscaled data is taken to be simply the mean perception of the sample on the seven point scales, while we of course use the estimated position for the scaled instance. We have also computed two forms of the actual vote. First, we use the actually reported vote itself and examine the relationship in two way contests between pairs of candidates. In this case, we look only at those voters who actually voted for one of the pair. We use a second measure based on the SRC's 100-point-thermometer measures of candidate evaluation. Here, the "vote" is measured by assuming that the individual would vote for whichever candidate in the pair stands higher on the thermometer preference measure. Finally, it should be pointed out that this spatial prediction is a special case of the Downsian type spatial model, where it is assumed that the citizen votes, if at all, for the candidate closest to him in space.

All forms of the predictions are uniformly high, as reported in Table 6. It is noteworthy that voters seem to do remarkably well in conforming to the predictions of even this simple Downsian model, as witnessed particularly by the very high proportions of accurate predictions in those situations involving Wallace, where the citizens are generally presented two alternatives which are more easily distinguished. In this case, more than three quarters of the vote is correctly predicted.

Even with the uniformly high percentage of votes correctly predicted, the scaling based predictions consistently outperform the unscaled data. The marginal improvements run anywhere from about 2 to 8 per cent. To make sure that the improvement of the scaling results was not entirely due to the method of handling the negative weighting parameters, we reran the predictions including only those respondents who were estimated to have positive weights. These results, summarized in Table 6, indicate that the predictions continued to be improved consistently by the scaling estimations. The predictions were also improved in the unscaled data with the negative weights removed, although the scaling predictions still outperform

Table 6. 1968 Vote Predictions, Comparing the Scaled and Unscaled Data

	Entire Sample				Negative Weights Removed			
	Vote		Thermometers		Vote		Thermometers	
	Unscaled	Scaled	Unscaled	Scaled	Unscaled	Scaled	Unscaled	Scaled
H-N	66%	68%	65%	69%	68%	70%	67%	72%
N-W	79	87	76	82	85	89	83	85
N-W	75	78	79	83	77	81	84	85
	Both Issues Salient							
	Vote		Thermometers		Vote		Thermometers	
	Unscaled	Scaled	Unscaled	Scaled	Unscaled	Scaled	Unscaled	Scaled
H-N	67	77	69	78	72	78	74	79
H-W	86	94	81	87	93	96	86	88
N-W	78	80	83	86	81	82	88	87
	Both Issues of Low Saliency							
	Vote		Thermometers		Vote		Thermometers	
	Unscaled	Scaled	Unscaled	Scaled	Unscaled	Scaled	Unscaled	Scaled
H-N	63	61	62	64	67	66	67	68
H-W	74	80	75	81	79	83	80	85
N-W	76	77	79	80	80	82	83	82

the seven point scale based ones. This finding illustrates the often noted tendency of people to vote their perceptions more often than they vote on the basis of the "true" positions of the candidates.

The high level of success in predicting the vote is based on a very simple model relating distance to electoral behavior. Nonetheless, some error in the prediction may result from the implicit assumption that the two issue dimensions are of equal importance. Citizens differ, of course, in the importance they attach to different issues (an argument made most forcefully by Repass[10]). Issues that the citizen believes to be of little importance to him are likely to have a smaller impact on his behavior than more salient issues. To check this possibility and any effect it might have on our results we applied a simple control. In particular, we looked at (a) the subset of citizens who claimed that both issues were either "very important" or "the single most important thing in the election" in the two questions following the seven point scale responses, and (b) the subset of citizens who claimed that neither issue was very important. We then reran the same predictions for these two groups, both for the whole subsets and for the subsets with negative weights removed. These results again conform to all our expectations. The scaling-based predictions are consistently higher, and sometimes greatly so, in all but a couple of minor instances, and the saliency control improved the predictions uniformly when both issues were thought to be of importance. In fact, in the "clearest" case of predicting the actual vote between Humphrey and Wallace using the scaling estimates with high saliency control and no negative weights, 96 per cent of the vote was accurately predicted (and 94 per cent when negative parameters were included).

Scaling Estimates—1972

We conclude this paper with a brief report of the scaling estimates for the 10 seven-point issue scales administered to the entire sample in 1972. The estimates are summarized in Table 7 and Figure 9.

The most prominent feature of the estimations is the general overall similarity of the candidates' relative positioning on most dimensions. Without exception, McGovern is clearly the most liberal candidate, followed by the estimated location of the Democratic party. As we saw earlier, McGovern is always separated from his party by a noticeable distance. Obviously, the Democrats nominated a candidate in 1972 who was not seen as a typical party member. The distance between McGovern and the Democratic party, perceptible though it may be, is always less than the sometimes very large relative distance between the Democrats and the third most liberal candidate. Thinking of the mean of the candidate distribution (i.e., 0) as the center of gravity, we see that it takes the three remaining candidates on the opposite

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[10] See David E. Repass, "Issue Salience and Party Choice," *American Political Science Review*, 65 (June, 1971), 389–400.

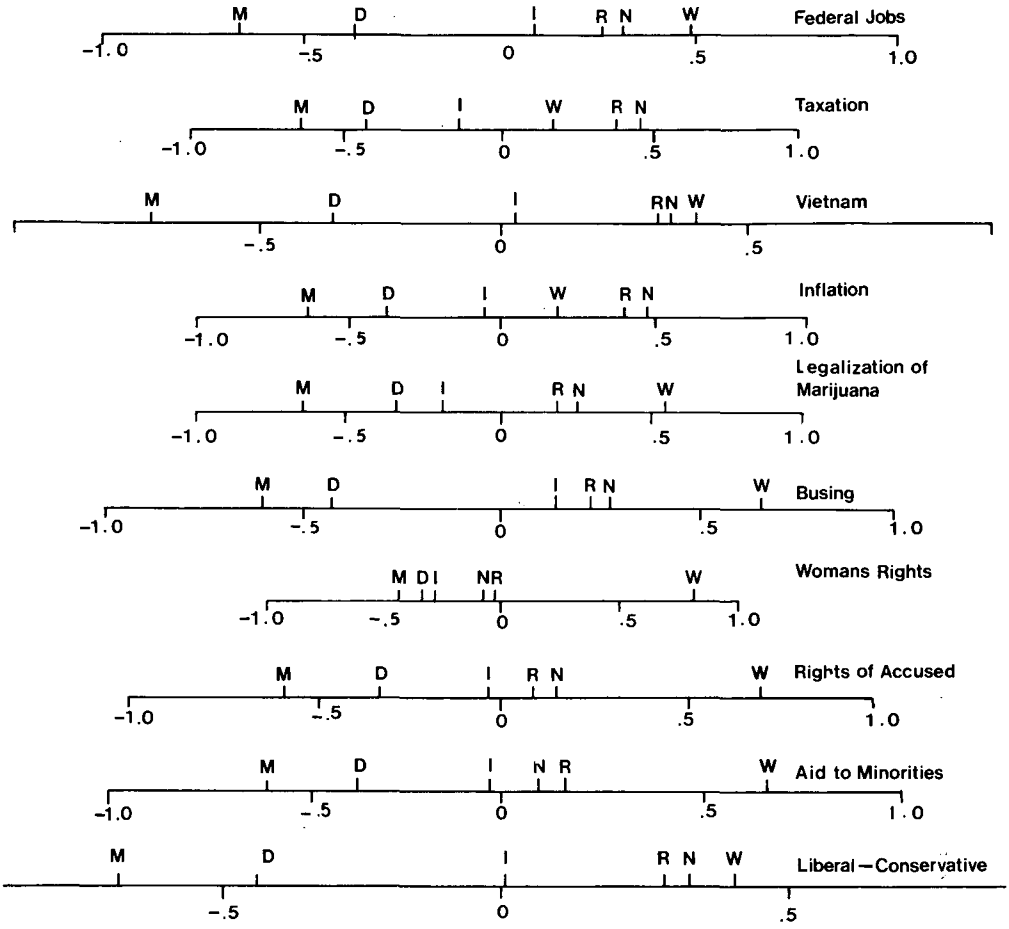

Figure 9. Candidate Positions in 1972

side of the mean to balance the Democratic Party and its nominee, excepting only the issue of women's rights. This contrasts with the estimates in 1968, when Wallace was estimated to be so extreme that the two Democratic candidates were not sufficient to balance him. In opposition to the distance separating McGovern and his party, the Republican party and Nixon are estimated as very similar on all ten dimensions. We cannot tell from these data whether this means that Nixon is seen as a (perhaps *the*) typical Republican or whether it means that a "typical" party nominee, especially an incumbent president, defines much of what the party stands for in the eyes of the electorate. The consistent tendency (19 out of 20 instances) to find the party closer to the mean of the distribution than its nominee may imply that parties evoke less clear cut perceptions than single individuals. This perception may be true in the eyes of each citizen, or it may be based on the obvious regional and other background differences within each party that evoke different perceptions, tending to "cancel out" extreme positionings.

The position of Wallace presents the most striking cross-dimensional differences. On most issues, Wallace is the most conservative candidate (although, as we have seen, perhaps not so obviously in 1972 as in 1968). On such issues as aid to minorities, the rights of the accused, busing, and especially women's rights, the distance separating Wallace from the two Republican stimuli is very large. The difference is much smaller on such issues as Vietnam and the general liberal/conservative continuum. Most importantly, on the two economic issues, Wallace is perceived to be only slightly on the conservative side of the mean, in a middle position dividing the Democrats from the Republicans. Again, the scaling estimated positions seem to reflect rather well the nature of the 1972 election. For example, the positioning of Wallace on the issues seems most reasonable in some detail. If he is viewed as a populist governor, his moderate economic stance could be expected.

Table 7. 1972 Candidate Position Estimates

	RMN	McG	GCW	Dem.	Rep.	I
Federal Jobs	.307[a]	–.663	.474	–.401	.283	.106
	.315	.383	.438	.345	.308	.871
Taxation	.462	–.647	.166	–.402	.421	–.110
	.377	.462	.494	.424	.383	1.099
Vietnam	.361	–.705	.376	–.355	.326	.046
	.290	.338	.430	.327	.277	.694
Inflation	.457	–.628	.198	–.436	.409	–.028
	.402	.454	.515	.431	.396	1.100
Legalization of Marijuana	.290	–.672	.551	–.354	.185	–.159
	.307	.410	.394	.355	.322	1.140
Busing for Integration	.240	–.587	.634	–.420	.133	.248
	.359	.406	.391	.365	.322	.857
Women's Rights	–.031	–.464	.827	–.314	–.018	–.320
	.382	.437	.559	.358	.365	1.433
Rights of Accused	.139	–.571	.714	–.370	.089	–.045
	.344	.414	.448	.325	.324	.927
Aid to Minorities	.116	–.606	.699	–.337	.128	–.008
	.303	.361	.406	.326	.297	.876
Liberal/Conservative Continuum	.343	–.669	.415	–.405	.315	.006
	.261	.348	.438	.311	.272	.597

[a]Entries are the scaled candidate or average ideal-point position on top and standard deviation underneath.

The distribution of ideal points has been indicated in Table 7 and Figure 11. Since the distribution is only an aggregate measure, the "average citizen" not surprisingly is located rather near the center on all dimensions, dividing the two parties and nominees. Fairly consistently, then, as would be expected, the Democratic party and candidate are seen as at least somewhat liberal (this holding in 1968 as well), while the Republicans tend to be somewhat more conservative on all issues (again, the same is true in 1968). In 1972, the average citizen is consistently much closer to Nixon than McGovern. The voters tend to be, on average, slightly on the conservative side of the mean of the candidate distribution, the principal exceptions being women's rights, taxation and the legalization of marijuana. This is not to say, of course, that the average citizen leans towards the legalization of marijuana. In fact, they are strongly conservative on the seven-point scale data. They simply see the three conservative candidates as even more conservative than themselves. These aggregate figures are, of course, only very general indicators. Our principal interest in the distribution of ideal points in this section is to rescale the unit of measurement of the dimensions, as we did previously. Recall that this would allow us to make cross-dimensional comparisons, at least to the extent of indicating the dispersal of candidates through the distribution of citizens. The relationship between the variance of ideal points and the dispersion of candidates should be inverse. That is, the smaller the variance of ideal points estimated in the scaling technique, the more widely dispersed the candidates relative to the citizens. Therefore, we have taken the issue with the smallest ideal-point variance (the liberal/conservative dimension) and set all other dimensions to have the same variance, keeping the candidate positions the same on the "base" issue dimension and narrowing the distance on all others by the appropriate proportion. As Figure 11 shows, the liberal/conservative dimension and Vietnam are quite similar in their relatively large dispersion of candidates (and especially McGovern). On the other hand, on the issue of women's rights, the candidates assume virtually identical positions. This issue has "shrunk" to less than one-half of its original size.

Conclusion

This article has attempted to apply a probabilistic model of the individual's response to questions on candidate perception to "factor out" the influences due to variations in reaction to the response task. This method then estimates the candidate positions by using the common part of individual perceptions. We have applied the method to the 1968 and 1972 election studies with reasonable success, in that the estimates correspond to a great degree with the a priori expectations, and explain voting behavior with a high degree of accuracy.

The Effect of Military Service on Political Attitudes: A Panel Study*

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Introduction

As commonly conceived, adult socialization involves the imparting of specific knowledge and skills rather than the inculcation of basic values. Brim has stated:

> In general, then, socialization after childhood deals primarily with overt behavior in the role and makes little attempt to influence motivation of a fundamental kind or to influence basic values. Society is willing to spend much less time in redirecting the motivation and values of adults than of children; for the latter it is understood that this is a necessary task of the institutions involved such as the family, and they are organized to carry out this function.[1]

While this perspective is quite sensible, it should not be overlooked that as individuals progress through the life cycle they typically encounter a number of experiences which may either reinforce or alter pre-existing values. Examples of such experiences include formal education, marriage and child-raising, occupational vicissitudes, and geographical mobility. At the political level, alterations in various orientations, down to and including opinions on important issues of the day, may flow out of postchildhood experiences. The dramatic surge in political cynicism in the United States, the weakening of what once were considered to be rather immutable ties to the political parties, and the shifting opinion structure about women's role in society are examples of how postadolescent experiences may alter (or reinforce) pre-existing states of being.

The place of specific formal and informal institutions in these processes of alteration and reinforcement is of special interest to us. Specific institutions can accomplish both of the major types of socialization functions suggested above. That is, they enable people to perform institution-specific behaviors, and they may also affect basic value structures. Medical schools, for example, have traditionally not only trained their members to perform strictly curative roles, but have also passed on sets of politically relevant values such as the superiority of private medicine over socialized medicine.

Our interest lies in the socialization effects of an institution touching in direct fashion the lives of millions of men, and in indirect fashion those of their families as well. We refer to the military service. Our intent is not to assess the socialization consequences of military service in the narrow sense of "what makes a good soldier" and how he is produced. Rather, we are drawn to the question of what impact the military service has on political orientations which are of relevance to all citizens, civilians and soldiers alike, and to the body politic in general. Are there particular experiences encountered in the military which leave political trace marks on the members setting them apart from their fellows who do not enter the military? In short, does military service have a political impact? In this paper we will make use of a 1965–1973 panel study of young men in order to provide a first answer to such questions.

Background

Nearly one-half of all living adult American males have been in the armed forces. The grand total of veterans in 1973 amounted to almost 29,000,000—a staggering number by any account. In the years 1968 and 1969 alone approximately 3,500,000 personnel were on active duty.[2] There are now between eight and

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*We wish to acknowledge the financial support of the Ford Foundation, the National Science Foundation, and the U.S. Army Research Institute for the Behavioral and Social Sciences.

[1] Orville G. Brim, Jr., and Stanton Wheeler, *Socialization After Childhood* (New York: Wiley, 1966), p. 27. Much of our thinking about institutions and later-life socialization has been influenced by this volume.

[2] All data and estimates come from the U.S. Bureau of the Census, *Statistical Abstract of the United States, 1973* (U.S. Government Printing Office, 1973), Tables 425, 446.

nine million Vietnam-era veterans. Scarcely any American family has remained untouched by military service in this century. Given the sheer numbers involved, plus the salience of war and civil-military relations, one would imagine that a great deal of attention would have been directed toward observing the political effects of military service.

Yet our knowledge is surprisingly limited. There are accounts of the behavior and attitudes of the men while in service. A classic piece of social science research, *The American Soldier,* demonstrated the feasibility of studying men in uniform.[3] There are also several learned works about the military as a profession, and about the military's role in the larger political, social, and economic life of the country.[4] More recently, attention has been focused on the likely composition and impact of an all-volunteer army.[5]

On the other hand, we know relatively little about individual-level consequences of the military experience. Physical and mental disabilities testify painfully to some such consequences. Marital discord, educational and occupational disruptions, and geographical dislocations are other obvious consequences. Yet even in these areas it is by no means clear that the military service, per se, is a major predictor variable of the outcomes. For example, there is a vigorous debate about the *eventual* occupational, income, and educational attainments of veterans versus nonveterans.[6]

Turning to the explicitly political arena, we are on even shakier ground. Although there are any number of conflicting images about what the military experience does for a young man, systematic evidence is hard to come by and, in fact, may run contrary to popular images. Among the less satisfying tests are public opinion studies which compare the views of veterans and nonveterans. These inquiries—subject to the weaknesses of static design and limited information about the military experience—typically find scant differences between veterans and nonveterans except on narrowly defined military issues; and even on such issues, differences tend to be small.[7]

Somewhat more appropriate designs, though very limited in number, also fail to support some popular images. For example, one of the most widely shared beliefs (especially in intellectual circles) is that, as a total, hierarchically organized institution, the military socializes its members into authoritarian modes. The empirical evidence from a few small-scale studies in the United States does not support this view.[8] And in the best-designed study to date, it was found that authoritarianism scores actually *decreased* among West German recruits. Moreover, the recruits were no more authoritarian than were control groups of civilians by the end of their service period.[9]

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[3] Samuel Stouffer et al., *The American Soldier,* Vols. 1 & 2 (Princeton: Princeton University Press, 1949).

[4] See, among others, Morris Janowitz, ed., *The Professional Soldier* (Glencoe, Ill.: Free Press, 1960); Morris Janowitz and Roger W. Little, *Sociology and the Military Establishment* (New York: Russell Sage, 1965); "The Military and American Society," special issue of *The Annals,* 406 (March, 1973); Samuel P. Huntington, *The Soldier and the State* (Cambridge: Harvard University Press, 1957); Charles C. Moskos, Jr., *The American Enlisted Man* (New York: Russell Sage, 1970); Kurt Lang, *Military Institutions and the Sociology of War* (Beverly Hills: Sage Publications, 1972); and Roger W. Little, ed., *Handbook of Military Institutions* (Beverly Hills: Sage Publications, 1971). Virtually all of the military socialization studies have dealt with the officer corps. See, among others, John P. Lovell, "The Professional Sociology of the West Point Cadet," in Janowitz, *The New Military* (New York: Russell Sage, 1964); William Lucas, "Anticipatory Socialization and the ROTC," in *Public Opinion and the Military Establishment,* ed. Charles C. Moskos (Beverly Hills: Sage Publications, 1971); Sandford M. Dornbursch, "The Military Academy as an Assimilating Institution," *Social Forces,* 33 (May, 1955), 316–321; Ronald G. Bauer, "Military Professional Socialization in a Developing Country" (Ph.D. dissertation, University of Michigan, 1973); and Gary L. Wamsley, "Contrasting Institutions of Air Force Socialization: Happenstance or Bellwether?" *American Journal of Sociology,* 78 (September, 1972), 399–417.

[5] For example, Jerald G. Bachman, *Values, Preferences and Perceptions Concerning Military Service: Part II* (Ann Arbor: Institute for Social Research, 1974); Morris Janowitz, "The Social Demography of the All-Volunteer Armed Force," *The Annals,* 406 (March, 1973), 86–93; and Morris Janowitz and Charles C. Moskos, Jr., "Racial Composition in the All-Volunteer Force: Policy Alternatives," *Armed Forces and Society,* 1 (November, 1974), 109–124.

[6] See Harley L. Browning, Sally C. Lopreato, and Dudley L. Poston, Jr., "Income and Veteran Status," *American Sociological Review,* 38 (February, 1973), 74–85; and Phillips Cutright, "The Civilian Earnings by White and Black Draftees and Nonveterans," *American Sociological Review,* 39 (June, 1974), 317–327.

[7] See Bachman, *Values, Preferences and Perceptions Concerning Military Service: Part II.*

[8] Elizabeth G. French and Raymond R. Ernest, "The Relationship between Authoritarianism and Acceptance of Military Ideology," *Journal of Personality,* 24 (December, 1955), 181–191; Richard William Firestone, "Social Conformity and Authoritarianism in the Marine Corps," *Dissertation Abstracts,* 20 (July, 1959), 394; and Donald T. Campbell and Thelma H. McCormack, "Military Experience and Attitudes toward Authority," *American Journal of Sociology,* 62 (March, 1957), 482–490.

[9] Klaus Roghmann and Wolfgang Sodeur, "The Impact of Military Service on Authoritarian Attitudes: Evidence from West Germany," *American Journal of Sociology,* 78 (September, 1972), 418–433. There is an enlightening exchange of views on this article in the July, 1973 issue of the same journal between Arthur Stinchcombe and the original authors, pp. 157–164.

The Vietnam War added fuel to the fires of popular beliefs about the impact of military service. There are a raft of studies, most of them limited in scope, dealing with the adjustment problems of Vietnam veterans.[10] Returning veterans were said to be especially bitter about the government's conduct of the war, its treatment of returning veterans, the lack of support of the home folks, and occupational troubles. Fragging, excessive dope usage, and interracial conflict were also cited as dehumanizing aspects of the Vietnam War in particular. Lack of enthusiasm for the nation's mission and outright hostility toward the supposed South Vietnamese allies were other features of the Vietnamese conflict which were thought to distinguish it from other wars and, in turn, to be implanting some lasting effects on the men who actually served during that period.

Some of these assertions were made in the heat of battle, as it were, and have subsequently been revised.[11] For example, it turns out that the desertion rate during the Vietnam War was lower—except during the waning years—than it was during the Second World War.[12] The prediction that the returning veterans would constitute a phalanx of dope addicts has proved to be exaggerated. And a study using a variety of convenient military and nonmilitary samples concludes that middle-aged women had higher overall pro-violence scores than did a group of enlisted men awaiting separation from the service.[13]

Nevertheless, the Vietnam tragedy highlighted, among other things, the questions of individual life fortunes, of how and whether the serviceman changes as a result of that experience. As students of political behavior, we are concerned with the degree to which the military experience impinges on individual political attitudes and evaluations. We are also concerned with the degree to which varieties of military experience have differential effects.

In a larger sense we may consider the military experience as but one example of whether and how postadolescent experience bends and shapes the individual's political stance. Obviously there are distinctive differences between the military and other institutions. For one thing the military has many properties of a total institution, i.e., it structures in detail the comings and goings of its personnel, and it has traditionally been very hierarchically organized.[14] In addition, the military, for the majority of its members, is a nonvoluntary institution. Finally, the military years—especially in wartime—typically represent an interlude, a short-term deviation from preferred alternatives; only a minority perceives army service as a means to long-range personal goals.[15] We must keep these distinctions in mind when observing the impact of military service, and the generalizability of our results to other institutional experiences must be tempered accordingly. Given all this, however, the military experience is similar to other institutional experiences in encompassing a number of factors which must be considered in gauging its impact upon individual attitudes and beliefs.

The first component is the *duration* of the experience. In his essay on adult socialization, Wheeler noted that "organizations differ in the average length of the stay of their recruits. . . . A reason for calling attention to the average length of stay as an important variable in socialization programs is that so little is known about it."[16] Wheeler was concerned primarily with cross-institutional comparisons, but the duration variable is likely to be significant in the study of a single institution as well: to the extent that an institution propagates certain norms and modes of behavior, these should become internalized more by long-termers than by short-termers.

A second, time-related factor is the *recency* of the experience. We may expect that, once outside the sustaining environment of the institutional setting, individuals will decline in their adherence to the values stressed or required by the institution. Thus Roghmann and Sodeur found that the decline in authoritarianism

[10] One of the best known and most controversial is Robert J. Lifton, *Home From the War* (New York: Simon and Schuster, 1973). A thoughtful discussion of various problems faced by the returning veterans can be found in Veterans World Project, *Wasted Men: The Reality of the Vietnam Veteran* (Edwardsville, Ill.: Southern Illinois University Foundation, 1972).

[11] A useful and sympathetic series of corrective statements is found in Paul Starr, *The Discharged Army: Veterans after Vietnam* (New York: Charterhouse, 1973), especially chapters 1 and 5.

[12] *Statistical Abstract,* 1973, Table 430.

[13] David Brady and Leon Rappoport, "Violence and Vietnam: A Comparison between Attitudes of Civilians and Veterans," *Human Relations,* 26, no. 6 (1973), 735–752.

[14] Although the military is often called a total institution, it seems more appropriate to reserve that title for the basic training and other very structured periods, during which the classic "staff/inmate" dichotomy prevails. See Erving Goffman, "On the Characteristics of Total Institutions," in his *Asylums* (Garden City, N.Y.: Doubleday Anchor Books, 1961).

[15] See Moskos, p. 199; Jerome Johnston and Gerald C. Bachman, *Young Men and Military Service* (Ann Arbor: Institute for Social Research, 1972), chapter 9; and Human Resources Research Organization, *Attitudes toward Military Service,* prepared for the Office of the Assistant Secretary of Defense (1971).

[16] Brim and Wheeler, pp. 90–91.

which they attributed to the "specific military life situation," did not endure after release into civilian life.[17]

A third component might be termed the *intensity* of the experience. Although all members are subject to the rules and norms of a total institution such as the military, some members experience more intense forms of requirements. In the military, of course, the most intense expression consists of executing the institution's ultimate mission, *viz.*, serving in combat zones. We will focus attention upon Vietnam veterans to ascertain the attitudinal effects of their experiences vis-à-vis those of other veterans and civilians.

A fourth, more subjective, component consists of the *affective* dimension. Brim has pointed out that the affective relationship between the socializing agent and the individual is likely to affect the nature of the socialization process.[18] We expect that veterans who have positive feelings about their military experiences are relatively more likely to identify with the dominant values of the military, assuming these values can be expressed in some sort of consensual form.

A final aspect to be considered is the *salience* of the institutional experience to other attitudinal and behavioral domains of the individual's life. Stated another way, the values that are most pertinent to the continuity and functioning of the institution are the ones most likely to be affected by institutional experience.

Methods: Subjects and Design

Several problems often accompany the attempt to assess the impact of an institutional experience such as military service. One of these is self-selection or, as is sometimes the case, nonrandom selection by others. Recruits may be different from nonrecruits even before they enter the institution. Thus, members may appear to differ from nonmembers as a result of institutional experience when pre-institutional observations would have revealed already existing dissimilarities. A second potential threat to inference has been termed the maturation effect. To cite an example, veterans may appear to be more cynical following their military experience than they were initially, tempting one to conclude that serving in the armed forces leads to increased distrust of government. It may be, however, that political cynicism increases with age regardless of whether one has been in the military. A third source of invalidity lies in secular shifts. Historical events external to the military experience may have altered attitudes of veterans and non-veterans alike. With regard to cynicism, for example, there is evidence that a substantial increase in levels of governmental distrust has occurred recently among all age cohorts. It is likely that this trend has also affected servicemen, quite independently of their military roles.

Various strategems have been devised to cope with problems of this sort. One of the most successful is the panel design. Fortunately, the panel data at our disposal span virtually the entire period of the Vietnam War. The panel consists of a national probability sample of male and female high school seniors first interviewed in the spring of 1965 and recontacted almost exactly eight years later. Of the 853 males originally interviewed, some 542 were reinterviewed and another 132 (outside of our primary sampling units) returned a lengthy mail questionnaire. In sum, we have two-wave data for 674 males.[19] Somewhat to our surprise, as many as 391 of the 674 males (58 per cent) served in the military at some time during the period 1965-1973. In retrospect, we should not have been so surprised, since inductions and pre-emptive volunteering zoomed in 1966 and remained high during the next few years.[20]

Although we might legitimately utilize the full 391 cases for our analysis, we have eliminated 63 of them because the respondents were either in the National Guard or in the Reserves. A reading of the protocols as well as an understanding of the special conditions under which these units operate led us to this decision. If we wish to explore the effects of the military as a quasi-total institution, we are in better shape dealing with servicemen who experienced some minimal level of active duty. With this adjustment, our raw *N* of servicemen is 328, or 55 per cent of the 611 male respondents (eliminating the non-actives from the base entirely). Because of the initial study design and execution (see note 19), the data used in the analysis are weighted; the weighted N for the military respondents is 410.

To understand more about the composition of the personnel we will be studying, a number of service characteristics are presented in Table

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[17] Roghmann and Sodeur, pp. 428–430.

[18] Brim and Wheeler, pp. 35–37.

[19] The field work in each instance was carried out by the Survey Research Center, University of Michigan. Details regarding the original data collection, study design, and weighting of data are available in M. Kent Jennings and Richard G. Niemi, *The Political Character of Adolescence* (Princeton: Princeton University Press, 1974). The weighted data differ only slightly from the unweighted data. Comparisons between panel respondents versus those for whom only 1965 data are available reveal virtually no differences in terms of 1965 characteristics.

[20] *Statistical Abstract,* 1973, Table 433.

1. This information will take on added significance as we enter the analysis proper. Considering the number of respondents, the allocation by military branch is reasonably close to the overall distributions during this period. The proportion of enlisted men in the sample (95 per cent) is higher than that in the military as a whole (about 85–90 per cent) primarily because of the age of the cohort. The fact that the great bulk of the servicemen were either drafted or were fairly certain that they would be drafted testifies to the nonvoluntary status of the military as an institution; the numbers who volunteered even though they felt they would not be drafted constitute rare cases.

Since this is the graduating class of 1965, it is not unexpected that the modal year of recruitment is 1966. It is a bit more surprising, however, that nearly one-third did not enter until 1968 or later. This figure perhaps reflects the 1968 decision to recruit more college-educated personnel to meet the manpower needs occasioned by the war. Separation dates are, of course, related to entry dates, but they are not perfectly synchronized for two reasons: (1) varying years of obligation, and (2) re-enlistment rates.

Military members of this panel saw heavy duty in Vietnam. When classified by the number of months spent in Vietnam, the range is considerable, rotation plans notwithstanding. Thirty per cent served less than a year; 43 per cent, exactly twelve months, and 28 per cent, more than twelve months.

Table 1 also contains two subjective questions dealing with the years our subjects spent in the military.[21] For all the criticism and griping which accompanies military life, and despite the highly controversial nature of the Vietnam War, it is apparent from Table 1 that satisfaction exceeded dissatisfaction. Whatever

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[21] The two questions were: (1) "In general, how would you describe your experience in the service. Would you say you were very satisfied, somewhat satisfied, somewhat dissatisfied, or very dissatisfied with your experience in the service?" (2) "When you were in the service, did you have any important beliefs or values challenged by other servicemen, by places you saw, or by things that happened?" For those responding in the affirmative: "What was that about?"

Table 1. Characteristics of Veterans in 1965–1973 Panel

Branch		Mode of Entry		Status	
Army	46%	Draft	35%	Officer	5%
Air Force	24	Volunteer	65[b]	Enlisted	95
Navy	21		100%		100%
Marine	5		(347)		(377)
Other	3				
	99%				
	(325)[a]				
Year of Entry		**Year of Discharge**		**Total Years**	
1965	17%	≤1968	21%	≤2	41%
1966	38	1969	22	3	22
1967	13	1970	20	4	27
1968	10	1971	15	≥5	9
1969	13	1972	9		99%
≥1970	8	≥1973	13		(392)
	99%		100%		
	(392)		(400)		
Vietnam Duty		**Satisfaction with Service**		**Military Service Challenge Values**	
Yes	45%	Very satisfied	28%	No	56%
No	55	Somewhat satisfied	35	Yes	44
	100%	Somewhat dissatisfied	21		100%
	(408)	Very dissatisfied	16		(323)
			100%		
			(318)		

[a] Parenthetical entries represent the percentage base. Unless otherwise indicated, all *N*s reported in the paper are weighted. *N*s will vary because of occasional missing data and, more importantly, because some questions were not asked on the mail questionnaire.

[b] Of those who volunteered, 65 per cent thought that they "definitely" would have been drafted had they not enlisted. An additional 25 per cent believed they "probably" would have been drafted.

deleterious effects the military interlude may have had on the political (or nonpolitical, for that matter) perspectives of the veterans, they cannot be attributed to overwhelming resentment of the time rendered. Parenthetically, it may be noted that satisfaction levels were no lower for those serving in Vietnam, and that length of time in Vietnam made scant difference. There is also only modest evidence that the veterans saw the military years as a time which challenged deep-seated values; indeed slightly more than half of them rejected the notion entirely. Moreover, a content analysis of the examples offered by those replying in the affirmative reveals the generally narrow and somewhat superficial nature of these belief challenges.

Results and Discussion

The range of orientations that will be examined in this paper is constrained by two sources. The first constraint is methodological. Although we shall consider some variables for which we have only time_2 observations—either because selection effects can be safely assumed to be rather unimportant or because the phenomenon itself did not exist at time_1—we shall restrict most of our analysis to variables for which we have over-time information. The second constraint is a theoretical one. On *a priori* grounds it stands to reason that not all political orientations are likely to be affected by the military experience. For example, we possess cross-temporal information on opinions toward prayer in public schools. It would be difficult, however, to construct a rationale about why and how service experience should affect views on that issue. In view of these two factors, the analysis presented here will deal for the most part with variables measured at both time points and for which the theoretical potential for alteration because of the military experience is reasonably great.

Political cynicism. A drastic increase in cynicism during the period covered by our observations has been solidly demonstrated.[22] Yet some population segments have been affected more than others by this increase. At first blush, one might well hypothesize that being in the military would lessen the intensity of this secular trend. After all, the socialization of inductees is designed to emphasize respect for and obedience to one's superiors, including the commander-in-chief.

On the other hand, the military makes continuous demands on its members. The servicemen may come to resent this control, a resentment that colors feelings towards other arms of the government as well. More significantly, the Vietnam War was roundly criticized as being an unjust and uniquely vicious war, one in which the allies were sometimes pictured as being no better and perhaps worse than the enemies. There was also ample criticism of a "no win" policy. The furious polarization over the war could hardly be said to have inspired great admiration for the government(s) running it. Thus instead of being less affected by the secular trend of deepening cynicism, military personnel may well have been more touched by it, placed as they were in the center of the maelstrom.

Our measures of political trust are twofold. First, we use the standard five-item political trust (cynicism) scale.[23] Second, we use a measure which gauges the relative evaluation of national government in the context of the federal system.[24]

We find in Table 2 that both service and nonservice males expressed about the same levels of political cynicism in 1965. The secular trend uncovered in other studies is apparent for both groups, but a difference appears in 1973, with veterans exhibiting slightly less cynicism than their nonservice counterparts. On our second measure of political trust, the between-group difference is clearer. Among nonservice personnel in 1973, 37 per cent expressed low faith in the national government vis-à-vis the state and local levels, while only 25 per cent of the veterans did so. The comparable 1965 figures are 9 per cent for the former group and 7 per cent for the latter.

It is possible that this presentation masks important variations in levels of trust within the service group. No differences were found, how-

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[22] M. Kent Jennings and Richard G. Niemi, "Continuity and Change in Political Orientations: A Longitudinal Study of Two Generations," *American Political Science Review*, 69 (December, 1975), 1316–1335; Arthur H. Miller, Thad A. Brown, and Alden S. Raine, "Social Conflict and Political Estrangement, 1958–1972" (paper presented at the meetings of the Midwest Political Science Association, Chicago, 1973); and Arthur H. Miller, "Political Issues and Trust in Government," *American Political Science Review*, 68 (September, 1974), 951–972.

[23] See Miller for the items used. For present purposes we employ an additive index.

[24] The text of these questions reads: "We find that people differ in how much faith and confidence they have in various levels of government in this country. In your case, do you have more faith and confidence in the national government, the government of this state, or in the local government around here?" After a probe for why the respondent made the particular choice, the next question read: "Which level do you have the least faith and confidence in—the national government, the government of this state, or the local government around here?"

Table 2. Political Cynicism among Nonveterans and Veterans

Political Cynicism	1965 Nonveteran	1965 Veteran	1973 Nonveteran	1973 Veteran
Low	41%	45%	7%	11%
	42	44	30	34
	15	9	45	44
High	2	2	19	12
	100%	100%	101%	101%
	(338)	(393)	(324)	(372)

ever, when respondents were classed by the length or recency of their military experience. Apparently the decrement in cynicism associated with the service experience occurs in the initiation phase and is rather durable. The relationship between satisfaction with the military experience and cynicism is ambiguous, although the most dissatisfied veterans are more cynical than their fellows. Once again, however, the pattern emerges more sharply when responses to the faith in government item are considered. As Table 3 indicates, the more dissatisfied with his military experience the veteran was, the more likely he was to express low faith in the national government. Moreover, this result is not due to prior attitudinal differences, as demonstrated by the similarities in 1965 response patterns.

Two possible explanations for this relationship come to mind. The first is that dissatisfaction with one aspect of national government, i.e., the military, is "transferred" to national political leaders and institutions as well. This transfer need not require an explicitly political basis for the dissatisfaction, but is rather a nonspecific generalization of low affect for the military to other national political stimuli. A second thesis is that both dissatisfaction with the military and distrust of government have a common and, most likely, political origin. To be more exact, opposition to the conduct of the war and other related national policies may lead both to dissatisfaction with the military experience and to distrust of government. These hypotheses need not, of course, be mutually exclusive, and we will explore them later; but first we continue our examination of the effects of other aspects of the military experience upon levels of cynicism.

When Vietnam veterans were compared with other ex-servicemen, they were found to differ little in terms of the two dependent variables presented thus far. If duration of duty in Vietnam is considered, however, an interesting pattern arises. Cynicism is related monotonically to length of tour in Vietnam (Table 4). Indeed, those veterans who remained in the Southeast Asian theatre beyond the usual twelve month tour are more cynical as a group than are nonservicemen. Responses to the low-faith item exhibit a similar pattern.[25]

These results raise the question of why service in Vietnam—or, more particularly, lengthy service—leads to such high cynicism. Analysis of some probable explanatory variables yields little insight. Veterans of more than one year's duty in Vietnam were somewhat less likely than average to express high satisfaction with their experience, but they were not correspondingly more likely to voice high *dis*satisfaction. Responses to questions about whether the U.S. ought to have become in-

Table 3. Faith in the National Government among Veterans, by Satisfaction with the Military Experience

Faith in National Government	1965 Satisfaction Low			1965 Satisfaction High	1973 Satisfaction Low			1973 Satisfaction High
Low	5%	7%	6%	6%	33%	27%	24%	21%
Not Low	95	93	94	94	67	73	76	79
	100%	100%	100%	100%	100%	100%	100%	100%
	(45)	(63)	(101)	(77)	(40)	(52)	(72)	(77)

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[25] Since length of duty in Vietnam depends to some extent on the particular branch of service, and since the qualitative nature of the tour also differs by branch, we examined the relationship between duration of tour and cynicism controlling for branch-type: ground forces (Army and Marines) versus non-ground forces (Navy and Air Force). The basic relationship between cynicism and length of duty in Vietnam was little affected by this control. For similar overall findings for the 1969 high school senior cohort, see Jerald G. Bachman and M. Kent Jennings, "The Impact of Vietnam on Trust in Government," *Journal of Social Issues*, 31 (No. 4, 1975), 141–155.

Table 4. Political Cynicism among Veterans, by Months in Vietnam

Political Cynicism	1965			1973		
	<12	12	>12	<12	12	>12
Low	42%	43%	43%	13%	8%	0%
	58	48	49	48	39	36
	0	9	4	30	42	42
High	0	0	4	9	12	22
	100%	100%	100%	100%	100%	100%
	(40)	(59)	(34)	(40)	(57)	(32)

volved in Vietnam and whether the war was handled well similarly fail to explain fully the high cynicism of long-term Vietnam veterans.

An answer may be found in the conclusion of the Vietnam World Project, a study based on the reports of Vietnam-era veterans:

> Veterans return from the Vietnam War with deeply negative and suspicious attitudes toward their nation's leadership and particularly the government, deriving from their view of the war itself, the effect of the U.S. role, the quality and competence of the U.S. military and civilian leadership at both high and low levels, and the fact that they feel that they have been used, manipulated and cheated. The first contact with government they have on their return is with the agencies charged with veterans' welfare: and the treatment they receive has tended to harden their most negative stereotypes about their government and its competence and good intentions.[26]

The responses of veterans in our sample to a number of open-ended items lend support to this argument. Veterans as a group did not differ much in their evaluation of the U.S. effort in Vietnam: regardless of duty classification, from 80-90% of former servicemen rejected the idea that "the government handled the war as well as it could have." When compared with other ex-GI's, however, Vietnam veterans tended more often to couch their rejections in terms of their personal experiences and first-hand knowledge of the conflict.

It should be stressed that the roots of cynicism for these veterans lie not so much in any deep moral revulsion to the war as in a sense of being brought into a situation with an unclear sense of mission—and then being forgotten. Four men who saw extended duty in Vietnam supplied our interviewers with these comments:

> "The government doesn't keep its word. We should have won or not gotten involved."
>
> "I don't know what they [the national government] are doing or why. We should have bombed it out."
>
> "They should have let the military men take care of it."
>
> "The American people don't even think about us while we're over there, and no help or honor now that we're back."

To be sure, some veterans of the Vietnam War were struck by its moral consequences. But the conclusion that one must draw from the interviews is that it is the feeling of being "let down" by one's government, of being sent out to do a job and returning home a loser—and not a sense of guilt—that has politically embittered significant numbers of long-term Vietnam veterans.[27]

As a final evaluation of the impact of military experience on trust in government, the time$_2$ measures were regressed on the experience variables suggested as being important by the tabular results—namely, the service/nonservice dichotomy, length of duty in Vietnam, and satisfaction—plus a number of "control" variables. The latter, which consist of race, father's SES, education (college vs. noncollege), and the time$_1$ dependent variable, were included to ascertain whether the apparent effects of service-related variables were due partly to compositional differences, either demographic or attitudinal.[28] Two measures of opinions regarding the Vietnam War were then added to the regression equations to determine whether the link between satisfaction with the military experience and trust in government was direct, or whether, as suggested earlier, both attitudes stem from policy-specific opinions.

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[26] Veterans World Project, *Wasted Men: The Reality of the Vietnam Veteran*, p. I.3.

[27] Our reasoning here parallels in part that developed by Howard Schuman on the moral versus pragmatic opposition to the war. See his "Two Sources of Anti-War Sentiment in America," *American Journal of Sociology*, 78 (November, 1972), 513–536. See also Starr, chapter 1.

[28] The three compositional control variables are employed in all regression analysis presented in this paper. The race variable is a white (coded "0") nonwhite (coded "1") dichotomy. Father's socioeconomic status is measured by the Duncan decile SES code of the 1965 occupation for each respondent's father. The college variable is coded "1" if the respondent had any college experience and "0" otherwise.

The regression analysis, the final results of which are displayed in Tables 5 and 6, yields some important insights.[29] First of all, we note that the $time_2$ dependent measures, particularly the multi-item cynicism scale, are "predictable" to a fair degree and that the parameters associated with the lagged dependent variables are significant. These results take on greater substance when the age location of our sample and the eight-year gap between waves is considered. Our confidence that we are tapping a political orientation that is meaningful to these young adults—rather than a "nonattitude"—is thus enhanced. Second, the depressing effect of military service on political cynicism and the increase in distrust arising from long-term service in Vietnam persist after other control variables are taken into account.[30] Parenthetically we note that college education and race are factors affecting cynicism, as expected, but SES, *ceteris paribus,* is not.[31] Finally, the satisfaction variable is no longer an important predictor of trust in government, as indexed by either dependent variable, once the two war-related opinions are taken into account. This result suggests that rather than there being a direct link between dissatisfaction and cynicism—a generalization of negative affect—both attitudes are based on prior evaluations of war-related policy matters.

Political attention frames. Joining the military almost necessarily exposes the individual to new geographical areas and political jurisdic-

Table 5. Regression Equation for Political Cynicism t_2

Variable	Standardized Coefficient	Unstandardized Coefficient	t-ratio
Constant		14.568	
Cynicism t_1	.195	.228	4.74
College	.117	1.211	2.77
Non-white	.156	2.887	3.80
Military service	−.113	−1.093	2.53
⩾12 months Vietnam	.096	1.353	2.16
U.S. do right	−.245	−2.500	5.82
War handled well	−.123	−1.901	2.96

Multiple $R = .44$ Unweighted $N = 488$

Table 6. Regression Equation for Low Faith in the National Government t_2

Variable	Standardized Coefficient	Unstandardized Coefficient	t-ratio
Constant		.387	
Low faith t_1	.135	.244	2.78
Military service	−.125	−.113	2.38
⩾12 months Vietnam	.107	.146	2.05
U.S. do right	−.201	−.194	4.13
War handled well	−.106	−.156	2.20

Multiple $R = .29$ Unweighted $N = 397$

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[29] Certain of the independent variables in the regressions, e.g., satisfaction with the military experience, are substantively meaningful for only a portion of the total male sample. In a given regression equation, therefore, such a predictor is, strictly speaking, an interaction variable—for example, satisfaction x service/no service, where the service/no service variable is a dichotomy scored "1" for males with active service experience and "0" otherwise. The coefficients associated with other variables may also differ across subgroups of the total sample; this possibility may be explored within each subgroup by examining the distribution of residuals, the correlations of residuals with predictor variables, and by the inclusion of additional appropriate interaction terms into the regression equation. See J. Johnston, *Econometric Methods,* 2nd ed. (New York: McGraw-Hill, 1972), pp. 192–207.

[30] In both regressions, length of duty in Vietnam was operationalized by a dummy variable coded "1" for respondents with 12 months or more service in Southeast Asia and "0" otherwise.

[31] In these regressions and the ones to follow,the coefficient of a predictor variable was required generally to be equal to twice its standard error for that variable to be included in the final equation. None of the three compositional controls passed this criterion with respect to the equation for the "low faith" dichotomous dependent variable. They were, therefore, deleted, and the equation was re-estimated.

tions. This is, of course, especially the case for those serving overseas. Experiencing these new locales directly rather than vicariously should heighten their sensitivity to larger geopolitical domains. At the same time, we know from previous analysis that this cohort is now less focused on larger domains than it was in 1965, a decline which we attribute primarily to life cycle changes. Other things being equal, then, we should expect military service to impede the decrease in the attention directed toward larger arenas. Our indicator comes from applying the Coombsian unfolding technique to rank orderings of interest in international, national, state, and local affairs.[32]

The hypothesis that military service should at least retard the shrinkage in concern with larger geopolitical domains is moderately supported by the analysis. Table 7 reveals a slight overall decline in concern over the eight-year period among both civilians and servicemen. Yet it is apparent, especially at the upper reaches of the dimension, that the decline was more precipitous for the nonveterans. This is all the more remarkable because they began the period with a slightly greater attentional focus on larger political domains. By 1973 this position had, in fact, been reversed. Thus the relative decline in concern was less among the veterans, and their absolute positions on the scale are now higher. In this sense, then, the military experience helps maintain or even increase broader political horizons.

The nature of the veterans' experiences suggests, however, that the military-civilian reversal will not necessarily be long-lived. One might logically expect that the duration of military service and the intensity of that service (as captured by Vietnam exposure) would be related to a sustained "cosmopolitanism." Yet neither of these factors is discriminating. What is important is the *recency* of the experience. Although there is some irregularity in the individual cells, the general pattern is for recency to be very much related to broader frames in 1973 (Table 8). The 1965–1973 comparisons reveal a bit more jaggedness, but the mean tendencies are still pronounced. The more recent the tour of duty, the higher the cosmopolitanism has remained.

It is difficult to know precisely if what we are tapping here is a deterioration function, but that would appear to be the case. We may expect the relative difference among the veterans to smooth out over the ensuing years as the military experience fades into the background. In other words, we are observing short-term rather than long-term effects. For specific individuals these short-term effects may, of course, be extended into long-term consequences. For others a temporary surge or maintenance may also open up other political vistas of longer-lasting duration. Strictly on the basis of the measure itself, however, the institutional effect appears to recede over a relatively short time.

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[32] This measure has elsewhere been called the local-cosmopolitanism scale. See Jennings and Niemi, *The Political Character of Adolescence*, chap. 10.

Table 7. Political Attention Frames among Veterans and Nonveterans

	1965		1973	
Attention Frames	Nonveterans	Veterans	Nonveterans	Veterans
Narrow (local)	12%	11%	15%	12%
	16	16	24	17
	12	18	16	15
	31	35	34	39
Broad (international)	28	21	10	17
	99%	101%	99%	100%
	(310)	(355)	(309)	(356)

Table 8. Political Attention Frames among Veterans, by Recency of Discharge

	1965			1973		
Attention Frames	⩽1968	1969–70	⩾1971	⩽1968	1969–70	⩾1971
Narrow (local)	10%	14%	8%	24%	13%	3%
	27	15	8	20	18	15
	25	12	22	16	18	11
	16	42	37	26	36	49
Broad (international)	22	18	25	14	15	22
	100%	100%	100%	100%	100%	100%
	(73)	(146)	(125)	(74)	(143)	(129)

The regression analysis indicates that military service in general and the recency of that service in particular remain as important predictors of attention objects even when the lagged dependent variable is taken into account. Neither socioeconomic status, race, nor college experience are related significantly to $time_2$ scores. With regard to the deterioration in the effects of the military experience with the passage of time, the unstandardized coefficients suggest that the increment from the service experience ceases to be apparent after about five years of civilian life, a prediction consistent with the tabular presentation.[33]

Tolerance of others. The military treats individuals of varying backgrounds in a rather indiscriminate manner. Boot camp and basic training are essentially the same for all enlisted men, and the various other forms of initial socialization show little respect for station in life. With the *de facto* racial desegregation of the armed forces (especially the Army) by the mid-1950s, even the time-honored black-white discrimination has been attenuated. For many young men, being in the military thus represents their first experience of a heterogeneous situation which has little respect for caste or class.

A prominent hypothesis about social groups is that exposure to outgroups increases the positive affect for those groups.[34] Of course, there are qualifications in terms of particular situations and pre-existing attitudinal states. Nevertheless, the essential hypothesis is that regard between contrasting groups should increase as a result of military service. To operationalize this hypothesis we assess the change in feelings of non-Southerners about Southerners and of whites about blacks.[35]

Pursuing the same line of reasoning, exposure to diversified groups, practices, and beliefs, should breed a greater tolerance for political nonconformity, *ceteris paribus*. Offsetting this liberalizing force, however, is the persistent, official message of the military that the American way is the best and that challenges to it should be resisted. In addition, the military men serving in a foreign environment often draw comparisons—especially about material well being—which indeed make the American way seem superior.[36] And simply being an outsider may increase chauvinism. We use a three-item index of civic tolerance to test these competing hypotheses.[37]

Turning first to group evaluations, the findings, with one notable exception, do not support the social contact hypothesis. The results are unambiguous with respect to how non-Southerners felt about Southerners. Bivariate and multiple regression analysis reveals that while 1965 ratings, higher education, and race are important predictors of 1973 ratings ($R = .35$), no service variables account for any significant portion of the variance. Apparently the daily interaction of recruits from different regions that is afforded by the armed services does little to modify the feelings of one regional group towards another.

In a second test of group evaluations we examined the feelings of whites toward blacks. Once again, the ratings applied by majority group members to minority group members were not altered by service-related experiences. A reconsideration of the basic hypothesis, however, led to the subhypothesis that serving in the military would have differential impact depending upon the prior relationships which whites had had with blacks. In 1965 the white respondents were asked whether they had any black friends. It seems probable that enforced interaction would produce more profound effects on those who had no black friends than on those who did. A reanalysis of the data along these lines demonstrates that being in the service did alter the evaluations of blacks by whites who had no black friends in 1965. The effect, however, is negative: white veterans who professed to having no black friends felt less favorable toward them in 1973 than did other whites. Regression results show that this effect is undiminished when 1965 ratings, further education, and parental SES are taken into account. The latter two control variables made

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[33] The unstandardized equation is: $Cosmopolitanism_2 = 3.77 + .210\ Cosmopolitanism_1 + .924\ Military\ service - .173\ Years\ since\ discharge$. All coefficients are greater than twice their standard errors (multiple $R = .30$).

[34] The classic formulation is that of George C. Homans, *The Human Group* (New York: Harcourt, Brace, and World, 1950), especially pp. 11–12, 101–106.

[35] These feelings were tapped by the "feeling thermometer" technique by which groups are assessed on a cold (unfavorable) to warm (favorable) dimension running from 0° to 100°.

[36] Some components of military life abroad are sketched in by Moskos, *The American Enlisted Man*, chapter 4.

[37] The three agree-disagree items were:

1) "The American system of government is one that all nations should have."
2) "If a Communist were legally elected to some public office around here, the people should allow him to take office."
3) "If a person wanted to make a speech in this community against religion, he should be allowed to speak."

Table 9. Regression Equation for White Feelings toward Blacks t_2

Variable	Standardized Coefficient	Unstandardized Coefficient	t-ratio
Constant		49.545	
Feeling score t_1	.184	.145	3.89
No black friend x service	–.105	–3.876	2.22

Multiple R = .23 Unweighted N = 442

little contribution in the final analysis and were deleted from the equation (Table 9).[38]

Under certain conditions, then, interaction between contrasting groups in an institutional setting may increase rather than decrease cross-group antipathies. Two probable reasons account for the negative effect in the case of racial groups and the absence of effects in the case of regional groups. First, we had no way of separating non-Southerners according to the type of previous interaction which they had experienced with Southerners. Second, and more significantly, racial tensions are more manifest and of greater salience in American society than are tensions involving region. As a result, feelings about racial groups are more likely to be modified by social interaction in the institutional setting.[39]

Having looked at evaluations of outgroups as one indicator of tolerance, we now take up the more general theme of civic tolerance, the willingness to countenance a diversity of viewpoints in matters of public concern. At the gross level, serving in the military scarcely distinguishes one young male from another with respect to our measure of civic tolerance. A marked shift among both groups has led toward greater tolerance over the eight-year period (Table 10). There was only a marginal difference between the two in 1965 and, if anything, the small edge which the civilians held originally has all but disappeared.

Although military service, per se, seems to have had little impact on civic tolerance, some components of the service experience ostensibly do leave an imprint. Those serving four years or more are moderately less tolerant in 1973 (tau-b = –.12), but part of this difference rests in their also being a bit less tolerant than the "short-termers" in 1965. Moreover, in the regression analysis this duration factor disappears as a predictor of 1973 civic tolerance scores. From a strictly descriptive standpoint, however, the lower tolerance of the longer term servicemen is important. It suggests that the more senior enlisted men at any given point in time will probably be less tolerant and less likely to take a civil libertarian and non-chauvinistic approach to politics. We see here a hint of the careerists, stereotypical "rah-rah" noncommissioned officers, and jingoistic commissioned officers.

As a whole, veterans spending time in Vietnam do not differ appreciably at either point in time from those not sent there, but length of time in Vietnam is of some consequence. While the pattern is slightly erratic, the longer the Vietnam duty, the more tolerant was the Vietnam veteran in 1973 (Table 11). Short-term Vietnam veterans were, however, noticeably *less* tolerant in 1973 than military

Table 10. Civic Tolerance among Nonveterans and Veterans

Civic Tolerance	1965		1973	
	Nonveteran	Veteran	Nonveteran	Veteran
Low	36%	40%	15%	12%
|	34	36	25	31
High	31	24	60	57
	101%	100%	100%	100%
	(348)	(406)	(349)	(401)

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[38] Our analysis undoubtedly would have been sharpened if we had been able to obtain such information as the degree of racial integration in the serviceman's company, the actual amount of social interaction, and the like.

[39] Our findings on race run contrary to those for World War II (*The American Soldier,* Vol. I), and the Korean War (Leo Bogart, *Social Research and the Desegregation of the U.S. Army* [Chicago: Markham, 1969]). But they are of a piece with much impressionistic evidence about the Vietnam period. See Charles C. Moskos, Jr., "Studies of the American Soldier: Continuities and Discontinuities in Social Research" (paper presented at the annual meeting of the American Sociological Association, New York, 1973).

Table 11. Civic Tolerance among Veterans, by Months in Vietnam

Civic Tolerance	1965			1973		
	<12	12	>12	<12	12	>12
Low	50%	50%	38%	18%	20%	8%
|	25	25	40	40	30	30
High	25	25	22	42	50	63
	100%	100%	100%	100%	100%	101%
	(42)	(60)	(39)	(40)	(58)	(39)

men who did not serve at all in Vietnam. Thus, the increase in tolerance linked with duration of Vietnam experience serves, in part, to "make up" for this initial decrement. This movement should not be overemphasized, but it will become clearer in the regression analysis below.

Simply in terms of bivariate relationships, recency of separation from the service is the most influential determinant of tolerance. Whereas in 1965 the pattern was irregular and essentially weak, by 1973 there was a clear positive relationship between recency of separation and level of tolerance (Table 12). This finding supports the hypothesis that the diversity to which the enlisted man is exposed does, indeed, have a liberalizing effect. That the absolute 1973 scores and the relative gain of those longest removed from the military are lower than those of the most recently discharged appears to justify the recency theorem: the effects of the institutional experience will be most noticeable among those most recently undergoing it. It should follow that the gap between the more recent and less recent veterans would decline over time.

This set of findings struck us, at first glance, as a handsome example of the recency effect. On reflection, two features bothered us. One was the apparent strength of the process. Why should recently discharged veterans be *that* much more tolerant of diversity? There was little in the systematic or anecdotal literature to suggest an elevation of that magnitude. Another worry was compositional effects. It is well known that the early years of Vietnam build-up drew disproportionately from the noncollege educated, partly as a result of the lowering of entrance standards in 1966. Not until 1968 was there a conscious decision to enlarge the manpower pool by dropping deferments for college graduates. Given these trends and the relationship between date of entry and date of exit, it is quite likely that the recently separated are more highly educated than those discharged earlier. When coupled with the known association between education and tolerance of diversity, this differential composition could mean that the apparent salutary effects of military service are artifactual and that there is no recency effect at work.

As it turns out, our suspicions were justified. Having had at least some college training varies directly with the period of discharge. Among those discharged in 1968 or earlier, the figure is 30 per cent; among those discharged between 1969–1970 it is 59 per cent; and among those discharged most recently, it is 77 per cent. This striking range does not, of course, prove the case. What does prove it is what happens when education and recency of departure are entered into a multiple regression along with other predictor variables of the 1973 scores. Until educational attainment enters the equation, recency of discharge still makes a statistically significant contribution. But once education is entered the effects of recency disappear completely (Table 13). Correspondingly, educational attainments and the 1965 civic tolerance scores make positive and substantial contributions to the 1973 outcomes.

The regression equation also reflects the pattern of the relationship between Vietnam

Table 12. Civic Tolerance among Veterans, by Recency of Discharge

Civic Tolerance	1965			1973		
	⩽1968	1969–70	⩾1971	⩽1968	1969–70	⩾1971
Low	50%	43%	32%	16%	13%	9%
|	30	33	40	37	38	17
High	20	24	28	46	49	74
	100%	100%	100%	100%	100%	100%
	(84)	(166)	(145)	(79)	(168)	(143)

Table 13. Regression Equation for Civic Tolerance t_2

Variable	Standardized Coefficient	Unstandardized Coefficient	t-ratio
Constant		8.280	
Civic tolerance t_1	.270	.220	6.96
College	.346	2.180	8.89
Military service	.079	.469	1.89
Vietnam duty	−.196	−1.461	2.05
Length of Vietnam duty	.166	.571	1.78

Multiple R = .51 Unweighted N = 564

service and tolerance. A decrease in tolerance is predicted for veterans serving in Vietnam, length of duty held constant. With increasing time in Vietnam, this decrease is reversed, as is indicated by the positive coefficient associated with the length of Vietnam duty variable. The coefficients for the military service and the length of Vietnam duty variables are not quite equal to twice their respective standard errors. These predictors were retained in the regression, however, to clarify the pattern described above.

Returning to the relationship between tolerance and date of separation, what at first took on the quality of a classic example of the recency effect has now been dissolved. Among other things, this exercise illustrates the necessity of controlling for compositional differences when trying to assay the socialization consequences of institutional experiences. At the same time, it does not invalidate the recency theorem. We saw that with respect to attention frames, recency persisted as an explanatory variable even when a number of controls (including education) were introduced. In addition, the practical implications of our findings should be noted. Even a casual knowledge of the course of the Vietnam War indicates that the more recently returned veterans were a different ilk than were the early returnees. They were more outspoken and more politically liberal in their behavior. Surely the nation's growing exasperation and self-questioning played a part in eliciting the differential behavior of the more recent veterans. But it seems equally likely that the returning veteran's higher education, with its corollary of greater commitment to political tolerance and diversity, also played a role in setting the tone and content of postdischarge behavior.

War-related issues. Being in the military, especially during a war, is likely to make certain kinds of issues more salient than for those not directly involved. The penetration of news coverage about the Vietnam War, to say nothing of the widespread demonstrations against the war, perhaps blurred this distinction more than in previous wars. What one could not experience directly could be experienced vicariously through the medium of the evening news broadcast. But there is a difference in kind between viewing a bombing raid or scouting expedition on television and directly participating in such activities or being faced with the possibility of doing so.

In particular, the *saliency* of the Vietnam War and views about it should be of special relevance for this cohort of veterans. The most prominent view about the Vietnam War veterans is that even though doubt and suspicion abounded about the worthwhileness of the effort, they were at least more supportive than comparable civilian cohorts.[40] In part this would be a function of dissonance reduction. Their participation would have to be justified and sustained in the face of vicious attacks against their involvement, on the one hand, and their own skeptical opinions about the purposes of the war, on the other. The reduction of dissonance could be accomplished either through denigrating the character and motives of the war's attackers or in developing a rationale, however primitive, of the war's justification. Our hypothesis is that, other things being equal, the servicemen will be more supportive of the Vietnam effort than will comparable nonveterans. To capture these sentiments we rely on multiple indicators: (1) the degree to which the respondents believe that U.S. entry into the war was a mistake; (2) the degree to which they think the war could have been handled in a better fashion, and (3) the respondents' reasons for criticizing the war effort.[41]

[40] Moskos, *The American Enlisted Man,* p. 35.

[41] The questions, in order, were: (1) "People have different views about America's involvement in Vietnam. Do you think we did the right thing in getting into the fighting in Vietnam or should we have stayed out?" (2) "Do you think the government handled the Vietnam War as well as it could have?" Negative responses to this question were followed by, (3) "What do you think the government should have done?"

In 1973 the respondents were asked a series of questions regarding American involvement in Vietnam. Their replies underscore the widely held view that the Vietnam War was unpopular in a number of respects. Although the degree of unpopularity varies somewhat according to certain service-related experiences, no class of respondents—veteran or nonveteran—gave the U.S. engagement a very favorable evaluation.

A minority of both veterans and nonveterans in our sample agreed that the U.S. "did the right thing in getting into the fighting in Vietnam," with the figure being somewhat larger among the veterans (39% vs. 29%). Among the latter, those who saw Vietnam duty were more likely to support U.S. entry. Still, only 46 per cent of the war-zone veterans versus 32 per cent of the others thought that American entry was appropriate. The duration of the Vietnam tour of duty scarcely affects this 46 per cent figure.

Length of overall service accounts for little difference in opinions about U.S. involvement, but the bivariate results suggest that recency of service may be consequential. The most recent veterans are less likely than their comrades to think that the U.S. should have entered Vietnam (27% vs. 46% for the remainder). This result should be interpreted cautiously, since we know that recent veterans differ from others in being better educated. They also differ in terms of less Vietnam duty. The multiple regression indicates, in fact, that the relationship between recency of discharge and opinions about U.S. involvement is reduced substantially (though not completely) once education and Vietnam exposure are taken into account (Table 14).

With regard to a second Vietnam-related question, the unpopularity of the American action in Southeast Asia is even more evident. Both veterans and nonveterans rejected by a 9:1 ratio the proposition that "the government handled the Vietnam War as well as it could have." None of the service-related variables affects this ratio significantly.

Although nearly all respondents were critical of the handling of the war, the reasons underlying the criticism vary. Veterans were slightly more likely than nonveterans to have preferred a more vigorous, all-out pursuit of the war (33% versus 24%). Those spending time in Vietnam do not differ from others in desiring greater commitment, nor is the length of time in Vietnam an important ingredient. When recency of service is considered, the variation is more pronounced. Veterans who returned to civilian life after 1970 were much less likely than other veterans to favor a stronger stand. This difference remains when compositional variables are held constant (Table 15). Thus the most recent returnees, who were in the service while the "Vietnamization" program was underway, are noticeably less enthusiastic about a vigorous policy than are the returnees of earlier years.

Rather than representing a recency effect in the traditional sense, this finding suggests that institutional experience exerts a differential impact depending upon events in the external environment. As the war ground on, disillusionment grew about the propriety of U.S. involvement and about the capacity to accomplish any definitive, positive conclusion. Servicemen in the later years of the war would have been especially exposed to these sentiments. Consequently, their lessened commitment to a strong war policy reflects a different historical moment than that of the early returnees. In this instance, we might expect to find that the differences noted will persist for some time and not fade away, as would be true in the more customary instance where recency effects are noted.

A number of respondents volunteered that the Vietnam War had caused them to lose faith in American leaders. These people were as likely to be veterans as nonveterans, and the veterans could not be differentiated to any appreciable degree by the quality of their military interlude (i.e., duration, recency, etc.). There was a tendency, though, for long-term

Table 14. Regression Equation for Favoring U.S. Involvement[a]

Variable	Standardized Coefficient	Unstandardized Coefficient	*t*-ratio
Constant		.469	
College	−.138	−.135	3.20
Vietnam Service	.102	.113	2.26
Discharged since 1970	−.092	−.092	1.96

Multiple R = .23 Unweighted N = 578

[a] Opposition, coded "0," includes those disagreeing (or "not sure," "depends") that the U.S. did the right thing in getting involved in Vietnam. Those agreeing are coded "1."

Table 15. Regression Equation for Favoring a Firmer Stand in Vietnam[a]

Variable	Standardized Coefficient	Unstandardized Coefficient	*t*-ratio
Constant		.555	
College	–.214	–.212	4.99
Nonwhite	–.120	–.212	2.92
Discharged since 1970	–.156	–.152	3.65

Multiple R = .32 Unweighted N = 533

[a]Those maintaining a firm stand are coded "1"; others are coded "0."

Vietnam duty personnel to be attitudinally distinct from those who had spent less time in the war theater. Although the number of cases is small, we do find that more than one-fourth of these intensely exposed veterans avowed that their experience caused them to lose faith in political and military leaders, a result consistent with our earlier account of political trust.

A similar pattern is found with respect to negative references about U.S. foreign policy. The longer a serviceman was in Vietnam, the more inclined he was to state that the war led to a loss in his confidence about American foreign policy. Specific reactions included such comments as "The government should not be the world's policeman" and "The U.S. interferes too much in the affairs to other countries." Veterans just returning to civilian life were also relatively more prone to volunteer such judgments, probably for the same reasons that they were less approving of a vigorous war policy. Neither of these two relationships are very strong, but they suggest how the intensity and the timing of military experience can color one's policy positions.

In our discussion of the findings on civic tolerance we noted that the strength of some service-related corollaries of civic tolerance was reduced in the regression analysis, but that there were still practical, policy-relevant implications in these corollaries. Much the same can be said about the opinions and supporting arguments about the Vietnam War. In particular, the positions taken by the more recent returnees and the men who saw extended service in Vietnam placed them at variance with their fellow servicemen. To the extent that these two groups were more vocal and more attended to than were other servicemen—and there is at least impressionistic evidence that they were—their views came to weigh more heavily in the political process. This would seem to be especially true for the recently returned veterans in this cohort, with their high proportion of college-educated men.

Conclusion

Our interest in the effects of military service grew out of our larger interest in the impact which institutional experiences can have on political attitudes.[42] The military was of special interest because it touches the lives of so many men at a still tender age, because it is a total institution acting in the name of the state, and because, for most of its members the military as an institution is a transient, non-voluntary experience. The basic question was whether certain political attitudes were altered and shaped among a cohort of young American men as a result of their serving during the Vietnam War period. To answer this question we examined panel data comparing veterans and nonveterans; and among the former we considered varying experiential histories.

To summarize, gross comparisons between those serving and those not serving point toward modest effects at best. These were most noticeable in the areas of political trust and attention frames. A working premise, however, was that the simple distinction between service and nonservice was too crude a cutting tool and that it would be necessary to make finer distinctions. Specifically, we took into account the duration, intensity, recency, affective orientation, and salience of the serviceman's institutional experience. With the possible exception of the salience factor, each of these components at some point and with respect to certain attitudes took on significance.

Not surprisingly, the components did not always function in the same manner across the various political attitudes. Given the variety of attitudes we examined and the interplay between pre-existing attitudes, secular shifts, and life cycle developments, there is little reason to expect uniformity of effects. What should be emphasized is the importance of specifying gradients and nuances in the institutional experience. Indeed, our analysis suggests that we might have specified further effects, such as those derived from the component of timing, i.e., the precise historical period in which service was rendered.

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[42] We have treated the effects on behavior in our "Political Participation and Vietnam War Veterans: A Longitudinal Study," in *The Social Psychology of Military Service*, ed. Nancy L. Goldman and David R. Segal (Beverly Hills: Sage Publications, 1976).

Without denying the impact of varying military experiences, it is still true that their effects are not statistically large. Although in some instances the first glance suggested strong effects, these were usually reduced or vanquished with the introduction of relevant controls. To some degree, these results are reassuring, since evidence that the political outlooks of veterans were markedly different from those of other citizens would be cause for concern. But the question remains: Why is the impact of military service on political attitudes not stronger? Among a number of possible answers, three stand out.

First, the military does not work with empty slates when it receives its young recruits. They come in with sets of pre-existing attitudes, although these attitudes are often labile. As our regression analyses reveal, these pre-service attitudes are ordinarily important predictors of the comparable postservice attitudes. These pre-existing states of mind constrain the potential impact which even a "total" institution such as the military might have.

A second reason for modest impact on political attitudes is that a number of other characteristics and experiences are affecting servicemen during and after their period of service. For example, the regression analyses clearly showed educational attainment to influence several of the attitudinal indicators used. Thus the military institution competes to some degree with educational institutions as a molder of attitudes. Similarly, ascribed characteristics, such as race, are important for certain attitudes. In addition, being in the military does not completely isolate servicemen from the currents flowing in the "outside" world. They are not impervious to widespread sociopolitical trends.

Third, it should be recognized that for most men being in the service, especially during a war, is not instrumental to the achievement of personal goals. Rather, it is seen as an obstruction, an unwelcome intrusion. Thus any formal structures or processes designed for the purpose of modifying or inculcating broader political values and attitudes will encounter a reluctant membership. By the same token, even though informal social interaction may be important in the short-run, the transience of the interaction reduces the likelihood of long-range effects.

Having said all this, we must not forget that for particular individuals the political consequences of military service may be severe and even traumatic. And on a more general level, we have found that in some instances military-related variables have a greater impact on political attitudes than do such "all-purpose" predictors as race, parental SES, or education. Our conclusions and reflections are, of course, limited by the nature of the study, the time frame, and the particular orientations dealt with. It will be necessary to consider other types of political orientations as well as more general outlooks which have direct lines to political life. Finally, to put the effects of military service into proper perspective, it will be useful to consider the effects of other institutional experiences.

Bureaucratic Response to Citizen-Initiated Contacts: Environmental Enforcement in Detroit*

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One of the more significant developments in political science in the 1970s has occurred in the study of political participation. As in most intellectual movements, the change has been gradual and cumulative rather than abrupt; but the new emphasis has recently received several particularly clear expositions. We refer to the increasing tendency to view political participation as instrumental activity, designed by the participant to extract some benefit from governmental authorities. Verba and Nie conceive of participation as "acts that aim at *influencing* the government, either by affecting the *choice* of government personnel or by affecting the *choices made* by government personnel."[1] When citizens participate in elections or campaign activities, they attempt to affect the choice of which individuals will serve in the capacity of decision makers. When they participate in nonelectoral activities, they generally wish to affect the decisions made by governmental officials, whether they are elected or appointed officials, or are civil servants—all represent authorities who can deliver benefits to citizens. When active in such nonelectoral activities, the citizen may engage in cooperative activity by joining with other citizens toward achieving a desired goal, or he may contact officials individually; Verba and Nie refer to this later form of participation as "citizen-initiated contacts."[2]

Although citizen-initiated contact has received little study in comparison to electoral behavior or interest-group activity, such contact may be quite important in maintaining governmental responsiveness. Peter Eisinger views citizen contact as a critical component of the representational system. "A citizen contact . . . is a demand on the part of the represented for consideration of some interest in which he has a perceived stake. Contact is made necessary in cases in which political society has delegated to government the authority to regulate, protect, or advance certain interests."[3] Verba and Nie emphasize the differences in participation via citizen contact and participation through electoral channels. "Our data on the content of citizen-initiated contacts show a citizenry involved with the government in ways that are highly salient to them, on issues that they define, and through channels that seem appropriate. What we are suggesting is that on matters of the politics of everyday life, citizens know what they want."[4] This contrasts vividly with electoral participation, where the citizen can at best choose among a limited set of competing agendas; or, even worse, choose between competing candidates advocating identical agendas. But Verba and Nie are not satisfied with descriptive generalizations concerning the difference in modes of participation. They argue that citizen contacts represent "an alternative way of simplifying social choice."

> This is done by decomposing the choice to the individual level. These contacts often deal

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*We wish to express our appreciation to Professor Charles Parrish, formerly Chairman of the Department of Political Science, Wayne State University, for his continued assistance during the course of this project. The data analysis was conducted through grants provided by the Wayne State Computing and Data Processing Center and the Department of Political Science. Finally, we wish to thank the Office of the City Clerk and the Environmental Enforcement Division of the (then) Department of Public Works for their cooperation.

[1] Sidney Verba and Norman H. Nie, *Participation in America: Political Democracy and Social Equality* (New York: Harper and Row, 1972), p. 2.

[2] *Ibid.*, chapter 3.

[3] Peter K. Eisinger, "The Pattern of Citizen Contacts with Urban Officials," in *People and Politics in Urban Society*, ed. Harlan Hahn (Beverly Hills, California: Sage Publications, 1972), p. 64.

[4] Verba and Nie, p. 113.

> with particularized problems; in many cases the response to a contact would have major impact on the individual without affecting the overall allocation of societal benefits in more than marginal ways. But the sum total of all such contacts and the myriad responses to them do represent a mechanism for social allocation without the clear necessity of general social choice.[5]

It would have perhaps been more appropriate if the argument had read "*can* represent a mechanism for social allocation," since the degree of governmental responsiveness to contacts is an empirical question, influenced by the character of the political system, the content of the contact, and the nature of the contactor. The weakness of the few existing studies we have on citizen contacts is that they have not been able to assess the response of government to the citizen contact. Using survey research, several studies have assessed the extent of contact attempts, the characteristics of the contact-initiator, and the satisfaction of the contactor with the results.[6] Survey research is the best way to ascertain the characteristics of contactors, although it relies totally on the memory of the respondent concerning the existence and nature of the contact. It is also the best way to assess general levels of satisfaction with policy outcomes or service delivery, but these two variables cannot be linked on the basis of survey evidence alone. In the first place, satisfaction may be a function of general life circumstance rather than public policy outcomes or services delivered. Furthermore, some groups may experience greater dissatisfaction than others even though governmental authorities respond to citizen-initiated contacts in a standard manner regardless of the social source of the contact. The dissatisfaction may result from a lack of delivery of services because groups of citizens have differential propensities to contact service agencies. This can clearly result in a greater quantity of a service being delivered to high contact neighborhoods. The response of government to citizen contacts may often be better assessed by studying government records, especially when the response is delivered by record-conscious public bureaucracies, as frequently happens.

In this paper we attempt to link the individual contact with government to the response of government to the contact. First we develop a theory of the mechanisms that are responsible for the generation of citizen-initiated contacts, based on the *need* for the service or policy which government is capable of providing and on the *awareness* of citizens that governmental action is able to ameliorate the need. Then, using governmental records of citizens contacts, we present data relevant to the testing of this theory. Finally, governmental records are again used to examine the service agency's response to the individual contact. The records examined were all contacts which were processed by the Office of the City Clerk of the City of Detroit in the general category of environmental enforcement in 1973. At that time, the Office of the City Clerk bore the primary responsibility for handling citizen contacts which did not go directly to the city agency responsible for the functions in question. Environmental enforcement complaints, along with contacts concerning building safety, composed the largest categories of contacts processed by the Complaint Bureau of the Clerk's Office. In 1973, some 2,368 citizen contacts concerning environmental enforcement were processed by the Office. We have supplemented this data with records from the Environmental Enforcement Division for the months of July and October of 1973. Such use of multiple data sets should yield increased confidence in the findings, if they are similar. If they are not, we may suspect bias in record-keeping, and may be able to assess the direction of the bias. In public bureaucracies in America, however, record-keeping is more likely to reflect negligence than bias; this adds random error to the data, but will not result in biased findings.

A Theory of Citizen-Initiated Contacts

A large class of services provided by government is distinguishable in that these services confer particularistic benefits on citizens. While such services can obviously have collective benefits when examined in total, each government-to-citizen contact can confer immediate benefits on a very limited number of citizens at most. If, for example, an abandoned automobile is removed by order of an environmental inspector, the immediate neighborhood is most benefited, although all citizens can be benefited from the aggregate activities of all inspectors, since the city may be cleaner than would otherwise be the case. Many of the services provided by local governments fall into this category. Such services may be delivered on site, directly to the citizen or his neighborhood, or they may be delivered from fixed facilities. Of those services delivered on site, such as police and fire protection, garbage collection, and enforcement of building codes and environmental ordinances, provision may be made via some bureaucratic routine or on request by citizens. Many local bureaucracies employ both modes of delivery; police, for example, provide both for citizen requests for service and for routine patrol. In the discussion which follows, we limit ourselves to the consideration of citizen-initiated contacts in service areas which provide immediate, visible particularistic benefits to citizens.

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[5] *Ibid.*, p. 112.

[6] See Eisinger; Verba and Nie; and Herbert Jacob, "Contact with Government Agencies: A Preliminary Analysis of the Distribution of Government Services," *Midwest Journal of Political Science,* 16 (Feb., 1972), 123–146.

Most investigations of participation have begun with what Verba and Nie term the standard socioeconomic model, according to which occupation, income, and education have primary influence on the degree of participation engaged in by individuals.[7] The argument is that individuals of higher status develop attitudes and skills conducive to successful participation. Successful participation further encourages the development of skills and attitudes which increase participation further, and so forth. The basic outlines of this generalization have been confirmed repeatedly in the literature. When Verba and Nie examined their four modes of participation separately, however, they found some puzzling results. The standard socioeconomic model worked quite well for campaign activity, and communal activity, and only slightly less well for voting. But it worked much less well for particularized contact, that is, citizen-initiated contact with particularistic referents.[8]

The failure of the standard model in explaining citizen-initiated contacts with particularistic referents leads to a consideration of other factors which produce such contacts. One of the curious omissions of the classic literature on participation is any discussion of the individual's need for the policies or services. This omission has probably occurred for two reasons. First, voting and campaign activity have been the most intensely studied acts of participation, and such activities are only tenuously connected to the perceived or objective needs of the participants. Second, the power of the standard socioeconomic model in explaining voting and campaign activity has perhaps led investigators away from studying the relationship between need and participation. If socioeconomic status and need are inversely related, as often seems the case, those who need governmental services most participate the least.

Wayne Cornelius has developed a model of the causal antecedents of political demand-making, which he conceives as "individual or collective activities aimed at extracting certain types of benefits from a political system by influencing the decisions of incumbent governmental officials."[9] Cornelius's work is noteworthy in two respects. First, he focuses on the role of objective needs in generating participation. Second, he notes that the translation of needs into political demand-making is not automatic. Objectively defined needs are translated into political demand-making only if the individual perceives the needs as requiring ameliorative action, perceives the government as capable of satisfying his needs, and perceives a channel for influencing the government to act on his needs. Cornelius goes on to investigate "major lags" in the link between objective needs and demand-making among migrants to Latin American cities.

Since need and awareness are likely to be linked to socioeconomic status, it is possible to derive the relationship of status to participation via these two intermediaries. We begin by offering two relatively noncontroversial propositions. For the present we focus on relations likely to exist in urban areas, because most of the particularistic services relevant to this discussion are delivered by local governments. It is obvious, however, that the argument can be extended to the social structure more generally.

I. *The need for most governmental services of the particularistic type we are considering is inversely related to social well-being.* Social well-being is likely to be related rather strongly to socioeconomic status, but the two are not identical. Specifically, racial discrimination, particularly in the real estate market, may force middle-income blacks, relatively well-off socioeconomically, to accept housing in deteriorating neighborhoods. In this case, the use of income as an index would overestimate the social well-being of this group. Another example of the divergence of socioeconomic status and social well-being is the existence of white, working-class neighborhoods on the fringes of a city. These neighborhoods may be *buffered* from many of the city's problems by middle class black or white neighborhoods; thus the social well-being of the neighborhood may be underestimated by its income or occupational level. In general, however, the two concepts of social well-being and socioeconomic status are likely to be intertwined.

Even though Proposition I is intuitively compelling, direct empirical evidence is not as easy to come by as one might think. Studies conducted by urbanists since the zenith of influence of the Chicago school have examined the spatial coincidence of various urban problems (juvenile delinquency, crime, drug abuse, civil disorder, etc.) and other social conditions (income, education, occupation, housing, etc.). Such studies indicate that the areas of a city which have high concentrations of various social problems also are characterized by lower income and educational levels, housing quality, and the like.[10] To the degree that governmental policies are directed toward alleviating social problems, and to the degree that social well-being and the concentration of social problems are inversely related, *need* for the delivery of services which implement the policy and social well-being will be inversely related.

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[7] Verba and Nie, chapter 8.

[8] *Ibid.*, pp. 135–36.

[9] Wayne A. Cornelius, "Urbanization and Political Demand-Making: Political Participation Among the Migrant Poor in Latin American Cities," *American Political Science Review*, 68 (September, 1974), 1125.

II. *Social well-being is directly related to the awareness of government as an instrument for alleviating a social need.* A number of studies have found that persons of high status and education tend to have more knowledge and greater sophistication about government and politics than do their lower-status counterparts.[11] This probably holds true in the case of particularistic public services as well.

Participation in the form of citizen-initiated contacts with governmental officials is likely to be a function of both the need for the service and the awareness that government is the appropriate provider of the service. But the relationship is evidently not a simple additive one, since it would seem that at least some need and some awareness would be necessary for any contact to occur. No matter how intense the need, unless citizens recognize that government can help meet that need, contacts will not be generated. The same would seem to be true of awareness; a person would have no reason to contact government about a service if he did not need it, even though he knew that government was responsible for providing it. That is, there is an *interaction* between need for a service and awareness of government as relevant to the need in the generation of citizen contacts.

If we can mathematicize the relationships postulated in the above discussion, we will be able to derive the relationship between social well-being and the propensity to contact. We postulate that a simple positive linear relationship characterizes the relation between social well-being and awareness, while a simple negative linear equation will describe the relation between social well-being and need. These assumptions, which are mathematical specifications of Propositions I and II, are not likely to be completely correct empirically, but they serve as useful first approximations to the situation.

$$A = C_1 + K_1 S \qquad (1)$$

$$C_1, K_1, C_2, K_2 > 0$$

$$N = C_2 - K_2 S \qquad (2)$$

Here, A is awareness, N is need, and S is social well-being. The assumption that there is an interactive combination between needs and awareness which produces citizen-initiated contacts may be represented by the following multiplicative equation:

$$P = C_3 A N, \qquad (3)$$

where P is propensity to initiate contact. We may now substitute Equations 1 and 2 into Equation 3.

$$\begin{aligned} P &= C_3(C_1 + K_1 S)(C_2 - K_2 S) \\ &= C_3(C_1C_2 - C_1K_2S + C_2K_1S - K_1K_2S^2) \\ P &= C_3C_2C_1 + C_3(C_2K_1 - C_1K_2)S \\ &\quad - C_1K_1K_2S^2 \qquad (4) \end{aligned}$$

or, substituting for the indeterminate constants,

$$P = D + B_1 S - B_2 S^2. \qquad (5)$$

Taking the first derivative, and setting it equal to zero,

$$\begin{aligned} f'(P) &= B_1 - 2B_2 S = 0 \\ 2B_2 S &= B_1 \\ S &= \frac{B_1}{2B_2} \qquad (6) \end{aligned}$$

P therefore has a critical point at $S = B_1/2B_2$. This will be a positive number (indeed, since social well-being cannot assume negative numbers, it makes sense only when it is positive) when both B_1 and B_2 are positive. B_2 is positive since it was assumed that all the constants in Equations 1 and 2 were positive. B_1 is positive whenever $C_2K_1 > C_1K_2$. This is a most likely occurrence, since C_2 must be a large number relative to C_1 and there is no reason that K_1 and K_2 would differ by a large amount.

We need to know whether the critical point is a maximum or a minimum point for the curve. Taking the second derivative,

$$f''(P) = -2B_2, \qquad (7)$$

which is everywhere negative. This indicates that the critical point is a maximum point; the curve relating P to S is therefore a downward-opening curve with a maximum point at $S = B_1/2B_2$. (Eq. 5 is in fact the general equation for a downward-opening parabola).

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[10] Several such studies are summarized in David M. Smith, *The Geography of Social Well-Being in the United States* (New York: McGraw-Hill, 1973), chapter 4; see also chapter 9.

[11] Lester W. Milbrath, *Political Participation: How and Why Do People Get Involved in Politics?* (Chicago: Rand McNally, 1965), p. 68.

Given the assumptions, we have derived a relationship between the propensity to initiate contacts and social well-being. If one were to travel from neighborhoods most deprived in terms of social well-being toward those more favored, he should observe an increase in the propensity of citizens to contact officials about particularistic services until he reached the point $B_1/2B_2$. From that point. propensity to initiate contact should decline to the empirical limits of social well-being. This is illustrated in Figure 1.

Method

The most fruitful tool in the study of political participation at the present time is survey research. It has several major advantages, among them that it allows the researcher to correlate demographic characteristics and political attitudes with each other and with acts of participation, up to the limit of the recall ability of the respondent. In the study of citizen-initiated contacts, however, the survey method has several distinct disadvantages. First, in a sample which attempts to be representative, one is likely to net only a small minority who have actually contacted governmental officials. The Verba and Nie study reported this group to compose about 20 per cent of their sample, but this included all contacts, not just contacts concerning particularistic services which is our focus here.[12] Second, we are dealing with activities of individuals which are particularly susceptible to faultiness of recall. The acts which the respondent would be asked to remember are not those associated with the emotional mobilization of national elections, but mundane tasks like calling the sanitation department. Third, the method as it is currently employed cannot give us thorough knowledge of the nature of the governmental response.

In this study we employ governmental records to examine citizen-initiated contacts and

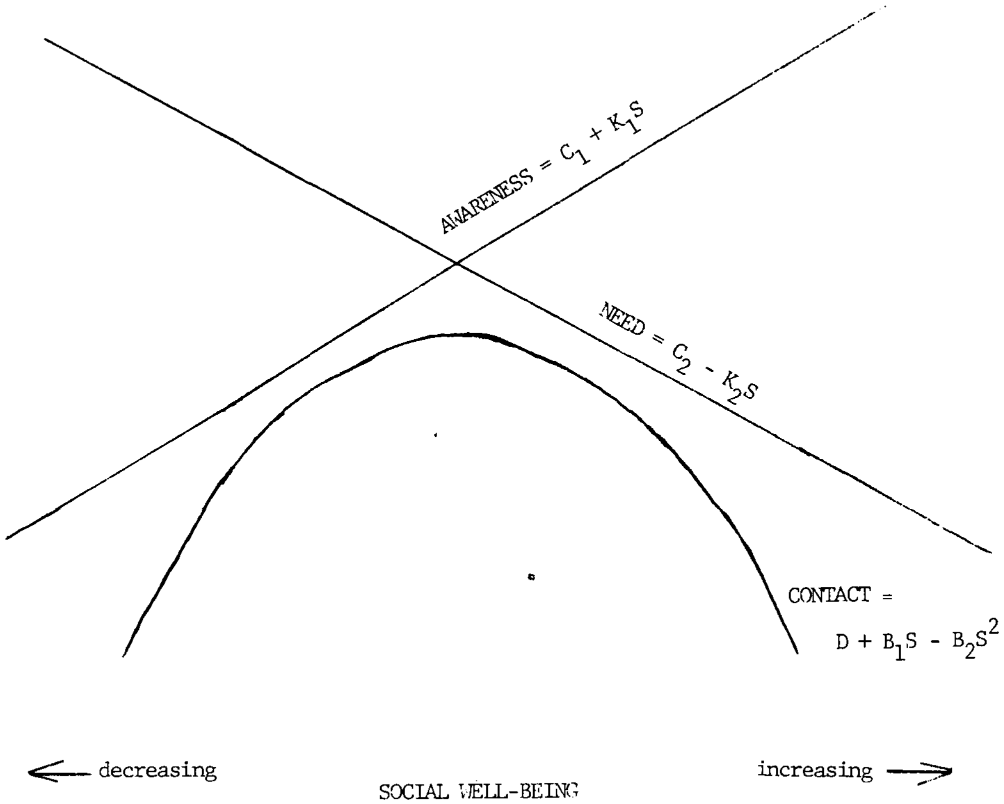

Figure 1. Theoretical Relationships Between Social Well-Being and Citizen-Initiated Contacts

S indicates Social well-being; C_1, C_2, K_1, K_2, D, B_1, and B_2 are constants.

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[12] Verba and Nie, 31. Studies in urban areas alone suggest slightly higher levels of citizen contact. See Joel D. Aberbach and Jack L. Walker, "The Attitudes of Blacks and Whites Toward City Services: Implications for Public Policy," in *Financing the Metropolis*, ed. John P. Crecine, (Beverly Hills, California: Sage Publications, 1970), pp. 519-37, and Peter Eisinger, "The Pattern of Citizen Contacts with Urban Officials," cited in note 2, above.

governmental response. Obviously, an advantage of this method is that it permits one to examine contacts without the interference of faulty recall. Further, the governmental response as it is recorded can be traced. The major disadvantage is that one cannot associate individual demographic or attitudinal characteristics directly with the act of participation. We can only associate the act with the aggregate characteristics of the area in which the contactor lives. A second potential disadvantage arises from relying on the accuracy of governmental records. The general problem is the lack of care with which some, but by no means all, local governmental organizations assemble records. This type of measurement error is almost surely random, and thus will not bias results gleaned from such records. Indeed, it may present less of a problem than recall errors in survey investigation, since recall may well be associated with such variables as education and income.

Our approach to the inadequacy of the government's recording of its response to citizen contacts might be considered a form of triangulation; we employ three different data sets drawn from two city bureaucracies. One data set consists of the 2,368 citizen contacts concerning environmental enforcement processed by the Office of the City Clerk of the City of Detroit. During the period of this study, the City Clerk's Office received complaints and requests concerning city services, and forwarded a form recording the nature of the complaint to the appropriate city bureaucracy. The bureaucracy handled the problem, and routed the form back to the City Clerk with a notation of the action taken. The address of the contactor, along with the address to which the governmental response was directed, were noted on the form. In the case of the Environmental Enforcement Division, which bears primary responsibility for enforcing local ordinances concerning litter, weeds, rodent control, and other environmental abuses, most responses are directed at a location in the immediate vicinity of the address of the contacting citizen's address. The existence of these addresses allowed us to merge the data from the Office of the City Clerk with data on Detroit's 420 census tracts. We have supplemented the standard census variables with several additional variables, as will become clear later in this paper.

Our other two sources of data were the records of the Environmental Enforcement Division for July, 1973, and for October of that year. Data in that bureaucracy are collected on monthly summary sheets for the forty or so full-time investigators employed by the Division. Each inspector covers a different section on each day of the week; there are thus about 200 sections in the city on which data is available. At this point, there are two possibilities. One can aggregate census tract data up to the section level, or one can disaggregate the section data to the census tracts. We have chosen the latter strategy, for theoretical reasons.[13] If the unit to which services are delivered is kept constant, one can readily compare service responses from more than one data set, or more than one bureaucracy. The strategy has costs, however. In the first place, one must assume that contacts, whether citizen-to-government or government-to-citizen, are homogeneously distributed across the section. Second, and more seriously, one invites statistical problems in the data analysis, since the number of census tracts does not correspond to the number of independent pieces of information available from the governmental sources, and there is no sensible way to adjust for this. In the following analysis, the primary statistical technique used is regression analysis. Since the independence assumption is not involved in the derivation of the estimators of the regression parameters, the resulting findings will not be biased due to this source.[14] However, one must assume independence to derive standard formulas for the variance and standard errors of the estimators, and these may be somewhat understated in our results.[15]

Assessing Social Well-Being in the City. If we were to plot social well-being against citizen-initiated contacts, we would expect a downward-opening curvilinear plot. But how is social well-being to be measured? From a variety of possible measures, we have chosen two which we believe to be most preferred out of the available alternatives: the distance in miles from the central business district (the foot of Woodward and Jefferson, for those who know Detroit) to the center of the census tract, and a measure of the "newness" of the housing structures in census tracts. The latter measure was constructed by subtracting the average age of housing structures in tracts from 75 years, the average age of the housing in the tract having the oldest housing. This transformation makes the variable a direct measure of social well-being, with zero assigned to the tract with the oldest housing, and 55 assigned to the tract with the most recent housing. The variable then assesses the number of years between the date of construction of the housing in the oldest tract to the date of construction in the tract in question.

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[13] This apportioning was accomplished using the city block to ascertain the proportion of the service area data assigned to each census tract. For a complete description, see Joseph Drew, Saadia Greenberg, and Bryan Jones, "The Development of a Data Systems Technology for Assessing the Geographic Distribution of Urban Government Services (paper presented at the annual conference of the Urban and Regional Information Systems Association, Atlanta, Georgia, August 29–September 1, 1976).

[14] Thomas H. Wonnacott and Ronald J. Wonnacott, *Introductory Statistics,* second edition (New York: John Wiley, 1972), pp. 268–69.

[15] We are aware that our strategy may compound the usual problems of spatial serial correlation one can encounter in using data which is geographically based. On these problems, see Alexander Lebanon and Howard Rosenthal, "Least Squares Estimation for Models of Cross-Sectional Correlation," *Political Methodology,* 2 (May, 1975), 221–244. The present state of theory does not allow us to specify an adequate model of the disturbances, which is necessary to employ the techniques discussed by Lebanon and Rosenthal.

These two variables were chosen for two reasons. First, they are "safe" variables, in that they are not subject to radical change over time. Traditional census variables are liable to change between the time of their collection and the time of the collection of the data from a public bureaucracy. In Detroit, the period 1970 to 1973 was a period of substantial neighborhood succession; such changes differentially affect such census variables as per cent black, per capita income, and so forth. On the other hand, the distance measure is not subject to change over time, while the housing measure changes at a constant rate for all census tracts simultaneously.[16] These measures are, then, more *reliable* than traditional census materials. Second, particularly for Detroit in the 1970s, these variables are *valid* measures of social well-being. We have already noted that indicators of socioeconomic status may imperfectly assess social well-being. Further, regarding the distance measure, the urban geographer Robert Sinclair notes that the classic concentric zonal model of urban land use fits Detroit better than other models, and better than it fits other North American cities.[17] The concentric zone model, given its formalization by E. W. Burgess, suggests that the desirability of residential areas increases with distance from the central business district.[18] The housing measure complements the distance measure partly as a consequence of the concentric land use pattern in Detroit. The older residential sections of the city have fairly uniformly declined in quality, and only feeble efforts at restoration have been made. Thus the more desirable residential areas are almost always newer. The intercorrelation between these variables is .922. The intercorrelations of each of these variables with selected census variables are presented in Table 1.

Citizen-Initiated Contacts and Social Well-Being

Figures 2 through 5 are plots of social well-being, assessed by either distance or housing, versus the number of contacts (on a log scale) made by citizens residing in neighborhoods (census tracts) characterized by various levels of the social well-being variable. In those plots we have grouped on the social well-being measure, since raw scatterplots for the large number of data points are quite unmanageable; the vertical bars indicate variability in the data. Because of this strategy, the end groups may be

Table 1. Intercorrelations of Two Measures of Social Well-Being with Selected 1970 Census Variables, Detroit

Census Variable	Distance from the CBD[a]	Housing[b]
Per Capita Income	.437	.459
% Employed as Professionals or Managers	.609	.610
% Population in Poverty-Level Families	–.711	–.727
% Housing Units Vacant	–.707	–.749
Average Value of Owner-Occupied Units	.534	.565
Average Rent	.712	.708
% Units with Washing Machine	.813	.833
% Black	.459	.431
Average School Years Completed	.615	.590

[a] Distance from the central business district to the center of the census tract, in miles.

[b] Average "newness" of housing structure = 75 years minus average age of housing structures in the census tract.

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[16] This is not entirely true of the distance measure. Over time there may be a shift in the meaning of the variable. The core city may be rebuilt for middle-class residents, the poor may be forced outward. But these changes are likely to be glacial compared to the change wrought by neighborhood succession.

[17] Robert Sinclair, *The Face of Detroit: A Spatial Synthesis* (Detroit: Department of Geography, Wayne State University, 1972), pp. 46–53.

[18] David Herbert, *Urban Geography: A Social Perspective* (New York: Praeger, 1972), pp. 70–71.

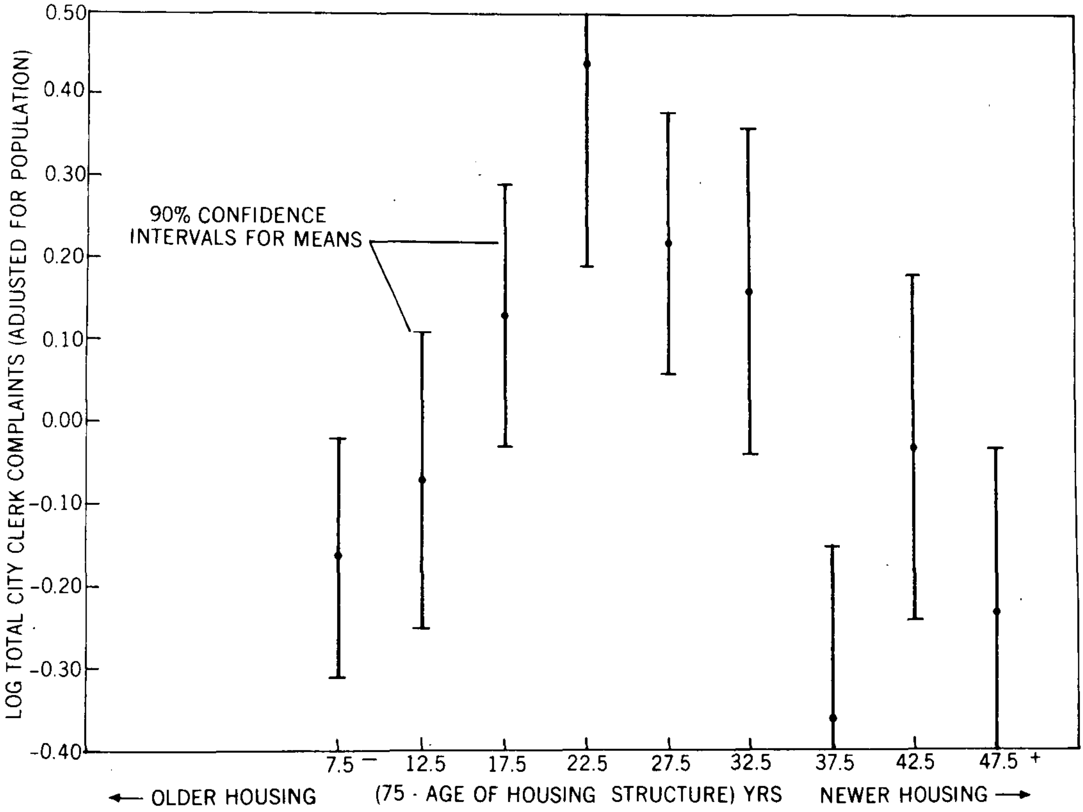

Figure 2. Citizen-Initiated Environmental Contacts and Housing Structure

slightly misplaced on the abscissa (but not the ordinate). The number of citizen contacts per tract has been transformed by taking the natural logarithm (after adding unity, to eliminate zeroes); count data are almost always improved by taking logarithms, according to Acton.[19] In these data, some tracts have an inexplicably large number of contacts. Taking logarithms causes these "outliers" to move relatively closer to the main body of the data. These deviant cases are not the result of poor measurement, so far as can be determined; they ought to be left in the analysis. Taking logs helps, although they still contribute to variability in the plots.

Before plotting, the effects of varying population in census tracts had to be taken into account. This could have been accomplished by forming a ratio of the number of complaints to tract population, but this procedure would have caused relatively high scores to be assigned to tracts with few complaints and a small population. Our solution to the problem has been to take the influence of population out by regressing contacts on population, and using the residuals from this regression to assess citizen contacts, thereby adjusting for population differences. The analysis has also been repeated using number of complaints per 1000 population as the dependent variable; the results are essentially identical to those reported below.

Figures 2 through 5 clearly reveal the expected downward-opening curves. In the cases of total contacts filtered through the City Clerk's Office and direct Environmental Enforcement contacts for July, the curve starts and ends at about the same level of complaints, rising to a single maximum between the two endpoints. In the case of calls to the City Clerk's Office which related to rodent control, and for all complaints called in to the Environmental Enforcement Division (EED) during October, the curve starts higher at low levels of social well-being, rises to a maximum, and then drops to a level lower than that at which it began. There is a good deal of variability in the data, as indicated by the confidence intervals for group means; this is particularly true for the City Clerk data. Even though this variability indicates that citizen contact with government is incompletely explained by our model, whether because of incomplete theory or inadequate measurement, it makes even more impres-

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[19] Forman S. Acton, *Analysis of Straight-Line Data* (New York: John Wiley, 1959), p. 223.

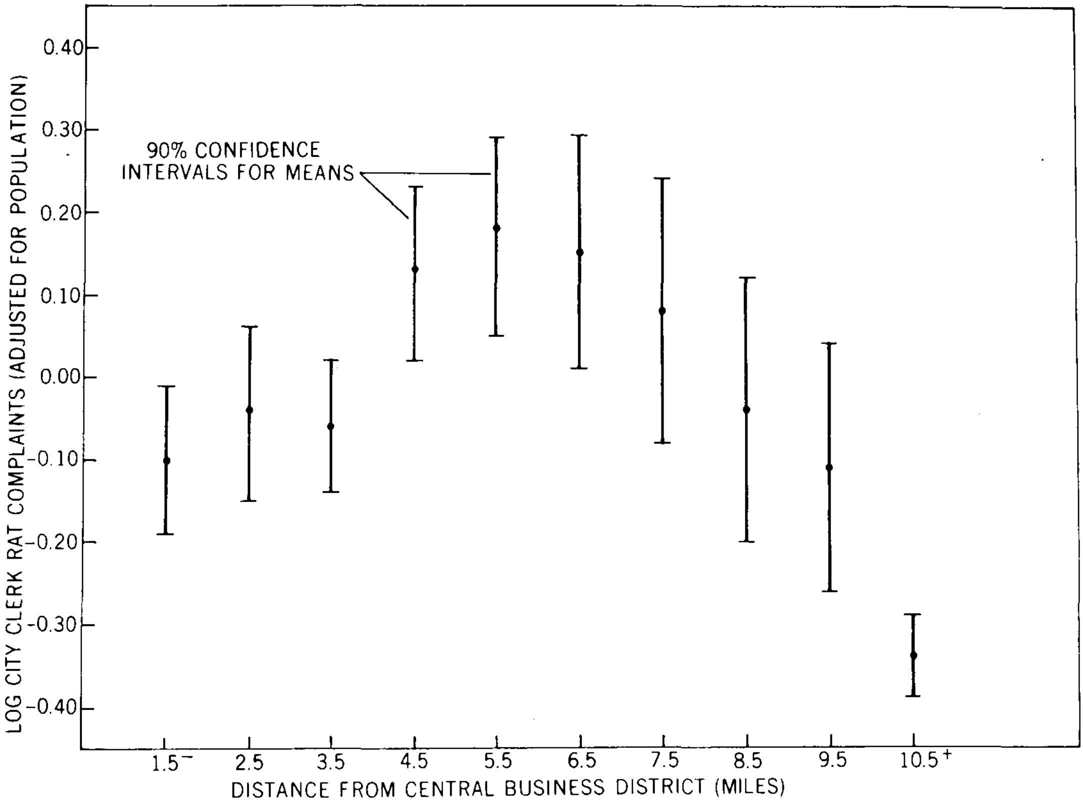

Figure 3. Citizen Contacts Concerning Rodent Problems and Distance from the Central Business District

sive the finding that the characteristic curve is clearly visible in the three diverse data sets.

We can extend the analysis by quantifying the relation between citizen contacts and social well-being through regression analysis. If, say, distance and the square of distance are both entered into a regression with contacts as the dependent variable, one would expect a positive coefficient for distance, and a negative coefficient for distance squared. Such an equation describes a downward-opening parabola (see Eq. 5, above). In Table 2, results from several such regression analysis are presented. Both distance and housing serve as independent variables in explaining citizen contacts in separate regression equations. Citizen contacts are assessed via the City Clerk data, as well as both EED data sets. In addition, citizen contacts concerning rodent control, which are part of the total complaints called into the Office of the City Clerk, serve as the dependent variable in one set of regressions. Since this function is handled by a separate work section, it seemed desirable to see if the relationship maintained itself in this segment of the data. Not only do the slope coefficients take on the expected sign in each of the eight regression equations, but the coefficients are always statistically significant, as is indicated by the associated *t*-value. The comment regarding independence of measurement mentioned above is relevant, however; *t*-values may be somewhat inflated.

This is persuasive evidence for the hypothesis of the expected parabolic relationship between social well-being and citizen contacts in the environmental enforcement field. But we can go even further. First, in each of the four categories of citizen contacts concerning environmental enforcement called into the City Clerk's Office in 1973 (litter, weeds, rats, and containers), the characteristic parabola was in evidence for both of the indicators of social well-being. Second, for other indicators of social well-being, the parabolic relationship was in evidence for total city clerk complaints, and for July and October EED contacts. For example, when per capita income was employed as an independent variable, the curves were substantially identical to those presented in Table 2. There was, however, a little more residual variation in evidence.

Note that the correlations between values predicted from the equations in Table 2 and the actual values of the dependent variable are not large, especially for the City Clerk data; the range is from about .21 to .43. There are likely

Table 2. Regression Equations for Citizen Contacts and Social Well-Being[a]

LN (TOTAL) =	−0.5023	+ 0.2239 DISTANCE	− 0.0189 DISTANCE^2
std. error:	(0.1568)	(0.0532)	(0.0040)
t-value:	−3.20	4.20	−4.71
$N = 420$; $R = .235$			
LN (TOTAL) =	−0.447	+ 0.0496 HOUSING	− 0.0010 HOUSING^2
std. error:	(0.1681)	(0.0138)	(0.0002)
t-value	−2.66	3.58	−4.00
$N = 417$; $R = .207$			
LN (RODENT) =	−0.2975	+ 0.1425 DISTANCE	− 0.0126 DISTANCE^2
std. error:	(0.0957)	(0.0325)	(0.0025)
t-value:	−3.11	4.38	−5.12
$N = 420$; $R = .266$			
LN (RODENT) =	−0.3604	+ 0.0380 HOUSING	− 0.0007 HOUSING^2
std. error:	(0.1023)	(0.0084)	(0.0001)
t-value:	−3.52	4.52	−4.96
$N = 417$; $R = .248$			
LN (JULY) =	−0.2602	+ 0.1301 DISTANCE	− 0.0118 DISTANCE^2
std. error:	(0.1099)	(0.0362)	(0.0027)
t-value:	−2.37	3.60	−4.44
$N = 381$; $R = .266$			
LN (JULY) =	−0.5644	+ 0.0603 HOUSING	− 0.0012 HOUSING^2
std. error:	(0.1023)	(0.0082)	(0.0001)
t-value:	−5.51	7.32	−8.13
$N = 379$; $R = .408$			
LN (OCTOBER) =	−0.5027	+ 0.2605 DISTANCE	− 0.0256 DISTANCE^2
std. error:	(0.1069)	(0.0391)	(0.0033)
t-value:	−4.70	6.67	−7.82
$N = 343$; $R = .429$			
LN (OCTOBER) =	−0.3009	+ 0.0395 HOUSING	− 0.0009 HOUSING^2
std. error:	(0.1052)	(0.0085)	(0.0002)
t-value:	−2.86	4.64	−5.70
$N = 341$; $R = .363$			

[a] The variables tabulated above are as follows:

A. *Social Well-Being:*

DISTANCE = Distance from the central business district to the center of the census tract, in miles.

HOUSING = "Newness" of housing structure in years = 75 years minus average age of housing structures in the census tract.

B. *Citizen Contacts:*

TOTAL = Total environmental enforcement contacts processed by the Office of the City Clerk for the year 1973.

RODENT = Total contacts concerning rodent problems processed by the Office of the City Clerk, 1973.

JULY = Citizen contacts from all sources processed by the Environmental Enforcement Division, July, 1973.

OCTOBER = Citizen contacts from all sources processed by the Environmental Enforcement Division, October, 1973.

The contact variables were each adjusted for the influence of varying population sizes of census tracts by using the residuals from the regression of the contact variable on population.

LN refers to the natural logarithm. Unity was added to each value of each variable before the log transformation.

to be other conditions and reasons which cause people to complain than those assessed by our model. Further, measurement error is a distinct possibility. In particular, we cannot assess directly the objective need for the services provided by the Environmental Enforcement Division; the simple linear equation which we have postulated as describing the relationship between need and social well-being is likely to be somewhat in error. The hypothesized relationships are supported more by the consistency of the empirical findings than the ability to account for variance in citizen contacts. Specifically one ought to compare the

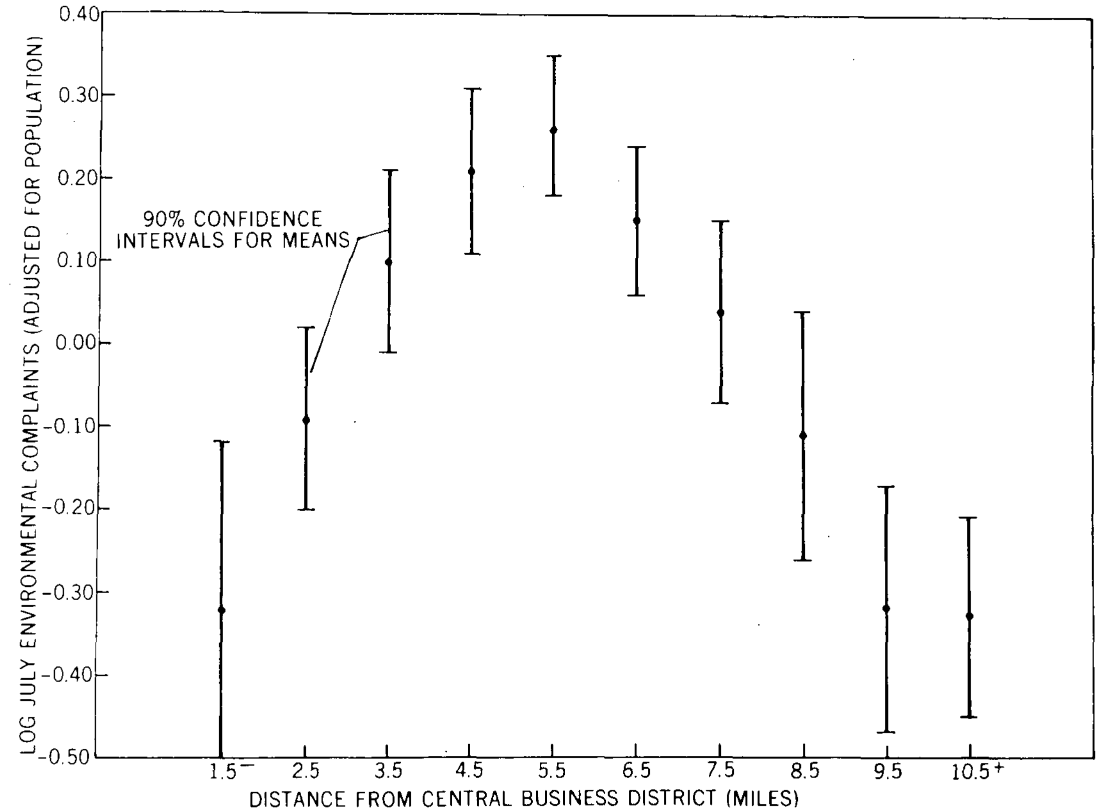

Figure 4. July Environmental Citizen Contacts and Distance from the Central Business District

actual values for the coefficients for the different equations presented in Table 2. This is possible because the independent variables (distance, housing "newness") and the dependent variables (logarithm of citizen contacts adjusted for population) are measured in the same units for each category of contact.

It is possible to test an additional hypothesis concerning the generation of citizen-initiated contacts. It is possible that, because of past patterns of racial discrimination, blacks are less willing to complain about the condition of their neighborhoods than are whites, even after the effects of social well-being are removed. This suggests, but does not logically imply, that as the percentage of black residents in a neighborhood increases, the propensity to initiate contacts with governmental officials should decline. More strongly, the propensity to initiate complaints should be less in all-black neighborhoods than in all-white or mixed neighborhoods. If we add the percentage of black residents to the regression equations of citizen contacts on social well-being, we can assess the impact of variations of the racial composition of neighborhoods on propensity to contact, with the influence of social well-being removed. That is, are neighborhoods with a large proportion of black citizens less likely to generate contacts even when the social well-being of such neighborhoods is controlled?

Table 3 contains the relevant data. The table presents the contribution of the variable "percentage black" in a regression equation in which a measure of social well-being is entered directly, along with the measure squared; that is, percentage black is added as a separate independent variable to the equations presented in Table 2. For the contact data filtered through the Office of the City Clerk, there is no significant effect for percentage black. However, for both sets of Environmental Enforcement Division data, percentage black is significant and contributes substantially to the multiple correlation coefficient. But the sign of the coefficient assessing the impact of variations in the racial composition is *positive,* indicating that neighborhoods with a high proportion of black residents are *more* likely to initiate contacts, when the influence of the social well-being of the neighborhood is held constant. There is, then, no evidence that neighborhoods with high proportions of blacks complain less about environmental problems.

Why the difference between the data sets? During the period under study, the City Clerk's

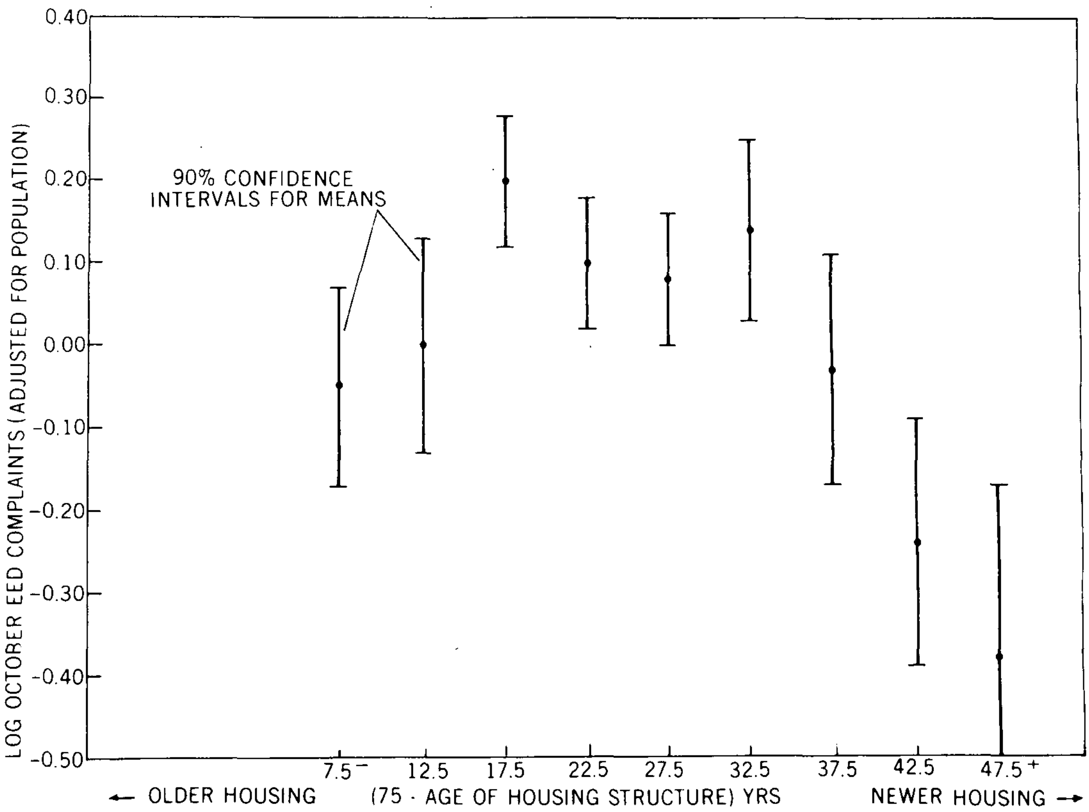

Figure 5. October Environmental Citizen Contacts and Housing Structure

Table 3. Contributions of Percentage Black to the Regressions of Contacts on Social Well-Being

Dependent Variable[a]	Social Well-Being Assessed by	Regression Coefficient for % Black	Standard Error	*t*-value	Additional Contribution to *R*[a]
LN (Total City Clerk Contacts)	DISTANCE[b]	0.0637	0.1092	0.58	.001
LN (Total City Clerk Contacts)	HOUSING[c]	0.0384	0.1099	0.35	.001
LN (July EED Contacts)	HOUSING[c]	0.2217	0.0633	3.50	.031
LN (October EED Contacts)	HOUSING[c]	0.1958	0.0633	3.09	.032

[a] See Table 2.

[b] DISTANCE is the distance from the central business district to the center of the census tract in miles.

[c] HOUSING is the "Newness" of the housing structure in years = 75 years minus average age of housing structures in the census tract.

staff was primarily white, while the Environmental Enforcement Division was mainly a black-staffed bureaucracy. The differences in the coefficients may reflect that black residents were more comfortable contacting the EED. Because our unit of analysis is an aggregate of population, our data do not speak directly and clearly to the point, which involves inference about individual behavior. The "ecological fallacy," as we understand it, simply states that when one correlates, say, an aggregate population characteristic with an aggregated behavior (per cent black with propensity to contact), there is a plausible rival hypothesis to the explanation which is first suggested by the data (that blacks are contacting at higher rates). The plausible hypothesis is that persons with other (opposite?) demographic characteristics increase their propensity to emit the behavior as the proportion of those individuals whose characteristic is assessed by the measured variable increases in the population aggregate. Whites may contact more when they reside in relatively black neighborhoods. The word "fallacy" suggests a logical problem; but of course *no* data will support any kind of logical

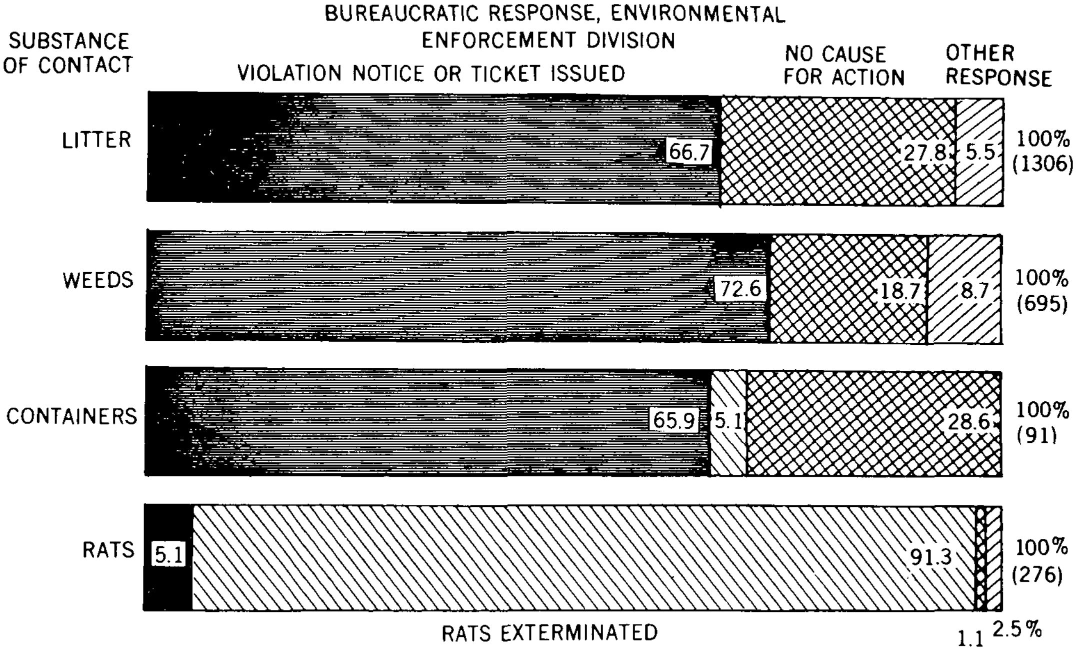

Figure 6. Substance of Contact and Response of the Environmental Enforcement Division (City Clerk Data)

inference, except in the sense of bringing empirical evidence to bear on a proposition derived logically from other assumptions. The problem is not one of deduction, but of inference. We believe that blacks contact EED at a higher rate than do whites in a like social circumstance. The data support, but of course do not "prove" this assertion.

The Response of Government

The act of a citizen contacting his government about matters of public policy or specific services may be viewed from two perspectives. On the one hand, one may attempt to discern the *causes* of the participant act; this has been the focus on the preceding sections of this paper. On the other hand, one may examine the *consequences* of the contact. What does government do because of the contact initiated by the citizen? This is the concern of the remainder of this paper.

What government does on receiving contacts from citizens which attempt to affect the distribution of governmental benefits depends on (1) the nature of the political system responsible for the delivery of the benefits; (2) the content of the citizen contact, and (3) the characteristics of the individual attempting to influence the government. Even within the United States, local jurisdictions may differ substantially in their sensitivity to incoming citizen influence attempts. This difference can arise from a host of factors, including the resources of the system that are devoted to providing the benefits about which the citizen contacts government. In the provision of local services by public bureaucracies, bureaucratic rules developed for the delivery of the service can be important in the determination of the governmental response. Some local bureaucracies have attempted to develop delivery criteria according to the intensity of the need of the receiving public. A second rule is response on demand; such a rule may emerge in a bureaucracy with low information-acquiring capacities even when the need of citizens is the primary justification for the service delivery. Need may be indexed by the existing citizen requests for the service, a notion which seems suspect given our earlier exploration of the mechanisms generating citizen contacts in the area of enforcement of environmental ordinances. The important point is that a citizen may get a satisfactory response to his contact not because of any skill or characteristic that he possesses, but because of the existing rule developed by the bureaucracy for delivering a service. If this is the case, the distribution of the service to different social groups will depend more on the mechanisms generating the influence attempt than on the discretion of the service bureaucracy.[20]

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[20] For a further analysis, see Bryan D. Jones, Saadia Greenberg, Clifford Kaufman, and Joseph Drew, "Service Delivery Rules and the Distribution of Local Government Services: Three Detroit Bureaucracies"

Second, the response of government may depend on the content of the contact. The inclination or ability of government to respond to a citizen's attempt to influence the delivery of policy benefits depends on what the citizen wishes. Response may be different in an area in which general policies have given government responsibility for the delivery of the benefit than in one in which they have not. Response may also be affected by the difficulty of the task requested, and the existence of opposed interests.

Third, the nature of the individual contactor can influence the character of the governmental response. Even where well-established bureaucratic rules are developed and in operation, there is often much leeway in the application of the rules. If, in the case of environmental enforcement, every contact got some response, the quality of the response could vary depending on the demographic characteristics of the contacting individual, or, more likely, the contactor's neighborhood. In general, the persons responsible for the provision of the service will not know anything about the person making the service request, but will know what his neighborhood is like. Services which are delivered to the neighborhood of the contactor can vary in speed of provision and in other aspects of the quality of the response. The nature of the contactor or his neighborhood can be even more critical in policy areas in which rules governing the delivery of services are ill-developed.

The data collected from the Office of the Detroit City Clerk allow us to associate each citizen-initiated contact with a governmental response. Since the records of the Environmental Enforcement Division were assembled on an aggregate basis by that bureaucracy, these data could not be analyzed in a parallel fashion. Figure 6 presents the major categories of response to citizen contacts, for the major categories of the content of the contact. In each category of citizen complaint, the most striking finding is that the Environmental Enforcement Division investigates virtually every case which comes into the bureaucracy via the Office of the City Clerk. The percentage of nonresponse was less than 2 per cent in each category, including cases in which the investigator could not find the address where the complaining citizen had indicated there was a problem. These responses are tabulated with the "no cause for action" responses—those responses which the investigator determined that the problem was not severe enough for governmental intervention.

Violation notices were issued in more than two-thirds of the cases for complaints concerning litter, weeds, and sanitation containers. These notices are orders to correct the problem; investigators are supposed to reinspect the situation within a reasonable amount of time to determine compliance. If the inspector judges at the time of reinvestigation that the problem has not been solved, he may issue a court notice; if it is solved, he issues an abatement of his original order. The Division has problems of compliance partly because of the unwillingness of inspectors to issue court notices, and partly because of the unwillingness of the courts to enforce the court notices, especially when the violator fails to show up for his court date. We cannot investigate the subsequent attempts of the EED to alleviate the problem, since the City Clerk receives the form tabulating EED action after the first investigation.

In about one-quarter of the total number of initial investigations the inspectors judged that there was either no cause for governmental action, or that the problem had been solved before the inspection. In many cases, it would be relatively easy for a citizen to correct or improve the situation in anticipation of a visit from a city inspector. From the perspective of the social scientist, however, the category has to be viewed with a degree of healthy suspicion, since it may be indicative of poor response quality. Presently we shall examine the distribution of this response category across neighborhoods of varying social characteristics. In the area of rodent control, however, either rats were exterminated or a violation was issued in over 96 per cent of the cases.

The data indicate that, at least in responding to complaints coming in through the Office of the City Clerk, the Environmental Enforcement Division virtually always conducts an on-site investigation. Thus whether or not a citizen receives some response to the environmental problem he perceives as existing in his neighborhood seems to be entirely determined by whether or not he initiates a request for action. It is worth noting that this is not the pattern of response which Mladenka found in Houston. He reports that when citizens contacted a central complaint bureau about problems of overgrowth and debris, they received no response 33 per cent and 25 per cent of the time, respectively.[21] As we noted above, character-

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(paper presented at the American Political Science Association Meeting, San Francisco, California, September 2-6, 1975).

[21] Kenneth Mladenka, "The Distribution of Municipal Services: Justice and Bureaucratic Responsiveness" (manuscript, Rice University, Houston, Texas, 1975), p. 32. Record-keeping differences make responses difficult to compare with the results from Detroit.

istics of the system responsible for delivering services may affect the response which citizens receive. The ease of generating an investigation in Detroit is doubtless the result of a bureaucratic rule concerning when investigations are to be made. The rule is simple: always investigate.

The Quality of Governmental Responses: The Distribution of Processing Time. Even though almost all complaints called in by citizens are investigated by EED, the quality of the investigation is likely to vary. It is important to know whether the quality of response varies systematically with discernible characteristics of citizens or their neighborhoods. Such variations have direct implications for the distribution of service benefits to citizens.[22] One indicator of response quality which varies from investigation to investigation is the time expended in processing the complaint. Three hypotheses concerning the systematic variance of processing time may be stated. First, in the face of sustained demand in a particular geographic area, the organization's resources may become so taxed that it cannot respond promptly to citizen-initiated contacts. This overload can be *differential,* in that some segments of the organization are more susceptible to it because of the geographical pattern of citizen demands. EED is particularly susceptible to such overload, since its inspectors are assigned to districts which correspond to those maintained for garbage collection by the Sanitation Division; these districts do not necessarily reflect the work load of the environmental inspectors. If the hypothesis of organizational overload is viable, processing time should be directly related to the number of complaints generated in geographical areas.

The second hypothesis relates to discrimination. Processing time may be related to the socioeconomic status of the neighborhood, or to its racial composition. In particular, better-off neighborhoods may be able to get shorter processing time for its complaints. Finally, areas maintaining strong community organizations may be able to get more immediate response to its complaints. If this is the case, the proportion of contacts which came through a local organization in a neighborhood should be inversely related to processing time.

In order to investigate these hypotheses, we regressed the natural logarithm of processing time in days on the log of the total number of complaints coming from a census tract, the distance of the tract from the central business district (a measure of social well-being), the percentage black in the tract, and the percentage of complaints which came from a community organization or block club. Only census tracts which directed some complaints to the City Clerk were included; processing time was the average time taken by the bureaucracy to handle the complaint and notify the City Clerk of the action taken. Neither the percentage of complaints which were relayed by a community group nor the percentage of black residents in the tract were significant in the equation. Indeed, both of the coefficients were opposite in sign from the expected direction; percentage black was negatively related to processing time, while the percentage of complaints handled by community groups was positively related to processing time.

The remaining two variables were significantly related to processing time. The resulting regression equation is presented in Table 4. It is clear from that equation that the hypothesis of differential organizational overload remains viable. As the total number of complaints from a tract increases, the average time it takes for processing the complaints increases directly. Thus we have an anomaly. More complaints about enforcement of environmental ordinances lead to better service in that more situations are investigated, but lead to poorer service in that it takes longer to get them

Table 4. Regression of Processing Time on Total Complaints, Distance[a]

LN (PROCESSING TIME) =	2.227 +	0.1527 LN (TOT CONTACTS) –	0.0197 DISTANCE
std. error:	(0.062)	(0.0069)	(0.0277)
t-value	(36.16)	(−2.88)	(5.50)
$N = 365$; $R = .298$			

[a] Variables are

PROCESSING TIME = Time in days from referral from the Office of the City Clerk to the return of the form with EED's action noted.
TOT CONTACTS = Total number of contacts received from the census tract; not corrected for population.
DISTANCE = distance from the central business district.

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[22] See Bryan D. Jones and Clifford Kaufman, "The Distribution of Urban Public Services: A Preliminary Model," *Administration and Society,* 6 (November, 1974), 337–360, for a further discussion of service distribution.

investigated. There is also some indication of discrimination in the regression equation presented in Table 4, since there is a significant relation between our measure of social well-being, distance from the center of the tract to the central business district and processing time. The better off the tract, the shorter the processing time; but of course there is a good deal of residual variation. This discrimination need not be deliberate. It may well be a function of the allocation of organizational resources. The inspectors serving the outlying districts have smaller work loads, and may be able to process complaints more quickly. At any rate, the association between distance and processing time is not a strong one.

The Quality of Governmental Response: The Distribution of "No Action" Investigations. We noted above that the categories of response in which the Environmental Enforcement Division took no action, for whatever reason, should be viewed somewhat suspiciously since failure to act could indicate either a decayed quality of governmental response or an accurate assessment that the situation required no action. The hypothesis of organizational overload may hold here as well as with processing time. One strategy for dealing with overload is to decide that marginal situations do not merit governmental intervention. If we examine the distribution of "no action" across neighborhoods, we would expect the largest proportion of such responses to come from just those neighborhoods in which a large number of citizen complaints are initiated, if the overload hypothesis is viable. Figure 6 graphs the proportion of investigations in which no action was taken against the distance of the census tract from the central business district in which the investigation was conducted, for weed and litter complaints.[23]

Before drawing any substantive conclusions from this data, we must rule out a rival hypothesis: that the series was generated by chance. To test this hypothesis, we employ a test which relies on the mean squared successive differences of the observations. A measure may be constructed by taking the square of the differences of successive observations, and dividing by $n-1$. If the ratio of the mean squared successive differences to the variance is taken, the resulting statistic has an expected value of 2, a variance of $(n-2)/n^2$, and is normally distributed.[24] Table 5 presents the results. For litter, the hypothesis of randomness is easily rejected; while for weeds the d-statistic is in the expected direction, but does not reach conventional levels of statistical significance. This is due entirely to the large proportion of no action responses in the category centered at 2.5 miles from the central business district. If we delete this observation, the statistic easily reaches statistical significance. Since deleting any observation from a random series should not make it nonrandom, this suggests that we ought to reject the hypothesis that the series have been randomly generated.

Table 5. Tests for Randomness for Series Plotted in Figure 7

	Value of D^a	Level of Significance
LITTER	1.485	.034
WEEDS	1.895	.348
WEEDS-CORRECTED[b]	1.214	.004

[a] *D is the ratio of the mean squared successive differences to the variance.*

[b] Series for weeds with observation at 2.5 miles from the CBD omitted.

Both series reach high points at distance = 5.5. This is just about the point that complaints reach a maximum (see, for example, Figure 3). The regression equation in Table 4 indicates that this is also about the point at which processing time is longest. It may be that the proportion of situations which require no governmental action are higher at this maximum point for three variables. But another explanation seems more reasonable. The evidence suggests that an organizational strategy to cope with differential overload is being implemented by inspectors responsible for enforcing environmental ordinances in Detroit. The strategy is to provide a lower quality of service *per contact* in the areas of the city where the total number of citizen-initiated contacts is high.

Conclusions

An instrumental view of political participation has led us to consider both the causes and the consequences of a very individualistic form of participation, citizen-initiated contacts with local government about services which confer particularistic benefits. In studying such contacts with one local government about one kind of service, at one point in time, we found the following:

(1) The number of citizen contacts generated, corrected for population, is curvilinearly related to social well-being of neighborhoods, such that propensity to contact is low in neighborhoods of low social well-being, increases with well-being until it

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[23] There were so few no action responses for rodent control that a plot could not be constructed for that category, while there were too few container complaints to give stability to the series.

[24] Wonnacott and Wonnacott, pp. 411–413.

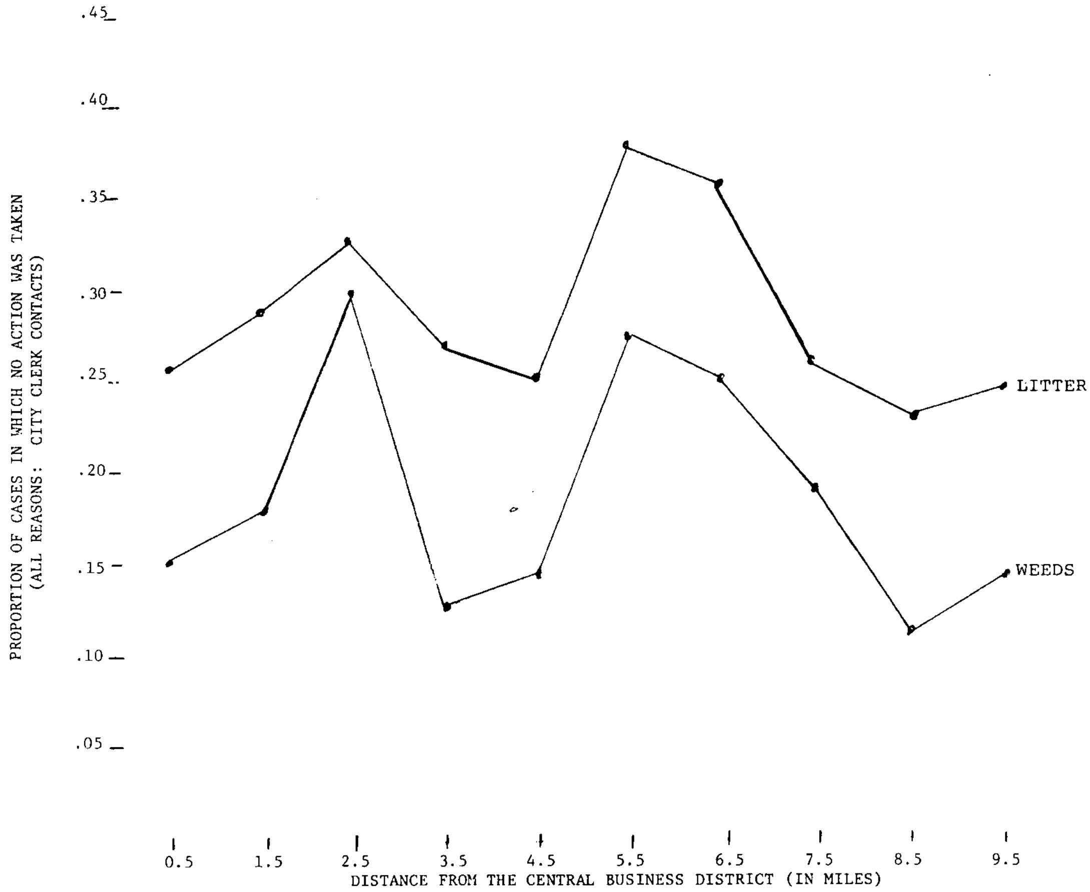

Figure 7. Proportion of "No Action" Responses Versus Distance

reaches a maximum in middle levels of well-being, then declines with increasingly high levels of social well-being.

(2) No evidence indicated that citizens from black neighborhoods were any less reticent in promoting their claims on government for environmental services than were citizens from white neighborhoods, when the effect of social well-being was removed.

(3) The records indicate that virtually every citizen contact generated a governmental response in the form of an on-site inspection.

(4) Processing time is related to the number of contacts which are generated from an area; response time increases as demand increases.

(5) The time expended in processing citizen contacts is also related inversely to social well-being; better-off neighborhoods thus get quicker response to their contacts, after the influence of the number of demands is controlled. The proportion of black citizens residing in a neighborhood did not have a significant effect on processing time.

(6) The proportion of investigations in which the city inspector decides that no governmental response is called for is highest in neighborhoods in the middle ranges of social well-being. These are the same neighborhoods which generate a disproportionate number of complaints and get the poorest processing time to complaints.

Although the findings are limited in time and place, they support two lines of theorizing. First, the findings concerning the generation of citizen contacts support the theory developed in this paper that such contacts, when they concern governmental programs or services which confer particularistic benefits, are a multiplicative function of need for the service and awareness that government is relevant for the amelioration of the need. Need and awareness are assumed to be linearly related to social well-being, the former inversely, the latter directly. We believe the theory to be general enough to characterize many categories of particularistic service, but may not be discernible in certain service areas because of the general awareness that government is responsible for the provision of the service—calls for fire-fighting services, for example.

Second, these findings indicate that the service bureaucracy we studied, the Environmental Enforcement Division of Detroit, adopted a strategy of reducing the quality of service contacts differentially according to the strain on organizational resources. That is, areas which had the highest incidence of citizen requests for services seemed to have received the poorest quality of service for each contact made. This does not mean that these areas received the poorest service *in toto,* since they received more investigations *because of* their higher propensity to contact city government about environmental problems. Paradoxically, an individual living in an area characterized by the best service, as indexed by *the total number of governmental responses* to citizen requests for services, is less likely to get a satisfactory response to his *individual* contact.

On the Decline of Competition in Congressional Elections*

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In a recent article, Mayhew discovered that since the middle of the 1950s there has been a steady decline in the proportion of "competitive" congressional districts.[1] In related work, Erikson found that the incumbency advantage more than doubled between the late 1950s and 1966.[2] For the same period Tufte showed that a substantial drop in the "swing ratio" (the percentage increase in House seats a party obtains when it receives a one per cent increase in popular vote) had taken place.[3] Finally, Kostroski also discovered a substantial increase in the incumbency advantage in postwar Senate elections.[4]

Not surprisingly, scholars differ in their explanations of these findings. Without doing violence to anyone's position, one can enunciate three proposed explanations. Some authors argue that changes in the institutional setting of congressional elections have worked to alter the outcomes of these elections. For example, Tufte attributes the decline in the swing ratio to the control incumbents have over redistricting:

> Our data indicate that a major element in the job security of incumbents is their ability to exert significant control over the drawings of district boundaries. . . . Ironically, reapportionment rulings have given incumbents new opportunities to construct secure districts for themselves, leading to a reduction in turnover that is in turn reflected in the sharply reduced swing ratio of the last few elections.[5]

Tufte argues further that in Senate districts (states, to institutionalists) there has been no reapportionment and no decline in the proportion of marginal seats. Finally, he notes that if House elections are examined in states that have reapportioned "there is an immediate decline in the competitiveness of the races in the first election after the new districting."[6]

A second position attributes the changing nature of congressional elections to a shift in the behavior of the electorate. Perhaps the most explicit statement of this position is advanced by Burnham:

> Tufte's argument about the effects of bipartisan gerrymandering of districts is ingenious but not ultimately convincing. For there is a host of evidence . . . to support the view that *the most important single factor has been systematic change in mass voting behavior since 1960.*[7]

Burnham argues that "the very high . . . swing ratios of the late nineteenth century were associated with a period in which party identification and party voting were extremely salient, by all aggregate indicators."[8] In a somewhat earlier contribution, Erikson anticipated Burnham's point:

> An increased incumbency advantage in 1966 is not so mysterious as it may seem, since the timing of its occurrence coincides with that of the reported erosion of party identification as

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*This paper has benefited greatly from the assistance of John Land, my research assistant, and from the detailed critical comments of Morris Fiorina, Sam Kernell, Robert Erikson, John Kingdon, Ben Page, Gary Jacobson, Michael Cohen, J. Vincent Buck, Robert Bates, and Lance Davis. I could not take all their criticisms into account, but I am deeply grateful for their generous donations of time. Some of the data employed in this study were made available by the Inter-University Consortium for Political Research at the University of Michigan. I alone am responsible for the analysis and conclusions.

[1] David Mayhew, "Congressional Elections: The Case of the Vanishing Marginals," *Polity,* 6 (Spring 1974), 295–317. Throughout this paper I define a competitive seat as one in which the margin of victory does not exceed 20 per cent. This definition is not only arbitrary but it also has the defect of suggesting that what might be called the vulnerability of a seat is related in some simple way to vote margin. While it is possible that the connection between vulnerability and vote margin is not only complicated but is also unstable in time, I cannot investigate this question in the present paper. The reader is therefore asked to keep in mind the provisional nature of this definition in interpreting the results reported here.

[2] Robert S. Erikson, "The Advantage of Incumbency in Congressional Elections," *Polity,* 3 (Spring 1971), 395–405; and "Malapportionment, Gerrymandering, and Party Fortunes in Congressional Elections," *American Political Science Review,* 66 (December 1972), 1234–1335.

[3] Edward R. Tufte, "The Relationship Between Seats and Votes in Two-Party Systems," *American Political Science Review,* 67 (June 1973), 540–554.

[4] Warren Lee Kostroski, "Party and Incumbency in Postwar Senate Elections," *American Political Science Review,* 67 (December 1973), 1213–1234.

[5] Tufte, "Relationship Between Seats and Votes," p. 551.

[6] *Ibid.,* p. 553.

[7] Walter Dean Burnham, "Communications," *American Political Science Review,* 68 (March 1974), 210.

[8] *Ibid.*

> an electoral force in the late sixties. Possibly the electorate's decreasing partisan loyalty, signaled by such indicators as the post-1964 surge in the number of independent voters, is the cause of the apparent boost in the incumbency advantage.[9]

A third, intermediate, possibility is that institutional change has modified voter behavior. For example, Mayhew argues that people in the same situation (in terms of information about the candidates) behaved in the same way in 1966 as they did in 1958 but that incumbents had more of an advantage in promulgating information than they did in the earlier period. According to this view there is aggregate behavioral change, but it is caused by a shift in the marginal distributions of people across the various informational categories. Mayhew hypothesizes that these shifts stem from the increasing use of the institutional advantages of incumbency such as the franking privilege, or from increasing skill in using polls and publicity. Mayhew writes, "The answer to the incumbency advantage question could be a remarkably simple one: the more hundreds of thousands of messages congressmen rain down on constituents the more votes they get."[10]

In this paper some data are presented which will help to clarify some of the issues in this debate. I argue that both Tufte's pure institutional change theory and Mayhew's argument that the informational advantage of incumbents has increased, are inadequate to account for the observed phenomena. Thus any acceptable explanation of why the incumbency advantage has increased must be based on a basic shift in the behavior of the electorate. Of course, a shift in electoral behavior may be of two basic sorts. What might be called the *distribution* theory holds that different kinds of party identifiers (strong Democrats, weak Democrats, etc.) are acting the same as always but the distribution of people into these categories has shifted. The *behavioral change* theory holds that within each party-identification category there has been a change in behavior. The data I present will provide some evidence that at least part of the change occurring is of the latter sort.

The plan of the paper is as follows. First, by presenting data on redistricting, I show why Tufte's explanation fails. Second, I analyze survey data which indicate an increase in incumbency voting at the level of the individual voter. Third, I show that increased incumbency voting results only partly from the increased informational advantage of incumbents over nonincumbents *and* the propensity of voters to cast their ballots in favor of candidates who are known to them. Both of these factors have undergone some change between 1958 and 1970, but the change in the informational advantage is not adequate to account for the change in incumbency voting. Finally, data are presented which suggest that the inclination of voters to vote for candidates they know has increased over the period under study at all levels of party identification.

Redistributing and Competition

In his reply to Burnham's comment on his 1973 article, Tufte remarks that more important than ascertaining whether or not there has been an underlying shift in voter behavior that would account for the shift in the swing ratio is "allocating the effects on political competition of redistricting on the one hand and the increase in incumbent resources on the other."[11] This prescription is sensible as long as there is some reason to believe that these two effects capture a substantial fraction of the variance in the dependent variable. In this section I argue that there is no reason to expect that redistricting has much influence on the variables of interest.

In two papers and a reply to a comment, Tufte has advanced several pieces of evidence indicating that redistricting has a major effect on the decline of the swing ratio. In his first paper (1973), Tufte notes that the proportion of competitive seats in the House has declined from about .20 in 1958 to .13 in 1970, while in the Senate (where no redistricting ever takes place), there has been no decline. He then says that "some recent redistricting laws have been described as the Incumbent Survival Acts of 1972."[12] He claims that "reapportionment rulings have given incumbents new opportunities to construct secure districts for themselves. . . ."[13] Tufte goes on to present data on the number of marginal seats in Michigan, Illinois, Pennsylvania, and Ohio for the 1970 elections (all these states had been redistricted during the decade). Finally he claims that "the independent contribution of reapportionment to the job security of incumbents can also be seen in the elections immediately following reapportionment in a state: there is an immediate decline in the competitiveness of the races in the first election after the new districting."[14]

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[9] Erikson, "Malapportionment," p. 1240.

[10] Mayhew, "Congressional Elections," p. 311.

[11] Edward R. Tufte, "Communications," *American Political Science Review*, 68 (March 1974), 212.

[12] Tufte, "Relationship Between Seats and Votes," p. 551.

[13] *Ibid.*

[14] *Ibid.*, p. 553.

In his rejoinder to Burnham's communication, Tufte presents what he calls the *"seats-votes" curves* for California in 1966 (before redistricting) and 1968 (after redistricting). These curves indicate a substantial decline in the number of competitive districts in the state following the redistricting.

Finally, in his article,[15] Tufte presents the seats/votes curves for Illinois, Michigan, Pennsylvania, and Ohio for 1950 and 1970. In each case there is a substantial decline in the swing ratio (and of course in the number of competitive districts). As far as I know, this is all the evidence that Tufte has presented in support of the redistricting explanation.

As the reader may suspect, I have several objections to this explanation. First, it is highly implausible *a priori.* Before the Court rulings on reapportionment, there were fewer legal restrictions on the amount of gerrymandering that could be done than there are now. Aside from some anecdotal remarks, Tufte has presented no evidence that incumbents have more control over redistricting now than they ever did. It appears to me that he must bear the burden of proof on this point and establish the plausibility of his contention.

Second, while Tufte presents some data on the number of competitive districts in certain states before and after redistricting, he fails to look at changes in the number of competitive districts in states where no reapportionment has occurred. If *any* of the opposing explanations are correct he would find that there has been a decline in the number of competitive seats after reapportionment but that decline need have nothing to do with the reapportionment itself. In those states which underwent it, reapportionment is simply correlated perfectly with the change in voting behavior (if Burnham and Erikson are right) or with the increase in resources held by the incumbent (if Mayhew is correct). This problem seems to be easily remedied by comparing the number of marginal districts over time in states which redistricted with those which did not. In Tables 1 and 2 any district in which the winner received no more than 60 per cent of the vote is called competitive, while all others are called noncompetitive.

Table 1. Decline in Percentage of Competitive Seats in Non-Southern States That Have and Have Not Been Redistricted, 1962–1966[a]

	Redistricted	Not Redistricted
1962	51	51
1966	40	28
Number of districts	182	132

[a]The data are from *America Votes,* Vol. 9, ed. Richard Scammon, *Congressional Quarterly,* 1972. Entries are the percentage of competitive districts.

Table 2. Decline in Percentage of Competitive Seats in Non-Southern States That Have and Have Not Been Redistricted, 1966–1970[a]

	Redistricted	Not Redistricted
1966	35	39
1970	27	33
Number of districts	177	153

[a]The data are from Scammon. Entries are the percentage of competitive districts.

These tables indicate that the drop in the percentage of competitive seats that Tufte found following reapportionments is not due to redistricting, since the decline occurred in unredistricted areas as well. These data suggest that redistricting has no influence at all on the swing ratio. The decline in the number of marginal districts is a general one which must be accounted for by a theory of the sort advanced by either Mayhew or Burnham.

Before proceeding with a somewhat more detailed consideration of the explanations of Mayhew, Burnham and Erikson, I shall present one more piece of evidence which seems to bear on the problem. In an article on postwar Senate elections, Kostroski found that when the percentage of a senatorial candidate's popular vote is regressed on measures of "base party vote," "national tides," and "incumbency" within party, there has been a substantial increase in the effect of incumbency on vote percentage.[16] For the present purposes it is significant that this increase has occurred in "districts" in which no redistricting took place. In my view, Kostroski's results fit quite well with the observed drop in the swing ratio in House districts, since this drop might well be due to an increase in incumbency voting in House elections. Kostroski's research indicates that incumbency voting has in fact increased during the postwar period and that this increase occurred in areas which have not been redistricted.

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[15] Edward R. Tufte, "Determinants of the Outcome of Midterm Congressional Elections," *American Political Science Review,* 69 (September 1975), 812–826.

[16] Kostroski, "Party and Incumbency in Postwar Senate Elections."

On the Incumbent's Increasing Control of Resources

Mayhew suggests that a principal source of the change in the number of competitive seats may be found in the "greater electoral advantage" that incumbents hold over their opponents. He cites two pieces of evidence that this advantage has increased. First he remarked that Erikson found that the incumbency advantage more than doubled between the 1950s and 1966.[17] Second, Mayhew computed the drops in the percentage of the vote that a party suffers in a district when an incumbent retires. He found that these drops were larger in 1966, 1968, and 1970 than in 1962 and 1964. He concluded that "Incumbency does seem to have increased in electoral value, and it is reasonable to suppose that one effect of this increase has been to boost House members of both parties out of the marginal electoral range."[18]

Mayhew attempted to trace the decline in the number of marginal districts and the concomitant apparent increase in the advantage of incumbency to real changes in the quantity of resources held and employed by incumbents. He argued that incumbent congressmen currently make substantially greater use of the franking privilege than did incumbents in the 1950s. Indeed the quantity of junk mail quadrupled between 1954 and 1970. Further, this increase in the control and utilization of tangible resources has allegedly translated into an increase in the level of recognition enjoyed by incumbents. Mayhew cites Gallup poll data which indicate that there was a seven per cent increase in the percentage of people who knew their congressman between 1966 and 1970.

While I do not have data that bear directly on whether incumbent congressmen enjoy more of an advantage over their opponents in the control of campaign resources than did the incumbents of the 1950s, it is possible to utilize data collected by the SRC to question whether any effects on voting behavior may be imputed to this alleged change. If Mayhew's argument is correct, one should be able to observe, first of all, an overall increase in the level of recognition of the incumbent. Second, the relative level of recognition of incumbents versus challengers should also show an increase. Additionally one ought to find that the increased level (or relative level) of recognition translates behaviorally into an increased level of incumbency voting.

The data I present below indicate the following: (1) a substantial increase in incumbency voting on the level of the individual voter; (2)

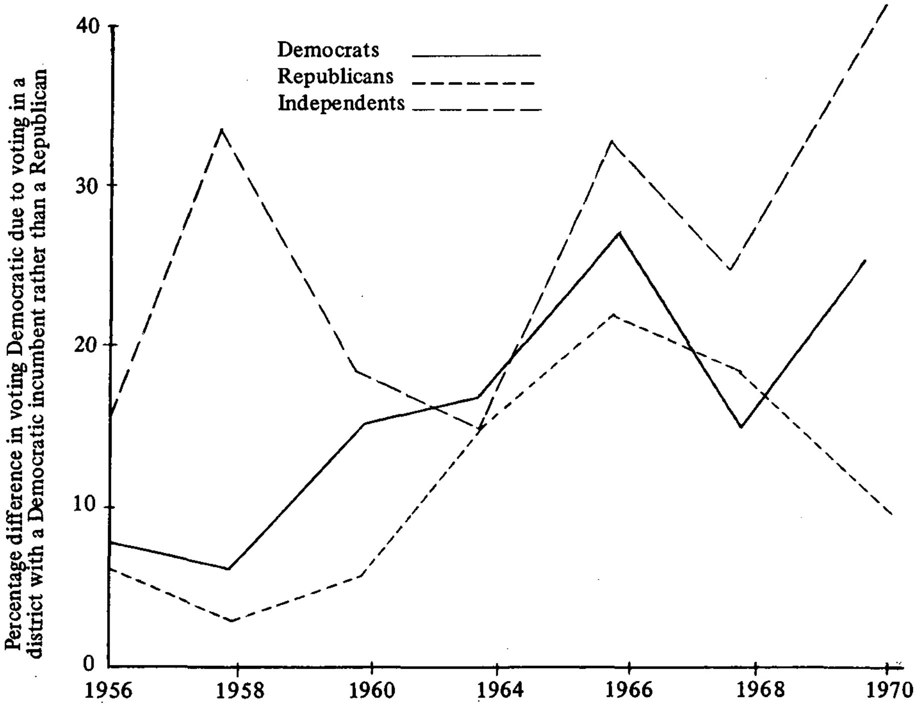

Figure 1. Change in Incumbency Voting, 1956–1970, for Republicans (SR & WR), Democrats (WD & SD) and Independents (ID & I & IR) in All Contested Districts

no increase in the level of recognition of incumbents; and (3) little if any increase in the gap between recognition levels of incumbents and challengers. I reserve treatment of the behavioral linkage between candidate recognition and voting until the next section of the paper.

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[17] Erikson, "Malapportionment."

[18] Mayhew, "Congressional Elections," p. 310.

The data utilized here are from the SRC election surveys for 1956, 1958, 1960, 1964, 1966, 1968 and 1970. These are all of the years in which information on incumbency was collected by SRC or in which congressional districts identification was provided so that incumbency status could be supplied by the author. Unfortunately, only three off-year elections are available for these purposes, and so some of the results are advanced here only tentatively.

Has there been a change in the frequency of incumbency voting during this period? To answer this question for each year and for Democrats, Republicans and Independents, the percentage of voters in each partisan category in districts with Democratic incumbents who voted for the Republican candidate was subtracted from the percentage of voters (in the same category) in districts with Democratic incumbents who cast their ballots for the Democratic candidate. Figure 1 reports these data during the period.

First notice that for Independents the tendency to vote for the incumbent is substantially greater in off years than in years of presidential elections. Further, while there is some discernible long-term shift in the behavior of the Independents, it is particularly interesting that partisan identifiers (especially the Democrats) became more likely to respond to incumbency later in the period of observation than they had been earlier. One may conjecture that their behavior has become more like that of the Independents over time. Of course, until more data are available, this possibility is only speculative.

We now examine an important intervening step in Mayhew's argument. Has the informational advantage held by incumbents increased during the period? To answer this question, each respondent was asked to name the candidates for the House in his district. If the respondent could provide the name of a candidate, then he was considered to be "aware" of the candidate, otherwise not. Among the surveys for which we had incumbency information, this question was asked only in 1958, 1964, 1966, 1968 and 1970 so that the data are a bit more limited than those reported earlier.

If Mayhew's theory is correct, these data should show that incumbents are more likely to be known to voters after 1964 than in 1958. Further, the advantage which incumbents enjoy in this respect ought to have increased over the three elections. Table 3 gives the percentage who know a candidate given that this candidate is or is not an incumbent in all three years. This table indicates that among voters in contested districts with incumbents running *there has been no increase in awareness of the incumbent.* Rather, in years of presidential elections among voters in contested elections who live in districts with an incumbent running, the percentage who know the incumbent's name is constant at 63 per cent. In off years the figure remains constant at about 55 per cent. On the other hand, the corresponding variable for nonincumbents displays no clear trend. During the off years, recognition of nonincumbents has declined, while during presidential election years, it seems to have increased somewhat. These data suggest that the increasing control of resources by incumbents, if it has any effect at all in incumbency voting, does not directly impinge on voter awareness of congressional candidates. In my view this result casts serious doubts on Mayhew's explanation of the declining number of competitive seats.

Table 3. Percentage of Voters Who Are Aware of House Candidates in Contested Districts

	Incumbent	Nonincumbent
1958	57.6 (738)[a]	38.0 (947)
1964	63.0 (856)	39.8 (920)
1966	55.9 (583)	37.6 (703)
1968	63.7 (703)	46.5 (861)
1970	54.7 (548)	31.3 (630)

[a] The number in parentheses is the number of voters in districts with an incumbent running (column 1) or a nonincumbent running (column 2).

Incumbency and Salience of Congressional Candidates

A critical component of Mayhew's argument is that an increase in the salience of a candidate will have the effect of increasing his vote. No doubt the source of this assumption is to be found in Stokes and Miller's classic article demonstrating that candidate salience has an effect on congressional vote. Mayhew drew the following policy conclusion from this study: if a candidate is able through the expenditure of campaign resources to increase his level of recognition, his vote will increase. This proposition, although never directly examined, seems to play a large part in popular reasoning about congressional elections. The following analysis is designed to illustrate whether or not this policy conclusion may be safely drawn from the Stokes-Miller data.

Under the assumption that the effects of salience would not interact with the effects of party identification or of incumbency status, the following regression equation was estimated utilizing an iterative generalized least-squares procedure described in Goldberger.[19]

$$Y = \alpha + \beta_1 X_1 + \beta_2 X_2 + \ldots + \beta_6 X_6 + \epsilon, \qquad (1)$$

where Y = 1 if respondent voted Democratic,
0 otherwise;

X_1 = 1 if respondent resided in a district with Republican incumbent,
0 otherwise;

X_2 = 1 if respondent was aware of the Democratic candidate,
0 otherwise;

X_3 = 1 if respondent was aware of the Republican candidate,
0 otherwise;

X_4 = 1 if respondent was aware of both candidates,
0 otherwise;

X_5 = 1 if respondent was a Democrat (SD or WD),
0 otherwise;

X_6 = 1 if respondent was a Republican (SR or WR),
0 otherwise.

The samples of observations on which the equation was estimated consisted of all contested districts in which an incumbent was running during 1958, 1964, 1966, 1968 and 1970 taken separately.

The question at issue was whether or not, when incumbency status and party ID were fixed, changes in candidate salience had an intuitively predictable effect on the vote. In particular, if a citizen learned of the Democratic candidate, having previously known neither candidate, or, alternatively, known only the Republican, would that citizen's probability of voting Democratic increase significantly? Table 4 gives the regression results.

The estimates reported in Table 4 indicate that, except for 1966 when a voter who knew the Republican candidate was more likely to vote Democratic than one who knew both candidates, the effect of salience was in the predicted direction. Further, the data in Table 4 indicate that in 1958, incumbency had no independent effect on voting (at the .05 level) once the effect of awareness is taken into account. On the other hand, these data suggest that in 1964, 1966, 1968, and 1970 incumbency had a significant effect on the voting decision, once salience is controlled. Voters were apparently using incumbency as a voting cue whether or not they could recall the names of the incumbent candidate in the interview situation.

The model estimated here is obviously extremely simpleminded and, in light of Tufte's results on the causes of voting decisions in congressional elections, using more aggregated data, unsatisfactory as an explanatory model of congressional voting behavior. It was employed here to learn if the widely held belief that the incumbency effect in voting works through candidate salience had any validity. Based on these data it appears that the popular view cannot be rejected for the 1958 data but that in 1964, 1966, 1968 and 1970 there was evidently an independent incumbency effect. In the later period perhaps many voters who were not able to identify the candidate for the interviewer were able nevertheless to distinguish incumbent from nonincumbent in the voting booth and use that information in making their voting decision.

Analysis of the residuals from the regression equations indicated that a number of cases

Table 4. Regression Estimates for Equation (1)

	1958	1964	1966	1968	1970
β_1 (R Increase)	–.023 (.026)[a]	–.082 (.034)	–.228 (.040)	–.067 (.028)	–.099 (.03)
β_2 (Aware Democrat)	+.073 (.028)	.148 (.037)	.123 (.032)	.177 (.048)	.301 (.04)
β_3 (Aware Republican)	–.089 (.030)	–.092 (.040)	.041 (.039)	–.110 (.033)	–.033 (.03)
β_4 (Aware Both)	+.032 (.036)	.012 (.047)	–.176 (.046)	.011 (.053)	–.147 (.05)
β_5 (Democrat)	.465 (.050)	.213 (.044)	.373 (.049)	.375 (.039)	.283 (.05)
β_6 (Republican)	–.321 (.051)	–.345 (.047)	–.249 (.052)	–.170 (.036)	–.250 (.05)
α	.446 (.051)	.558 (.045)	.509 (.050)	.331 (.041)	.366 (.05)
R^2	.583	.305	.455	.304	.380
N	720	920	555	723	592

[a] Standard errors are in parentheses.

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[19] Arthur S. Goldberger, *Econometric Theory* (New York: John Wiley, 1964).

produced estimates for the probability of voting Democratic outside the range between zero and one. This finding indicates interactions between the independent variables in their effects on the dependent variable; that is, the effect of salience on the conditional probability of voting Democratic apparently varies according to incumbency status or party identification.

In order to examine this phenomenon the following table was examined utilizing essentially the same information that was contained in the regression equations but allowing for the interactions between salience and incumbency.

The striking thing about Table 5 is that controlling for incumbency status, in four of ten comparisons increased awareness of own party candidate actually *decreased* the probability of voting for him. In two other comparisons there was essentially no difference at all. These data must cause scholars to reconsider very carefully the maxim advanced by Stokes and Miller "to be perceived at all is to be perceived favorably."[20] A candidate of the same party as a given voter may be more likely to receive his vote if the voter does not recognize him than if he does. On the other hand, if a similar set of tables was displayed with a variable indicating whether or not the voter is aware of the other party's candidate, the effects of salience appear to be much more intuitive. One may only conclude that the effects of name recognition seem to be quite complex and that more investigation is required before one can conclude that increased name recognition will increase a candidate's vote.

Behavioral Change Theories

The arguments in the first three sections of this paper provide strong *prima facie* evidence to believe that neither Tufte's nor Mayhew's theories can adequately explain the decline in the number of marginal districts. In this section I wish to turn from the gleeful enterprise of attacking existing theories to the more difficult and thankless one of constructing part of a new one. Unfortunately, while I cannot claim the credit for inventing the new theory—that must be divided between Burnham and Erikson—I would hold myself partly responsible if it too should turn out to be invalid.

The data in Figure 1 suggest that the principal change in incumbency voting between 1956 and 1970 occurred primarily among the partisan identifiers rather than among Independents. Thus, this section focuses mainly on examining the behavior of the partisans rather than that of the Independents. The major question is this: Is the changing level of incumbency voting due to the changing distribution of partisan identifiers or to changes in behavior within the various party identification categories? Of course one cannot expect a simple answer to such a question, and it seems likely that both kinds of change will be found. Nevertheless, I would think it significant and interesting if the hypothesis of behavioral change within party identification categories could not be rejected.

Table 5. Percentage Voting for Own Party by Awareness and Incumbency

	1958		1964		1966	
	Own Party Candidate Incumbent	Other Party Candidate Incumbent	Own Party Candidate Incumbent	Other Party Candidate Incumbent	Own Party Candidate Incumbent	Other Party Candidate Incumbent
Aware of Own Party Candidate	91.3 (206)[a]	85.2 (115)	92.0 (264)	71.3 (136)	95.4 (152)	72.6 (73)
Not Aware of Own Party Candidate	95.7 (140)	91.2 (137)	90.3 (145)	79.3 (140)	91.4 (116)	68.4 (95)

	1968		1970	
	Own Party Candidate Incumbent	Other Party Candidate Incumbent	Own Party Candidate Incumbent	Other Party Candidate Incumbent
Aware of Own Party Candidate	85.6 (174)	70.8 (113)	92.5 (133)	79.2 (72)
Not Aware of Own Party Candidate	89.2 (102)	70.1 (117)	92.2 (103)	68.4 (114)

[a] Entries in parentheses are the number of cases on which the percentages are based.

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[20] Donald E. Stokes and Warren E. Miller, "Party Government and the Saliency of Congress," in *Elections and the Political Order*, ed. Angus Campbell, Philip E. Converse, Warren E. Miller and Donald E. Stokes (New York: John Wiley, 1966).

In their paper on congressional elections, Stokes and Miller showed that "the saliency of the candidate is of critical importance if he is to attract support from the opposite party."[21] They produced the following table based on survey data from the 1958 elections. These data suggest that while party is a fairly good indicator of how a party identifier will cast his vote, the various categories of knowledge of the candidates have some effect on this relationship.

In Table 7, data are presented from the 1958, 1965, 1966, 1968 and 1970 SRC surveys which correspond roughly to the 1958 data presented by Stokes and Miller. The numbers on which the 1958 percentages are based do not quite agree with those presented by the earlier authors, but the percentages are fairly close to theirs.

The first thing to notice in Table 7 is that in every information category a smaller fraction of people voted for the candidate of their own party in 1964, 1966, 1968 and 1970 than in 1958. This difference is most pronounced in the category of people who could mention only the candidate of the other party. Chi-square tests for homogeneity between 1958 and each of the ensuing years were computed under the null hypothesis that the observations were drawn from the same populations. In each case this hypothesis was rejected at the .05 level.

To construct Table 7, all party identifiers were aggregated (weak, strong, and Independent-leaners). Perhaps a shift in the distribution of the electorate across the various categories in the seven-point SRC party identification scale accounts for this apparent change in behavior. If so, then one may hope to explain the apparent change in voting behavior by explaining why this distribution has shifted. Indeed, if the percentage of strong identifiers who resided in districts in which an incumbent was running in 1956 and 1958 is compared with the same percentages in 1966 and 1968, there was a decline from approximately 43 per cent to about 36 per cent. There was an increase in weak and Independent-leaning identifiers over the same period of about 6 per cent. Since party is less of an anchor for weak and Independent-leaning identifiers than for strong identifiers, the observed change in Table 7 may be due to the changing proportion of the electorate in various party identification categories.

In order to test whether this distributional shift accounts for these changes, a regression

Table 6. Effect of Information on Congressional Voting in Contested Districts in 1958

Percentage Who Voted for Candidate	Voter Was Aware of: Both Candidates	Own Party Candidate	Other Party Candidate	Neither Candidate
Of Own Party	83	98	60	92
Of Other Party	17	2	40	9
N =	196	166	68	368

Table 7. Effect of Information on the Congressional Vote in Contested Districts[a]

Percentage Who Voted for Own Party in	Voter Was Aware of: Both Candidates	Own Party Candidate	Other Party Candidate	Neither Candidate
1958	81.0 (221)	99.3 (134)	66.7 (30)	95.1 (290)
1964	78.8 (245)	94.8 (164)	59.6 (34)	85.6 (250)
1966	80.7 (163)	96.0 (96)	34.9 (15)	86.5 (193)
1968	77.0 (235)	94.9 (94)	48.3 (28)	81.7 (192)
1970	75.9 (107)	99.1 (110)	36.4 (16)	89.8 (185)

[a]Number of cases in each awareness category are in parentheses.

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[21] Stokes and Miller, "Party Government and the Saliency of Congress," p. 204.

model was constructed in which the dependent variable was 1 if the respondent voted for the Democratic candidate and 0 if he or she voted for the Republican. The independent variables were constructed to yield a two-way layout with six party identification categories (excluding Independents) and the four informational categories with all interaction terms included. If the changes in Table 7 are due solely to change in the marginal distribution of party identifiers, then the estimated parameters should not change between 1958 and 1964, 1966, 1968 and 1970. If, on the other hand, some of the change in that table is due to a changing propensity of citizens in a given category of information and party affiliation to vote Democratic, there should be a change in the parameters between 1958 and each of the four following elections—1964, 1966, 1968, and 1970.

The statistical model and estimation procedure are given in the Appendix as are the coefficient estimates for each of the equations. Of particular interest was the null hypothesis, i.e., that no parametric change had occurred between 1958 and each of the four later elections. This hypothesis was rejected at the .01 level in every case. Thus, the present evidence indicates that not all of the changes from 1958 can be accounted for by the changing distribution of party identifiers. At least some of the change in voting behavior has occurred *within* party identification levels.

This finding suggests that while political observers have been lamenting or celebrating, depending on their inclinations, the decline in the number of partisan identifiers, a related sort of change has been occurring. Those people who still identify with one of the parties seem to be using it less and less as a cue in making their voting decisions in congressional elections.

Discussion

The main purpose of this paper is to elucidate and examine critically the principal explanations proffered by scholars for the widely observed decline in the number of marginal seats. By and large the view advanced by Burnham and Erikson, that a behavioral change accounts for the decline, has received the greatest support. Voters are different than they used to be, and not merely because there are more Independents. The party identifiers seem recently to be more responsive to nonpartisan criteria for decision making in House elections than they have been in the past, and in that sense they are behaving more like the Independents.

As Tufte pointed out, the decline in the number of marginal seats may have the effect of mediating the responsiveness of House elections to national tides. The claim here is that the cause of this phenomenon is to be found in a shift in the behavior of the electorate. Perhaps, as some analysts suggest, the change in electoral behavior is rooted in an increased unwillingness of voters to utilize party identification as a voting cue. This possibility is certainly consistent with many other findings. For example, Tufte, and Arseneau and Wolfinger report that party identification accounts for a decreasing proportion of the congressional vote over time.[22] At the level of congressional voting the decreasing reliance on party as a "shorthand" cue may not turn voters toward issue voting but may simply increase their reliance on other rules of thumb such as incumbency or satisfaction with presidential performance. This would be a curious consequence, since it would suggest that increased issue voting in presidential elections and the declining number of competitive House districts have essentially the same causes. As the voters come to approximate more closely the "ideal citizens" of certain democratic theories, they may (inadvertently) end up insulating their congressmen from defeat and hence to some extent reduce their representatives' incentive to respond to constituent desires.

Indeed, recent research reported by Kernell indicates that the perceived performance of the President in office has a pronounced effect on individual citizens in deciding whether and how to vote in off-year elections.[23] Tufte found that at the aggregate level, presidential performance was an important variable in accounting for the midterm votes. Such findings suggest that the scarcity and resulting costliness of information in congressional elections forces most citizens to rely on simple decision rules in deciding how to cast their votes. The decision rules that currently seem to be operating in the electorate are based on party affiliation, presidential performance, and incumbency. The findings in this paper suggest that voters seem to be shifting away from the use of party affiliation as a decision rule and toward increased utilization of incumbency. I have had nothing at all to say about the fact that voters apparently also respond to presidential performance in deciding how to cast their vote. If the importance of this explanatory component is increasing, then at least the partisan makeup of Congress may end up being quite responsive to national forces.

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[22] Tufte, "Communications"; and Robert B. Arseneau and Raymond E. Wolfinger, "Voting Behavior in Congressional Elections," paper presented at the meeting of the American Political Science Association, New Orleans, September 1973.

[23] Samuel Kernell, "Presidential Popularity and Negative Voting," paper presented at the meeting of the American Political Science Association, Chicago, September 1974.

Given the limited quantity of data presented here and the difficulty of ascertaining voter responsiveness to national forces in the SRC data, only guesses and speculations can be advanced about the significance of the results reported here. One effect of the apparent increase in the electorate's use of incumbency as a voting cue has been to decrease the proportion of competitive seats. We might conjecture that a congressman with a safe seat would be less concerned with responding to constituency demands. I hesitate to endorse this conclusion since part of the explanation of the increased incumbency effect may be found in the increased ability of sitting congressmen to satisfy constituency requests. Indeed, the increasing decentralization of the policymaking process in the Congress would seem to point in this direction. It may still be true that if a congressman decides not to make use of his many opportunities to assist his constituents, he would not benefit from any incumbency advantage. Indeed, congressmen and congressional scholars are able to recount many stories illustrating this very point. Obviously much more research is needed to settle these questions.

Appendix: Procedures

The following regression equation formed the basis for the analysis in the discussion of Behavioral Change Theories in this paper:

The regression equation that was estimated was

$$Y_k = \alpha + \sum_{i=1}^{5} \beta_i x_{ik} + \sum_{i=1}^{3} \gamma_i z_{ik} + \sum_{i=1}^{5} \sum_{j=1}^{3} \delta_{ij} x_{ik} z_{jk} + {} + \epsilon_{ik}, \qquad (A.1)$$

where Y_k = 1 if respondent voted for Democratic candidate
0 if respondent voted for Republican candidate

X_{1k} = 1 if respondent is a strong Democrat
0 otherwise

X_{2k} = 1 if respondent is a weak Democrat
0 otherwise

X_{3k} = 1 if respondent is independent-leaning Democrat
0 otherwise

X_{4k} = 1 if respondent is independent-leaning Republican
0 otherwise

X_{5k} = 1 if respondent is weak Republican
0 otherwise

Z_{1k} = 1 if respondent is aware of neither candidate
0 otherwise

Z_{2k} = 1 if respondent is aware of his own party's candidate
0 otherwise

Z_{3k} = 1 if respondent is aware of other party's candidate
0 otherwise

The initial least-squares estimates $(\hat{\alpha},\hat{\beta},\hat{\gamma},\hat{\delta})$ were employed to estimate the conditional probability that the kth respondent would vote Democratic as follows.

$$P(\text{k votes Democratic} \mid X_1 = x_{ik},\ X_2 = x_{2k}$$

$$\ldots X_5 = x_{5k},\ Z_1 = z_{1k},\ Z_2 = z_{2k},\ Z_3 = z_{3k}) =$$

$$\hat{Y}_k = \hat{\alpha} + \sum_{i=1}^{5} \hat{\beta}_i x_{ik} + \sum_{i=1}^{3} \hat{\gamma}_i z_{ik} + \sum_{i=1}^{5} \sum_{j=1}^{3} \hat{\delta}_{ij} x_{ik} z_{ik}.$$

Thus, since $(\hat{\alpha},\hat{\beta},\hat{\gamma},\hat{\delta})$ are consistent estimates of the parameters $\hat{Y}_k$ is also a consistent estimate and so one can obtain a consistent estimate of the variance of ϵ_k as $\hat{Y}_k(1-\hat{Y}_k)$. We employed these estimates to generate an estimated variance-covariance matrix and then to form the generalized least squares estimates $(\tilde{\alpha},\tilde{\beta},\tilde{\gamma},\tilde{\delta})$. These are reported below.

Table A. Generalized Least-Squares Estimates of Equation A.1

	1958		1964		1966		1968		1970	
	Coeff.	Std. errors	Coeff.	Std. errors	Coeff.	Std. errors	Coeff.	Std. errors	Coeff.	Std. errors
$\tilde{\alpha}$	.085	.04	.133	.04	.050	.03	.119	.05	.038	.05
β_1	.878	.04	.748	.05	.814	.06	.738	.06	.787	.07
β_2	.712	.06	.648	.07	.634	.08	.523	.08	.586	.08
β_3	.524	.10	.597	.09	.617	.13	.714	.08	.337	.16
β_4	.248	.11	.158	.10	.283	.12	.068	.08	.362	.15
β_5	.242	.07	.229	.07	.220	.08	.209	.08	.179	.09
γ_1	−.085	.04	−.133	.04	.013	.07	−.119	.04	−.038	.04
γ_2	.486	.18	−.133	.04	.700	.21	−.119	.04	−.038	.07
γ_3	−.069	.04	−.133	.04	.050	.06	−.070	.05	−.008	.05
δ_{11}	.122	.04	.239	.06	.124	.08	.208	.07	.163	.09
δ_{21}	.257	.07	.216	.08	.303	.10	.432	.09	.368	.09
δ_{31}	.476	.10	.403	.09	.121	.19	.063	.16	.663	.17
δ_{41}	−.248	.11	−.158	.10	−.346	.13	−.068	.08	−.218	.20
δ_{51}	−.214	.08	−.047	.13	−.211	.12	−.209	.08	−.179	.09
δ_{12}	−.825	.22	−.248	.20	−.939	.27	−.147	.16	−.429	.15
δ_{22}	−.394	.21	−.016	.12	−1.241	.26	−.142	.16	−.433	.14
δ_{32}	−.595	.28	.003	.23	−1.167	.30	−.521	.14	.163	.26
δ_{42}	.180	.21	.342	.26	−.478	.29	.333	.16	.210	.24
δ_{52}	−.225	.22	.464	.14	−.278	.25	.422	.19	.266	.19
δ_{13}	.087	.05	.208	.06	.066	.08	.020	.09	−.082	.09
δ_{23}	.191	.07	.242	.07	.125	.10	.137	.10	.004	.10
δ_{33}	.243	.13	.153	.12	−.190	.18	−.404	.13	.133	.19
δ_{43}	−.077	.15	.231	.15	−.071	.17	.130	.12	−.342	.16
δ_{53}	−.165	.08	.073	.10	−.247	.10	−.176	.09	−.118	.11
N	853		845		565		755		585	
R^2	.653		.488		.530		.464		.558	

	1958-1964		1958-1966		1958-1968		1958-1970	
	Coeff.	Std. errors	Coeff.	Std. errors	Coeff.	Std. errors	Coeff.	Std. errors
$\tilde{\alpha}$	.112	.03	.069	.03	.098	.03	.066	.03
β_1	.810	.04	.860	.03	.795	.04	.830	.04
β_2	.677	.05	.680	.05	.579	.05	.647	.05
β_3	.561	.07	.560	.08	.574	.07	.402	.09
β_4	.197	.07	.264	.08	.109	.06	.291	.09
β_5	.233	.05	.234	.05	.211	.05	.204	.06
γ_1	−.112	.03	−.046	.03	−.098	.03	−.066	.03
γ_2	.123	.09	.567	.14	.045	.09	.184	.15
γ_3	−.102	.03	−.025	.03	−.068	.03	−.045	.03
δ_{11}	.183	.04	.117	.04	.147	.05	.152	.05
δ_{21}	.231	.06	.275	.06	.302	.07	.264	.07
δ_{31}	.439	.07	.299	.11	.292	.11	.597	.10
δ_{41}	−.197	.07	−.288	.08	−.109	.07	−.224	.11
δ_{51}	−.169	.06	−.218	.06	−.194	.06	−.190	.06
δ_{12}	−.454	.13	−.871	.17	−.331	.14	−.581	.18
δ_{22}	−.198	.12	−.760	.19	−.198	.15	−.488	.19
δ_{32}	−.251	.18	−.832	.21	−.440	.16	−.153	.22
δ_{42}	.234	.22	−.264	.22	.081	.18	−.041	.25
δ_{52}	.165	.13	−.237	.17	.090	.16	−.086	.19
δ_{13}	.150	.04	.078	.04	.026	.05	−.030	.05
δ_{23}	.218	.05	.159	.06	.141	.06	.071	.07
δ_{33}	.193	.09	.064	.11	−.124	.10	.135	.11
δ_{43}	.086	.11	−.058	.11	.028	.09	−.232	.10
δ_{53}	−.047	.06	−.194	.06	−.178	.06	−.156	.06
N	1698		1418		1561		1391	
R^2	.560		.589		.530		.599	

The Case of the Vanishing Marginals: The Bureaucracy Did It*

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Introduction

The ongoing Erikson-Mayhew-Tufte-Burnham-Ferejohn exchange illustrates once again that political events and processes are easier to describe than to explain. We now are aware of a clear political trend: the decline of competition for House seats. The significance of this trend becomes apparent when coupled with evidence that policy change in the Congress results more from the replacement of incumbents than from changes in their behavior.[1] Seemingly, the primary determinants of change in national policies will soon be individual deaths and retirements rather than elections.

But what are we to do? Any attempt to stimulate competition in congressional elections presupposes an understanding of the factors which foster and inhibit it. And here E-M-T-B-F come to a parting of the ways. Somewhat casually, Tufte suggests that institutional change, namely redistricting, has put marginal districts on the endangered list. Ferejohn rejects Tufte's suggestion, as does Bullock in another contribution.[2] Among other possibilities, Mayhew recalls the venerable Stokes-Miller dictum that "to be known at all is to be known favorably" and exhibits evidence that now, more than ever, congressmen follow the sage advice, "use the frank, use the frank, use the frank." But here too, Ferejohn raises questions. Neither in absolute nor in relative terms do the incumbents of today enjoy a greater informational advantage than those of yesteryear.

Having discarded two potential explanations Ferejohn next offers his own. Following Erikson and Burnham, Ferejohn argues that changes in electoral behavior underlie the vanishing marginals. Perhaps many citizens use simple rules of thumb when voting in low information elections such as those for the House. According to proponents of the behavioral change view, party identification probably serves as the most common rule of thumb. But in recent years the citizenry has apparently become more informed, issue conscious, and ideological. And as this changing electorate monitored the divisive, highly charged politics of the 'sixties, increasing numbers of citizens grew suspicious of their traditional rule of thumb. Ferejohn and others suggest that incumbency voting has filled the void left by weakening party identification: for a significant number of citizens voting for the incumbent has replaced voting for their party.

The preceding argument has a curious ring to it. On the one hand we are to believe that party identification has declined in importance because citizens are increasingly aware and informed. But on the other hand we are to believe that these same citizens increasingly rely on the seemingly simpleminded rule of voting for incumbents. In a nutshell, voters are getting smarter, while voting behavior (other than presidential) is getting dumber. Moreover, consider the data analyzed by Arthur Miller.[3] With citizens increasingly dubious about government competence, intentions, and efficiency, is it plausible to argue that they increasingly support the objects of their cynicism?

Marginal districts are on the wane. Why? Some kind of incumbency effect exists, and apparently has come to exert an increasingly strong influence on congressional elections. But what is the nature of the incumbency effect, and why has it become more important over time? In this comment I propose another possible answer to the preceding questions. My thesis emerged during explorations of two congressional districts relatively alike in their demographic profiles but strikingly different in their electoral history. Basically, I will argue the following.

In the postwar period we have seen both the decline of the marginal district and the expansion of the federal role and its attendant bureaucracy. I believe that these two trends are more than statistically related, that they are in fact causally related. An institutional change—the growth of the bureaucracy—has encouraged behavioral change *among congressmen*, which in turn has encouraged behavioral change among some voters.

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*Without implicating any of them, I wish to thank Richard Fenno, John Kingdon, Charles Bullock, David Mayhew, Douglas Price, and Glenn Parker for their thoughtful comments and criticisms.

[1] Herbert Asher and Herbert Weisberg, "Congressional Voting Change: A Longitudinal Study of Voting on Selected Issues," paper presented at the *American Political Science Association Meeting*, San Francisco, 1975.

[2] Charles S. Bullock III, "Redistricting and Congressional Stability, 1962–1972," *Journal of Politics* 37 (May, 1975), 569–575.

[3] Arthur H. Miller, "Political Issues and Trust in Government: 1964–1970," *American Political Science Review*, 68 (September, 1974), 951–972.

The Two Districts

The two congressional districts studied are reasonably similar in their demographic profiles. Both are in the same region of the country. Neither is metropolitan nor rural—each contains more than two counties, one medium-size city, and an important agricultural sector. Politically, however, the two districts present a striking contrast. District A is the quintessential marginal district. Since its creation in 1952 no election has produced a victory percentage as high as 58 per cent; the average winning percentage is 53. Both parties have won the seat with at least two different candidates during the 1952–1974 period. In contrast, the political history of district B illustrates Burnham's "triumph of incumbency."[4] Until 1964, Republicans won the district with margins around 55 per cent. But in 1964 a Democrat squeaked through, held on in 1966, and from the statistical record now appears safe.

Are there important differences between the two districts? Why did the Democrat who captured district A in 1964 not duplicate the feat of his counterpart in district B? Why has district A not experienced the triumph of incumbency on the Republican side? One explanation we can eliminate is redistricting. District A underwent no boundary change between 1952 and 1972, and then underwent a change amounting to less than 5 per cent of the district population. District B has remained unchanged since World War II. What then, explains the political differences between districts A and B?

Consider District A. During the 1950s both national and state races activated local ties, to Republican advantage in the former case and Democratic advantage in the latter. Congressmen rose and fell partially for reasons beyond their control. The Republican congressman who followed the Eisenhower era was a rural conservative, a crusty personality of unquestioned integrity, who took pride in his attendance record, perceived his job as the making (and obstructing) of national policy, and in general operated rather independently of his district. In 1964 he refused to separate himself from Goldwater and followed his leader into enforced retirement. Unlike the beneficiary of the Johnson landslide in district B, however, the Democrat in district A failed to retain his seat in 1966, losing it to the Republican he defeated two years earlier. In 1974, after more narrow victories, the conservative Republican retired. his party held the seat—by the narrowest of margins—but now Democrats and Republicans alike agree that after a year in office the freshman Republican successor is safe.

Why this turnabout, this triumph of incumbency? Several explanations are offered. The new Republican blankets his district with communications both greater in number and more "effective" than his predecessor's. District observers perceive that the freshman Republican's voting record is more closely attuned to his district than was his predecessor's conservative stands. "He throws a few votes our [opponents'] way now and then." And finally, the freshman Republican travels around his district from meeting to meeting saying, "I'm your man in Washington. What are your problems? How can I help you?" While generally favorable to his successor, the former Republican disapproves of the amount of time his successor spends in the district: "How can he do his job in Washington when he's back here so much? People shouldn't expect a Congressman to be running back home all the time."

In summary, district A was influenced by broader political forces during the 1950s. During the 1960s it elected congressmen who did not make all-out efforts to maximize their vote. Now that someone is doing so, local observers and participants are betting that another marginal has vanished.

Now consider district B which is simply ten years ahead of district A. Until 1964 district B was marginally Republican. The Representative for most of this period was involved in controversial legislation such as Taft-Hartley and Landrum-Griffin. He had a personal problem, and probably more important, a political problem: declining Republican registration. In 1948 Republicans had a comfortable registration edge, by 1964 this edge has dwindled, and today the parties are dead even. Some district politicians believe that the registration shift has little significance for national elections, that it is felt mostly on the local level. But it seems prudent to bear in mind that the triumph of incumbency in district B may reflect the changing political allegiances of the district. A defeated Republican congressman is partial to this view.

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[4] Walter D. Burnham, "Party Systems and the Political Process," in *The American Party Systems*, ed. William Chambers and Walter Burnham, 2nd ed. (New York: Oxford, 1975), pp. 308–357.

Still, the Democrat who barely won in 1964 bucked the tide in 1966 and has won by margins of 40,000 votes at times since. District observers agree that his strength is bipartisan; a county chairman contends that in only one instance has any other national, state, or local candidate of either party run ahead of the Democratic congressman in the district, or in the relevant common subarea of it. Equal registration does not explain such electoral one-sidedness.

But here again we find a behavioral difference between the pre-1964 Republican and the post-1964 Democratic congressmen. Although district B is not located in prime Tuesday-to-Thursday club area, the Democratic incumbent belongs to the club nonetheless. By general agreement he is a pervasive presence in the district. He relies on no campaign organization other than the formal party structure. But he personally works the district at a feverish pace. A party chairman from a Republican area commented: "Congressman ______ comes to see people. ______ [former Republican congressman] didn't. The people know ______. He's the first Congressman to take an active interest in them."

The Democratic incumbent maintains well-staffed offices in district B. In these offices secretaries busily work on social security and veterans' affairs matters. Here too we find a difference between the Democratic incumbent and his Republican predecessor. The latter commented: "When I was in office I had four staff members. Now they have a regiment. that's just not necessary. It's a waste of the taxpayer's money, a frivolous expense."

The matter of the congressional staff is especially interesting in that the retired Republican in district A spontaneously brought it up. In discussing examples of the "hypocrisy" of modern congressmen (one of which was the 1967 expansion of the staff) he said flatly: "No congressman could possibly use 16 staff members." The Democratic incumbent in district B is using them (ten in his district) and one can not dispute the results.

Clearly, the two districts show evidence that major changes in their congressional election patterns are associated with behavioral differences on the part of the congressmen they elected. What might produce such differences? Former Republican congressmen in the two districts lean toward the view that today's congressmen are not as good as the pre-1960 variety. Oversimplifying a bit, in olden days strong men walked the halls of the Capitol. They concentrated more heavily on affairs of state than do their successors. More than today's congressmen they believed that the public interest should take precedence over reelection.

Political scientists are justifiably skeptical of theories which postulate that human nature has changed for the worse, that yesterday's political giants have given way to today's political pigmies. Thus, I will not dwell on the notion that today's congressmen are more concerned with reelection than they were in the recent past. In all likelihood, since the New Deal era the average congressman's desire for reelection has remained constant. What has changed, however, is the set of resources he possesses to invest in his reelection effort. Today's congressmen have more productive reelection strategies than previously. And these strategies are an unforeseen by-product of the growth of an activist Federal government.

Better to be Reelected as an Errand Boy Than Not to be Reelected at All

A plausible explanation of the differing political histories of the two cases I have studied would run something like this. The changing nature of congressional elections in these districts stems from the changing behavior of the congressmen who represented these districts. Both districts are heterogeneous in their socioeconomic characteristics, and in their political allegiances (e.g., registration). Thus, so long as these districts are represented by congressmen who function primarily as national policy makers (pre-1964 in district B, pre-1974 in district A) reasonably close congressional elections will naturally result. But given congressional incumbents who place heavy emphasis on nonpartisan constituency service, the districts will shift out of the marginal category. Can we expand this explanation, and use it to explain the vanishing marginals nationally?

A basic fact of life in post-New Deal America is the growth of the federal role and its instrument, the federal bureaucracy. Bureaucracy is the characteristic mode of delivering public goods and services. Ceteris paribus, the more the government attempts to do for people, the more extensive a bureaucracy the government will require.

While not malevolent, bureaucracies make mistakes (of commission and omission). Moreover, attempts at redress often meet with a characteristic unresponsiveness, inflexibility, and incorrigibility. Members of the U.S. Congress, however, hold an almost unique position vis-à-vis the bureaucracy: congressmen possess the power to expedite bureaucratic activity. This capability flows directly from congressional control over what bureaucrats value most—higher budgets and new program authorizations. In a very real sense congressmen are monopoly suppliers of bureaucratic "unsticking" services.

As the scope of government expands, more and more citizens and groups find themselves dealing with the federal bureaucracy. They may be seeking positive benefits such as social security checks and government grants or seeking to escape costs entailed by bureaucratic regulations. But in either case their congressman is a source of succor. And the greater the scope of government activity, the more often will his aid be requested. Moreover, unlike private monopolists, congressmen can not curtail the demand for their services by raising their price (at least legally). When the demand for his services rises, the congressman has no real choice except to meet that demand—to supply more—so long as he would rather be an elected official under any circumstances, than an unelected one. This vulnerability to constituency demands, however, is largely academic. Congressmen probably do not resist the gradual transformation from national legislator to errand person. They have not rushed to create a national ombudsman, for example, nor to establish Congressman Reuss's Administrative Counsel of the Congress. The nice thing about casework is that it is mostly profit; one makes many more friends than enemies. In fact, some congressmen undoubtedly stimulate the demand for their bureaucratic fixit services. Recall that the new Republican in district A says, "I'm your man in Washington. What are your problems? How can I help you?" And in district B the demand for the congressman's services presumably did not rise so much between 1962 and 1964 that a "regiment" of constituency staff become necessary. Rather, possessing the regiment, the new Democrat did his damnedest to create the demand to which he could apply his regiment.[5]

In addition to profitable casework let us remember too that the expansion of the federal role has also produced a larger pork barrel. The pork barreler need not limit himself to dams and post offices. There is LEAA money for the local police; urban renewal and housing money for local officials; and educational program grants for the local education bureaucracy. The congressman can stimulate applications for federal assistance, put in a good word during consideration, and announce favorable decisions amid great fanfare. Bureaucratic decisions bestow benefits as well as create costs. By affecting either kind of decision, the congressman can accrue electoral credit.

Let us turn now to the matter of the incumbency effect. If, over time, an increasing number of U.S. representatives are devoting increasing resources to constituency service, then at the district level we would expect that increasing numbers of voters think of their congressman less as a policymaker than as an ombudsman. If so, other implications are immediate. First, party identification will be less influential in determining the congressional vote, not just because of the unusual presidential politics of the late 1960s, but because *objectively* the congressman is no longer considered so important for policymaking as he once was.[6] In legislative matters he holds one paltry vote out of 435. But in bureaucratic matters he is a benevolent, nonpartisan power. And if more and more citizens come to think of their congressmen in this manner, then the basis of the incumbency effect is obvious. *Experience in Washington and congressional seniority count when dealing with the bureaucracy.* Thus, so long as the incumbent can elude a personal morality rap, and refrain from casting outlandish votes, he is naturally preferred over a newcomer. *This incumbency effect is not only understandable, it is rational.* And it would grow over time as increasing numbers of citizens come to regard their congressman as a troubleshooter in the Washington bureaucracies.

The preceding argument provides a critical insight into Ferejohn's critique of Mayhew. Ferejohn concludes that the incumbency effect is not explained by the information that incumbents shower on constituents, because the informational advantage incumbents possess has not increased between 1958 and 1970, while the incumbency advantage apparently *has* increased during that period. But what if the *content* of the information has changed over time? What if in 1958 voters who have "heard or read something" about the incumbent have heard or read about a policy stand, whereas in 1970 they have heard or read about the good job the incumbent is doing to get Vietnam veterans' checks in the mail? Some voters will agree with a policy stand, some will disagree. But everyone will applaud the congressman's efforts in behalf of the veterans. Thus, a constant informational advantage may be quite consistent with an increasing incumbency advantage if information about the incumbent has become increasingly noncontroversial in content. And, as suggested above, those voters who have "heard or read nothing" about either candidate act quite sensibly in voting for the incumbent to the extent that he is an ombudsman rather than a legislative giant.

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[5] The expansion of the congressional office cries out for further study. At the beginning of the 90th Congress in which the last major expansion took place *Congressional Staff Directory* listed 3,276 individuals of whom 26 per cent were in the districts. In 1974, 34 per cent of 5,109 were in the districts. What are these people doing?

[6] Cf. Burnham, "Party Systems," p. 335. Burnham believes that the decline of party identification as an influence on congressional voting has increased the attractiveness of the ombudsman role. I am arguing that the causal influence is reciprocal if not the reverse.

For clarity's sake I have drawn the preceding argument in very bold strokes. Let me now fill in the picture a bit. In order to account for the vanishing marginals we do *not* need to argue that *all* congressmen have opted *exclusively* for an ombudsman role, and that *all* constituents now think of their congressmen in nonprogrammatic terms. The disappearance of a marginal requires only marginal change. To illustrate, if one deflates Mayhew's 1972 bimodal distribution by Erikson's 5 per cent estimated incumbency effect, the trough in the marginal range disappears. To explain the vanishing marginals one need only argue that over the past thirty years, expanded constituency service opportunities have given the marginal congressman the ability to capture 5–10 per cent of his district's voters who might otherwise oppose him on party or policy grounds.

One further question arises. The growth of the federal role has been continuous and reasonably gradual, although with definite jumps during the New Deal and World War II. The decline of congressional competition, however, has been more erratic. We would expect some lag between the onset of bureaucratic expansion and the decline of the marginals because congressmen presumably would not grasp the new opportunities immediately. But how would we explain the especially pronounced decline of congressional competition in the late 'sixties? It would be a bit much to contend that Great Society programs translated into casework and votes quite so quickly. One plausible explanation of the 'sixties decline lies in recent work by Richard Fenno.[7]

Fenno discusses the congressional "homestyle," a congressman's basic patterns of interaction with his district. According to Fenno, homestyles tend to persist once established. Now consider that between the 88th and 90th Congresses one-third the membership of the House changed. I think it is plausible to hypothesize that the homestyles of the new representatives placed relatively greater emphasis on constituency service than did the homestyles of the more senior congressmen they replaced.[8] The average freshman in 1965 for example, replaced a congressman elected in 1952 or 1954. Paradoxically, then, the electoral upheavals of the 1960s may have produced the electoral stability of the early 1970s. New congressmen chose homestyles best adapted to the changed congressional environment. Is it purely coincidence that these fresh Congresses raised staff allotments by almost 50 per cent (eleven to sixteen) between 1967 and 1973?

Conclusion

Congressmen can earn electoral credit by taking positions on the issues, by bringing home the bacon, and by providing individual favors. The first option is inherently controversial. The latter two need not be. As the federal role has expanded, congressmen have shifted emphasis from the controversial to the noncontroversial, from the programmatic to the nonprogrammatic. Ferejohn no doubt is correct; electoral behavior has changed. But at least part of that change is endogenous to the system. Congressmen are not merely passive reactors to a changing electoral climate. In no small part they have helped to change that climate.[9]

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[7] Richard Fenno, "Congressmen in their Constituencies: An Exploration," forthcoming, *American Political Science Review.*

[8] I should point out that the Democrat who won district A in 1964 and lost it in 1966 did *not* adopt an errand boy homestyle. According to local supporters he became totally engrossed in his Washington affairs.

[9] Obviously, my argument suggests a variety of implications for the future operation of the American government. Space precludes me from entering upon such a discussion here. The interested reader should refer to my *Congress–Keystone of the Washington Establishment* (New Haven: Yale, 1977).

Barber's Typological Analysis of Political Leaders*

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The Personality Factor

In explanations of the activities loosely classified as "Watergate," the relative importance of "ready money," of organizational pathologies, and of the personalities of protagonists has depended on which "truth about Watergate" was "operative" at the time the explanation was offered. When the "truth" incorporated only "undersupervised," "overzealous," free-spending subordinates, the emphasis on campaign financing and organization seemed appropriate. Since the culpability of Mr. Nixon and his senior advisors has become indisputable, however, the argument for the greater importance of the protagonists' personalities has become more cogent.

Of the many observers now persuaded by this argument, few appear to expect from it a practicable lesson about personality. Understanding the role money played has furthered the reform of campaign finance laws. Likewise, awareness of the encroachments of unrestrained organizations has prompted at least a few surface changes in executive branch operations. But recognition of the role of personalities seems only to have produced great sighs and occasional generalizations of the dementia, depravity, and despotic predispositions seen in the characters of the Watergate cast.

There are those, nevertheless, who contend that a practical lesson can be drawn from what is commonly perceived as an idiosyncratic basis of presidential behavior: if one knows where to look, one can discern personality dimensions that can serve as further criteria for selecting presidents.

So argues James David Barber, the author of a long-developing theory of presidential behavior, which he poses not only as a viable scheme for the systematic study of presidents but also as a practicable basis for predicting how presidential candidates would behave in office. The development of the theory culminates in *The Presidential Character: Predicting Performance in the White House,*[1] a study of Richard Nixon and each preceding president back through William Taft.

A Theory of Presidential Character. Barber proposes that a president is best conceived of "as a human being like the rest of us, a person trying to cope with a difficult environment," and that to this task of coping, a president "brings his own character, his own view of the world, his own political style."[2] These three Barber sees as patterned—as forming a "combination" that "makes psychological sense" to the man. They fit together in a "dynamic package" that is the president's personality.

"Style," says Barber, is "the most visible" of the three components; he defines it as "the President's habitual way of performing his three political roles: rhetoric, personal relations and homework." "World view" he defines as a president's "primary, politically relevant beliefs, particularly his conceptions of social causality, human nature, and the central moral conflicts of the time." "Character" he poses as the most important of the three components; it is a person's enduring orientation toward life, the person's "stance as he confronts experience." "Experience," however, encompasses more than observing and reacting to externalities. The man himself comprises a portion of that experience, and his "stance" as he confronts himself is "at the core of character." In this confrontation the paramount question is, How does he appraise himself? By this Barber means, first, What criteria does he use in judging himself? Second, What judgment does he arrive at using these criteria? Or as Barber phrases it, "Down there in the privacy of his heart, does he find himself superb, or ordinary, or debased, or in some intermediate range?"

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*I want to express my appreciation to Stephen V. Stephens, Alan J. Eade, Jeffrey M. Berry, David W. Depew, Edward V. Heck, Robert L. Peabody, and Frances E. Rourke for their suggestions and advice. In addition, I want to thank the *Review*'s anonymous referees and its manuscript editor, Ellen Siegelman, for their extremely helpful comments. Finally, I wish to express my gratitude to my wife, Deborah, for her support. Her knowledge and skill as a clinician have proved to be invaluable resources in my study of personality and politics. Responsibility for the contents of the article remains, of course, solely mine.

[1] James David Barber, *The Presidential Character* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1972).

[2] Unless otherwise identified, quotations in the following section are from Barber, *The Presidential Character,* pp. 3–14.

The question is paramount because self-esteem is a "prime personal resource" that a person carefully looks after. When self-esteem is tenuous or inadequate, he defends or advances it even at the "sacrifice [of] much else he values." The manner in which he defends or advances it—the strategy of adaptation he employs—depends upon the criteria by which he judges himself. Barber distinguishes four different sets of criteria: productiveness, power, loveableness, and usefulness. Every person employs all four but usually accentuates one. Barber argues that people who in judgments of themselves accentuate power, loveableness, or usefulness have low self-esteem, whereas those who accentuate productiveness have high self-esteem. The consonant adaptive strategies for the criteria are as follows. Those who judge themselves impotent show aggression and compulsion and "struggle to achieve and hold power." Those who judge themselves unloveable are "receptive, compliant, other-directed character[s]" who "search for affection as a reward for being agreeable and cooperative"; in short, they are best characterized as submissive. Those who judge themselves useless are oriented "toward doing dutiful service"; they "emphasize their civic virtue," but tend to withdraw in order "to escape from the conflict and uncertainty of politics." In contrast with these three is the person with high self-esteem; his behavior is not pervaded by a compensatory strategy. Instead he "shows an orientation toward productiveness as a value and an ability to use his styles flexibly, adaptively" and seems to "want most to achieve results."

It is of utmost importance that these differences in self-estimates and in criteria manifest themselves phenomenologically. Barber contends that they are captured in a quadripartite classification of presidents according to two dichotomized dimensions. On the first a president is classified as either active or passive, according to the amount of energy he invests in the office. On the second he is classified as either positive or negative, according to how he feels about what he does, that is, according to whether or not he enjoys his presidential role. Active-positives esteem themselves highly and are oriented toward achievement; active-negatives adjudge themselves impotent and struggle for power; passive-positives have low self-esteem; they employ the criterion of loveableness and are unduly submissive; passive-negatives deem themselves useless but seek to fulfill their civic responsibilities.

Barber is not explicit about his reasons for according character heightened importance relative to style and world view. A president's character certainly affects his ability to deal with the imperatives of the office. But so do his world view and style. Why then does Barber emphasize the first? Undoubtedly part of the answer—but only part, as we shall see—is that Barber conceives character to be an enduring orientation that develops earlier in life and contributes to the shaping of world view and style.

Barber does not, however, pose personality as *the* determinant of behavior. The environment in which a president functions is an integral part of the theory. Specifically Barber sees a president's personality interacting with two environmental variables: "the power situation [the president] faces" and "the national 'climate of expectations' at the time he serves." Straightforward interpretations of these two are not easily arrived at, but a few clues that point the reader to narrower definitions are given by Barber. The power situation is a composite of "the support [a president] has from the public and interest groups, the party balance in Congress, [and] the thrust of Supreme Court opinion." The second variable, "the climate of expectations" during a president's tenure, exhibits itself in at least three ways, or evinces "three recurrent themes," according to Barber. These themes, which Barber has also termed "the predominant needs thrust up to [the President] by the people," are (1) the need for "reassurance," (2) "the demand for a sense of progress and action," and (3) "the public need for a sense of legitimacy." According to Barber, "the tuning, the resonance—or lack of it—between these external factors and [the President's] personality sets in motion the dynamic of his Presidency."[3]

Finally Barber proposes that prediction of a president's personality (or the nature of this dynamic package of "character, world view and style") is possible if attention is focused on the way the components "were put together in the first place." Each component tends to develop at a different stage in a person's life: character during childhood, world view during adolescence, and style during early adulthood. The components come together, however, usually in early adulthood, to form a basic and fairly durable pattern. The locus of this conjunction is the individual's "first independent political success." It is most often "the time of emergence, the time the young man found himself."

The Nixon Prediction. Barber classifies presidents since Theodore Roosevelt according to their activity and affect toward that activity, explains their performances as being predominantly the consequence of their character, and from the explanations extrapolates similar performances by future presidents of the same character types.[4] Postdictions are not, however, the only evidence that the theory is practicable. *The Presidential Character* was published in 1972, and a substantial portion of the book is devoted to an analysis of Richard Nixon and to a prediction of his behavior in the event of his re-election.

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[3] Cf. Theodore H. White's analysis of the lack of resonance between the themes George McGovern symbolized and the mood of the American people in 1972. *The Making of the President 1972* (New York: Bantam, 1973), pp. 494–99.

The Richard Nixon who emerges from Barber's book is an active-negative. He is a man in whose makeup the central theme, "aggression and its control,"[5] must compete with a second theme, "the drive to attain and maintain power and independence";[6] a man for whom " 'principles' are more important as rationalizations justifying behavior than as guides for choice";[7] and, as a consequence, a man involved in managing his aggression and concerned with "getting, holding, and protecting power."[8] For Barber's Nixon "the task is not to understand but to defend."[9] He is busy "constructing himself retrospectively," ever "deducing a view of his character and its action from what anyone should have been and done" in similar circumstances.[10]

Barber's prediction fitted his image of the man and accorded with the general prediction for active-negatives.[11] Barber forecasted a presidential crisis rooted in Mr. Nixon's character and more significant than any of Nixon's previous crises:

> The danger is that crisis will be transformed into tragedy—that Nixon will go from a dramatic experiment to a moral commitment, a commitment to follow his private star, to fly off in the face of overwhelming odds. That type of reaction is to be expected when and if Nixon is confronted with a severe threat to his power and sense of virtue.[12]

Considering these words from a perspective shaped by months of unprecedented political turmoil, most readers of *The Presidential Character* will undoubtedly deem the prediction accurate.[13]

Organizing an Assessment. Theory that is complete with seemingly accurate prediction is rare in political science. Accordingly, Barber's work merits attention, but not in the form of recognition alone and not simply directed to the prediction. Barber's goal after all is to provide a theory that can be used to predict the performances of presidents; and an accurate prediction itself does not establish the theory that produced it: Valid theories whose premises are true imply predictions that are accurate, not the other way around; and no amount of acclaim for the prediction can reverse this implication. (In the present case, moreover, Barber's theory does not even imply his Nixon prediction. The rigidification of active-negatives and its tragic consequences is a generalization of Barber's explanations of specific performances by three active-negative presidents—Woodrow Wilson, Herbert Hoover, and Lyndon Johnson. The prediction is thus at least three, long steps removed from the theory. Intervening are Barber's conclusions that these presidents were active-negatives and that their characters were the primary determinants of their performances.) It is therefore appropriate that some students of personality and politics have tempered their acclaim for Barber's theory with careful criticism.[14] The reviewer who has written the most exhaustive and balanced critique is Alexander George.[15]

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[4] For a further discussion see Alexander George, "Assessing Presidential Character," *World Politics*, 26 (January 1974), 234–82, especially 249.

[5] *The Presidential Character*, p. 364.

[6] *Ibid.*, p. 365.

[7] *Ibid.*, p. 353.

[8] *Ibid.*, p. 373.

[9] *Ibid.*, p. 365.

[10] *Ibid.*, p. 357.

[11] George has summarized the general predictions for all four types. "Assessing Presidential Character," p. 250. The following is taken from that summary. (Page numbers in parentheses are pages in *The Presidential Character* from which George's quotations are taken.)

> . . . *Active-positives* . . . "display personal strengths specially attuned to the Presidency, strengths which enabled them to make of that office an engine of progress"(p. 210) . . . [but] "in their haste to make things happen, may too quickly and easily knock down the 'formalities' that hold the democratic order in place" (p. 246).
>
> The *active-negatives* . . . have character-rooted needs that "invade and dominate, to an unusual degree, their political habits and perceptions" (p. 141). They tend to "persevere rigidly in a disastrous policy"(p. 95).
>
> *Passive-negatives* . . ."pose a different danger . . . the danger of drift" (p. 145). They leave "vacant the energizing, initiating, stimulating possibilities of the role"(p. 173). Yet in certain historical circumstances this type can provide a needed "breathing spell" for recovery after a period of frantic politics (p. 145).
>
> *Passive-positives* . . . are, like the passive-negatives, "responders, not initiators or pushers . . ." (p. 174). Yet, "for a people in search of community, they provide a refreshing hopefulness and at least some sense of sharing and caring"(p. 206).

[12] *The Presidential Character*, p. 441.

[13] Certainly Barber's own sense of its accuracy has been strengthened. See James D. Barber, "The Things We Might Have Seen," *The New York Times*, 8 November 1973, p. 47; and "President Nixon and Richard Nixon: Character Trap," *Psychology Today* (October 1974), pp. 113–18.

George praises Barber's style construct, which he sees bridging "role and personality variables" in studies of leaders' behavior,[16] and lauds Barber's "trenchant analysis of the styles" of twentieth-century presidents.[17] He concurs with Bruce Mazlish that Barber succeeds in sensitizing the reader to the various "facets of personality that are expressed in a leader's political behavior."[18] And he joins Fred Greenstein in commending Barber's recognition of the "diversity of motivation" of political actors.[19] But on the whole, George—like Jeanne Knutson, Daniel Katz, and Erwin Hargrove—perceives serious problems.[20] At least two of these are fundamental.

The first is the emphases Barber places on personality and on its component, character. George points out that although Barber "disavows reductionist explanations in terms of character, the core of his theory and its use in explanation and prediction of the most important aspects of presidential performance comes to rest upon the special significance he attributes to the role of character."[21] George argues that since Barber regards world view and style as also playing "an important role in determining performance in the Presidency," Barber should have provided for them in the typology he used to classify presidents.[22] George also notes a similar disparity in Barber's accentuation of "personality" relative to "major role demands" and situational constraints.[23]

Here George is directing attention to manifestations of a common difficulty in personality and politics research. If one attempts to establish "the critical causal importance" of character or simply the pre-eminence of personality, one risks stumbling into *psychological reductionism.* As George states, "It is not difficult to find evidence that a subject's character-rooted needs or motives, or other aspects of his personality, are *expressed* in his behavior in a particular situation [emphasis in original]," but, given the fact that behavior is "overdetermined," frequently "the most that can be said is that personality needs or other personality characteristics were among the many contributing factors."[24] George fears that Barber is indeed in danger of committing this "cardinal sin," especially in his analyses of Herbert Hoover and Lyndon Johnson.[25]

Just as an accurate prediction cannot establish that Barber's accentuation of personality and character is correct, so such a prediction cannot establish that the relationships Barber draws between the components of character are true. The unknown degree to which these relationships conform to reality, then, is the second problem. It is fundamental because, as George argues, the merit that can be associated with Barber's approach is ultimately contingent on the truth of the assumption "that in each of Barber's character types the various features attributed to that type do indeed go together, and only with each other."[26]

George himself attempts, in effect, to solve the first problem, that is, to determine if Barber supplies adequate evidence to support his accentuation of character. In the context of establishing whether predictions can be derived from the performances of previous presidents, George asks three logically primary questions, the second of which has as its subject the importance of character: (1) Are the performances of presidents of the same type "comparable enough to support [Barber's] generalization[s]?" (2) *"Is the critical role which Barber assigns to character in his explanation[s] . . . adequately demonstrated?* [my emphasis]" (3) "Does a similar psychodynamic pattern . . . underlie each man's behavior. . . ?"[27] George argues that affirmative answers to all three are necessary for the derivation of Barber's predictions. In asking the questions about those presidents Barber classifies as active-negatives, George is able, however, to answer none affirmatively.[28] The absence of evidence supporting Barber's accentuation of character suggests that Barber errs in his emphasis and also raises the question, On what basis, if not on the basis of clear evidence, did Barber assign character the critical role?

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[14] Jeanne N. Knutson, "Personality in the Study of Politics," in *Handbook of Political Psychology,* ed. Jeanne N. Knutson (San Francisco: Jossey-Bass Publishers, 1973), pp. 28–56; Daniel Katz, "Patterns of Leadership," in Knutson, pp. 203–33; Fred I. Greenstein, "Political Psychology: A Pluralistic Universe," in Knutson, pp. 438–69; Bruce Mazlish, review of *The Presidential Character,* by James David Barber, in *New York Times Book Review,* 8 October 1972, pp. 30–32; Erwin C. Hargrove, "Presidential Personality and Revisionist Views of the Presidency," *American Journal of Political Science,* 17 (November 1973), 819–35.

[15] "Assessing Presidential Character."

[16] *Ibid.*, p. 242.

[17] *Ibid.*, p. 243.

[18] *Ibid.*, p. 279, see also p. 255; and Mazlish, review of *The Presidential Character.*

[19] "Political Psychology: A Pluralistic Universe," p. 456.

[20] A recent article by Barber, "Strategies for Understanding Politicians," *American Journal of Political Science,* 18 (May 1974), 443–67, appears to be his response to his critics, whom he leaves unnamed. But he by no means deals satisfactorily with most of the criticisms, and especially with George's.

[21] "Assessing Presidential Character," pp. 244–45.

[22] *Ibid.*, p. 246.

[23] *Ibid.*, pp. 249, 253, and 255. The preeminence of personality is, however, a necessary assumption for Barber's general predictions. If all three potential determinants were accorded equal importance, predictions extrapolated from previous performances would be contingent on *ceteris paribus* conditions.

[24] *Ibid.*, p. 253.

[25] *Ibid.*, pp. 255–60.

[26] *Ibid.*, p. 251.

Concerning the solution of the second problem, George simply notes that the truth of the relationships Barber premises will be hard to establish in *The Presidential Character,* because the data Barber supplies are not univocal.[29] Unrecognized by George, however, is a more important reason that *The Presidential Character* is an inappropriate source for establishing the truth of the premises, a reason at the same time that the earliest of Barber's works is the appropriate source.

Because Barber tailors *The Presidential Character* from loosely woven argument, one must in any case evaluate his theory in the context of his earlier writings, if only to ascertain the nature of his constructs—some of which have evolved considerably since he introduced them to political scientists (see Table 1). George relies on these earlier writings in his critique, and he admonishes his readers to do likewise in their own evaluations of Barber's theory.[30] But he does not point out that Barber's earliest work, on the recruitment and adaptation of Connecticut legislators, contains information that is essential to the solution of the foregoing, fundamental problems. That is, he does not point out that the psychological reductionism evident in Barber's study of presidents is foreshadowed in Barber's study of legislators, that the reductionism in the earlier work is accompanied by an explicit rationale, and that the empirical basis of the character construct must be sought in this study of legislators.

The dependence of Barber's formulation of character on his conclusions in the legislative study is not patent. The link comprises a series of methodological steps, which lead from these conclusions, to the criteria Barber used to turn biographical information about presidents into data, to his rules for ascribing characteristics to presidents on the basis of these data, and finally to his judgments of the relationships between the subconstructs that compose character. The steps that form the link and the necessity of moving back down these to Barber's legislative study in order to find the empirical basis of character become apparent only with an examination of Barber's "Coding Scheme."[31]

The "Coding Scheme" contains the criteria Barber used to code presidential biographies; it is for the most part devoted to the style construct—only three of 39 pages are about character. Two of these three pages state the criteria for data on a president's activity and affect. The third page—quoted below in its entirety—offers a loose guideline for placing presidents in the categories Barber had used in

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[27] "Assessing Presidential Character," p. 256.

[28] *Ibid.*, pp. 256–71.

[29] *Ibid.*, p. 251.

[30] *Ibid.*, p. 240.

[31] "Coding Scheme for Presidential Biographies," Yale University, January 1968 (mimeographed).

A Summary of Selected Criticisms of Barber's Theory[a]

	Determinants	**Components[c]**	
		A. World View	
	I. Personality[b,e]	B. Style	Phenomenology[b]
Presidential performance[d]	II. Situational constraints	C. Character[b,e,g]	Dynamics[f]
	III. Roles		

[a] Derived from George, "Assessing Presidential Character," pp. 234–282. (Criticisms d, e, and f were specifically directed at Barber's development of the active-negative type; reservations should, however, be extended to all types.)

[b] Emphasized in Barber's theory, methods, and case studies.

[c] Although Barber premises that every personality component is important, he bases his classification on "character" alone.

[d] The performances of presidents of the same type are insufficiently comparable to support generalizations.

[e] Barber fails to demonstrate the relative criticalness of personality and, in turn, of "character" in determining performance.

[f] Barber fails to demonstrate the similarity of the psychodynamics of presidents of the same type.

[g] The quality of the data reduces their value as evidence of the validity of the character construct.

his legislative study and states the criteria for data on a president's strategy of adaptation and level of self-esteem. These criteria are in four sets. Each set contains the criteria for a particular strategy of adaptation and for either high or low self-esteem. I have placed the label of each strategy in brackets after the criteria for that strategy. I have likewise indicated the levels of self-esteem after their criteria.

> Tentative *Lawmakers* categories.
>
> This will be one of the last things to be coded. Coders who have not already done so should read *The Lawmakers,* particularly the summary of characteristics in Chapter VI. The above two variables—activity in the role, and affect toward the role—should place the president in one of four categories, that is active-positive, active-negative, passive-positive, passive-negative. Watch here for evidences of general tendencies in one of the following directions;
>
> Emphasis on watching, heroes, being impressed, ceremony, kindliness, mushiness, mannerly propriety, entertainment, dependence. [Submission] Sense of being unlovable, need for affection. [Low self-esteem]
>
> Emphasis on ambition, glory, striving, frustration, suffering, security, independence, fighting, defending, justifying. [Aggression] Sense of being vulnerable; need for power, confirmation. [Low self-esteem]
>
> Emphasis on service, principle, responsibility, harmony, procedures, systems, formalities, character, honesty and loyalty. [Withdrawal] Sense of being inadequate; need for some useful role. [Low self-esteem]
>
> Emphasis on rational mastery, productive activity, focus on substantive issues, achieve-

Table 1. Outline and Development of Barber's Theory of Presidential Character

Constructs and Subconstructs*				
Label	**Origin**	**Definition[a]**	**Origin**	**Comments**
I. Personality	i	The "patterned," "dynamic package" composed of "character, world view, and style." (6)	i	
A. World View	g	A president's "primary, politically relevant beliefs, particularly his conceptions of social causality, human nature, and the central moral conflicts of the time." (7–8)	g	
B. Style	e	"The President's habitual way of performing his three political roles: rhetoric, personal relations, and homework." (7)	g	Initially Barber applied this label to the four patterns of activity and affect and to their underlying dynamics. Later he extended it to cover, as well, a president's "habitual way of performing . . . political roles." Ultimately he has reserved it for the latter.
C. Character	g	"The way the President orients himself toward life." (8)	b,c	Barber applied this label to the four patterns of activity and affect and their underlying dynamics when he began using "style" more restrictively.
(Phenomenology)				
1. Activity	b,c	The amount of energy a man invests in the presidency. (11)	b,c	
2. Affect	e,d	"How he feels about what he does." (11)	e,d	This dimension in c is a legislator's attitude toward membership in the legislature. As a consequence of the dimension's contextual specificity and of the fact that "unlike state legislators, individuals who reach the emminence of the presidency are not likely to be uncommitted to their institutional role," Barber introduced a surrogate in his study of presidents.[j]

(continued on next page)

Table 1. Outline and Development of Barber's Theory of Presidential Character (continued)

Constructs and Subconstructs*				
Label	**Origin**	**Definition**[a]	**Origin**	**Comments**
(Dynamics) 3. Self-esteem	b,c	How the individual appraises himself. (8)	b,c	
4. Strategy of adaptation	b,c	Accentuated method of "defining and maintaining and raising self-esteem." (10)	b,c	The construct itself appears in all Barber's cited work, but in *The Presidential Charcater* it is not labeled. Barber initially used the label virtually interchangeably with "style" (see c, p. 21). Later he applied it more restrictively to "the bundle of strategies . . . for protecting and enhancing self-esteem." (See f, p. 95; and c, p. 214.)
II. Power Situation	g	"The support he [the president] has from the public and interest groups, the party balance in Congress, the thrust of Supreme Court opinion." (8)	g	
III. Climate of Expectations	g	"The predominant needs thrust up to him [the president] by the people." (9)	g	

*Constructs are indicated by Roman numerals and capital letters. Their use in this table is consistent with their use in "A Summary of Selected Criticisms." The four constructs that compose character ("activity," "affect," "self-esteem," and "strategy of adaptation") are labeled "subconstructs" in order to differentiate them more clearly from the other constructs of Barber's theory. "Subconstructs" are indicated by Arabic numerals.

[a] Definitions are those given in i. Numbers in parentheses are page numbers in i from which definitions are taken. The origin cited is the origin of the ideas embodied in the construct.

[b] "The Legislator's First Session" (Ph.D. dissertation, Yale University, 1960).

[c] *The Lawmakers: Recruitment and Adaptation to Legislative Life* (New Haven: Yale University Press, 1965).

[d] "Coding Scheme for Presidential Biographies," Yale University, January 1968 (mimeographed).

[e] "Adult Identity and Presidential Style: The Rhetorical Emphasis," *Daedalus,* 97 (Summer 1968), 938–68.

[f] "Classifying and Predicting Presidential Styles: Two 'Weak' Presidents," *Journal of Social Issues,* 24 (1968), 51–80; reprinted in *The Presidency,* ed. Aaron Wildavsky (Boston: Little, Brown, 1969), pp. 94-120.

[g] "The Interplay of Presidential Character and Style: A Paradigm and Five Illustrations," in *A Source Book for the Study of Personality and Politics,* ed. Fred I. Greenstein and Michael Lerner (Chicago: Markham, 1971), pp. 384–408.

[h] "The Presidency: What Americans Want," *The Center Magazine,* 4 (January 1971), 2–6.

[i] *The Presidential Character: Predicting Performance in the White House* (Englewood Cliffs, N.J.: Prentice-Hall, 1972).

[j] Fred I. Greenstein and Michael Lerner, "Typological Analysis of Political Leaders," in *A Source Book for the Study of Personality and Politics,* ed. Greenstein and Lerner, pp. 383–84.

> ment of personal standards. [Achievement] Sense of self as developing; need for producing results. [High self-esteem][32]

This section reveals that the guidelines for placing a president in a category in effect invited the use in the placement decision of data that pertained to characteristics other than the defining ones, characteristics that Barber had found in his legislative study to be related to the types and that he was hypothesizing would be similarly related in his presidential study. Coders were to decide on types by (1) familiarizing themselves with the clusters of characteristics Barber had associated with the types in his legislative study, (2) assessing the data derived from applications of the criteria for the defining characteristics to presidential biographies, and (3) watching for evidence of a president's strategy of adaptation and level of self-esteem. How the coders were to translate these into decisions about types Barber never says; he apparently left each coder to his own devices.

The section further reveals that in the presidential study Barber simply *assumed* rather

[32] "Coding Scheme for Presidential Biographies," p. 9.

than *proved* the relationships between the four particular strategies of adaptation and the levels of self-esteem he had associated with the strategies in his legislative study. Barber adjoined the criteria for each adaptive strategy with the criteria for the level of self-esteem he had associated with it in the earlier study, thus producing four sets. He instructed coders to use these sets to gather data from which to determine the set that was most representative of a president's "general tendencies." What needed to be determined first, though, was not which of these four sets was the most representative but whether the strategies whose criteria Barber had clustered with the criteria for particular levels of self-esteem go exclusively with those levels of self-esteem. In offering the coders the choice of only these four sets, Barber ignored an equal number of possibilities: the sets that combine each strategy of adaptation with the *other* level of self-esteem. He therefore constrained coders' choices of a strategy to be the choices of a level of self-esteem as well. The possibility of a president who, for example, emphasized "service, principle, and responsibility" and esteemed himself quite highly was thus precluded.

In sum, Barber's instructions insured that two conclusions about character were foregone: (1) that the relationships hold between strategies and levels of self-esteem; and (2) that the relationships hold between presenting-symptoms and dynamics. The character construct in Barber's presidential research was thus merely an assumption in the guise of an empirical conclusion of that research.

If Barber's purpose was to confront with evidence what are hypotheses derived from his legislative study, one could conclude that Barber merely erred in setting out his assessment criteria. There are indications, however, throughout Barber's presidential research that he was not seeking, even implicitly, to test these hypotheses, that he simply suspended disbelief and applied his construct of character as if its truth were established. Alexander George notes two occasions on which Barber asserts the validity of the character types.[33] In the first, Barber describes Harding as displaying "the *typical* passive-positive theme: the hunger for love, the impelling need to confirm one's loveableness" (my emphasis).[34] In the second, Barber's characterization of Coolidge and Eisenhower extends the validity of a character type even beyond presidents: "Both shared with other passive-negative people in politics a propensity for withdrawal. . . ."[35] Regardless, however, of whether Barber sought to test the hypotheses or simply accepted his earlier formulation of character as axiomatic, the effect is the same: the relationships between the subconstructs are simply assumptions that are predicated on his legislative research. Accordingly, the quest for the empirical basis of the character construct must go back beyond Barber's presidential research to his research on Connecticut legislators.

The Lawmakers: Core of a Theory

An Overview. *The Lawmakers: Recruitment and Adaptation to Legislative Life* is for the most part simply a more polished report of the research Barber initially presented in his dissertation, "The Legislator's First Session."[36] (Because each nevertheless contains information not reported in the other, I shall rely on both in this examination.) In studying the recruitment and adaptation of representatives, Barber was seeking answers to two, deceptively simple questions about a phenomenon that had long troubled democratic theorists interested in the quality of leadership in American state governments: Why don't legislators stay long enough to become effective? Why do incumbents often choose not to run again?

Barber assumed that most ceased serving simply because they had no desire to continue.[37] He further assumed that a legislator's expressed willingness to return would be a good indicator of whether the legislator would actually do so. He therefore set out to discover "the determinants of willingness to return."[38]

Barber collected three kinds of data. He administered pre- and postsession questionnaires, which in addition to a broad range of other information, elicited expressions of the legislators' immediate willingness to return for the next session and for the several succeeding sessions. Next, Barber divided those legislators who were willing to be interviewed into three groups, according to the strength of their commitment to return. From each of these three groups he randomly sampled ten candidates for tape-recorded "focused" interviews.[39] Finally, and substantially as a consequence of a growing enigma in his questionnaire and interview data, Barber collected information on each legislator's level of activity during the session.

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[33] "Assessing Presidential Character," p. 252, n. 23.

[34] *The Presidential Character,* p. 199.

[35] *Ibid.,* p. 172. There is also evidence in pre-*Presidential Character* papers that Barber considered the character construct to be axiomatic. Evidence may be found, for example, in Barber's explication of his typology, which precedes his discussion of the styles of Hoover and Coolidge, in "Classifying and Predicting Presidential Styles: Two Weak Presidents," *Journal of Social Issues,* 24 (1968), 51–80; reprinted in *The Presidency,* ed. Aaron Wildavsky (Boston: Little, Brown, 1969), pp. 94–120, especially p. 95.

[36] *The Lawmakers: Recruitment and Adaptation to Legislative Life* (New Haven: Yale University Press, 1965); "The Legislator's First Session" (Ph.D. dissertation, Yale University, 1960).

[37] "The Legislator's First Session," p. 37.

[38] *Ibid.*

The enigma was twofold. One tentative hypothesis—"that 'other-directed' types were more likely to volunteer to come back than 'inner-directed' types"—which Barber had derived from the responses to the first questionnaire, failed to receive support in the focused interviews.[40] Moreover, an expected change during the session in respondents' willingness to return did not occur, as far as Barber could see.[41] The information on legislators' levels of activity, however, only compounded the puzzle initially; for, contrary to Barber's expectation, a legislator's willingness to return and the degree to which he had been active during the session were unrelated.[42]

Faced with this puzzle, Barber made several decisions. First, perceiving a lack of change in legislators' willingness to return, he concluded that determinants of a legislator's willingness were to be found in more enduring and personal characteristics; he therefore decided to forego a study of "legislative working conditions."[43] Second, although Barber's tentative hypothesis of a direct relationship between willingness to return and legislative activity had not proved to be correct, the cross-tabulation of these two variables caused him to ponder why it might be that, for example, someone who was not active in the legislature would nevertheless be willing to return for several more sessions.[44] Barber had taken as a working assumption that a freshman who " 'preserves his sense of well-being while continuing to get along effectively in his environment' " would more likely be willing to return.[45] The data invited further investigation of the possibility that each configuration of these two variables classified legislators who received from political participation a type of reward different from the types received by others. Third, as a consequence of the absence of specific leads in the questionnaire data and of his decision to investigate these four configurations, Barber decided to concentrate on the interview data and check any plausible conclusions against the questionnaire responses.[46]

Pursuing his interest in the absence of a relationship between activity and willingness to return, Barber theorized as follows: If those who are willing to return are willing because legislative service meets some of their "continuing needs," then their degrees of activity may differentiate them according to the "forms of rewards" they derive.[47] Proceeding with this idea, he dichotomized the two measures, producing a table whose cells he ultimately labeled as in Table 2. Upon classifying his interviews accordingly, he discerned some interpretable patterns.

Table 2. Classification of Connecticut Legislators

Willingness to return	Activity	
	High	Low
High	Lawmakers	Spectators
Low	Advertisers	Reluctants

Source: James David Barber, *The Lawmakers* (New Haven: Yale University Press, 1965), p. 20.

Each type comprised legislators who not only behaved similarly in the legislature but also exhibited the same level of self-esteem and based their self-estimates on the same major criterion.[48] Consistent with these criteria were typical "strategies" for "enhancing and protecting a sense of self-approval."[49] Lawmakers, who esteemed themselves highly, did not behave compensatorily. Instead they were oriented to productiveness, to rational mastery. Legislators of the other three types, esteeming themselves less highly, employed compensatory strategies consistent with their respective criteria: the Advertiser, measuring himself against a power criterion, employed aggression; the Spectator, concerned with his loveableness, employed submission; and the Reluctant, judging himself on the basis of his usefulness, employed withdrawal.

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[39] *Ibid.*, p. 40. Barber employed "an adaptation of the 'focused interview' technique" presented by Robert K. Merton, Marjorie Fiske, and Patricia L. Kendall, *The Focused Interview* (Glencoe, Ill.: Free Press, 1956). Barber describes the technique in the following way:

> The interviewer begins by referring to some event in which the respondent has been or will be involved. The respondent is asked to describe his behavior in this event: what did he actually do? This is followed by questions on his reactions, choices, feelings, and thoughts about this particular event. The method requires the constant moving back and forth between these responses and the specification of the events to which they refer. *The Lawmakers*, p. 16.

[40] "The Legislator's First Session," p. 42.

[41] *Ibid.*, p. 43.

[42] *Ibid.*, p. 44.

[43] *Ibid.*, p. 45.

[44] *Ibid.*

[45] *Ibid.*, pp. 38–39.

[46] *Ibid.*, p. 45.

[47] *Ibid.*, pp. 45–46.

[48] *The Lawmakers*, p. 214.

[49] *Ibid.*

Barber construed the data from the questionnaires and the published sources as corroborating these patterns. He divided a selected portion of these data into four profiles, which he included in *The Lawmakers*. In his discussion of each type, however, he compared responses of legislators of the type with the responses of all other legislators combined. Therefore, to facilitate intertype comparison and to promote the association of items with dimensions, I have revised these profiles and have corrected erroneous entries.[50] (See Tables 3-6.)

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[50] The more complete report is appended to "The Legislator's First Session," pp. 267–337. In that report Barber breaks down the legislators into their four classifications on every item. And he reports the complete distributions of many items that he simply dichotomizes in *The Lawmakers*.

Table 3. Legislative Activity Profile

	Spectator (n=30)	Reluctant (n=17)	Advertiser (n=16)	Lawmaker (n=33)
Originated action to get nomination	17%	29%	56%	61%
Attended many meetings in campaign	23	35	38	82
Likes campaigning very much[a]	33	58	8	55
Attributes election to own campaign efforts	7	12	19	15
Often introduced self to others at first of session	70	76	94	91
Took an active part in major negotiations	47	47	75	82
Was frequently sought for advice	33	24	69	58
Achieved leadership or important committee post[a,c]	*11*[b]	0	54	48
Considers self energetic rather than easy-going	50	59	75	*67*
Considers self more influential than others	50	35	69	55
Self-rating as a legislator: superior or excellent	20	24	*56*	42

[a] Indicates that percentages for the item are based on the number of respondents who answered both pre- and postsession questionnaires. Spectator n=27, Reluctant n=12, Advertiser n=13, Lawmaker n=31. All other percentages are based on postsession questionnaire respondents.

[b] Because of corrections for rounding errors, the percentages that are italicized differ from those reported by Barber in his tables in *The Lawmakers*.

[c] In *The Lawmakers*, pp. 25, 69, 119, 166, this item is incorrectly represented as having been computed on postsession questionnaire respondents only. Cf. Barber, "The Legislator's First Session," p. 323, Table E1.

Table 4. Political Ambitions Profile

	Spectator (n=30)	Reluctant (n=17)	Advertiser (n=16)	Lawmaker (n=33)
Identifies self as politician	33%	35%	44%	55%
Has considered seeking full-time elective office:				
Presession response[a]	26	*42*[b]	31	*65*
Postsession response[a]	30	17	*38*	61
Had considered full-time state appointive office[a]	*22*	33	*23*	42
Was interested in district or state party office[a]	63	58[c]	85	*81*
Was interested in assembly leadership position[a]	52	*42*	*77*	*94*
Willingness to return for three or more future sessions:				
Presession response[a]	*85*	*42*	*38*	81
Postsession response[a]	100	0	0	100
Ran again for nomination or election to assembly:				
In 1960	83	*76*	69	85
In 1962	57	41	31	39
Ran for or served in some government office (including Assembly) after initial session	90	94	81	94

[a] Indicates that percentages for the item are based on the number of respondents who answered both pre- and postsession questionnaires. Spectator n=27, Reluctant n=12, Advertiser n=13, Lawmaker n=31. All other percentages are based on postsession questionnaire respondents.

[b] Because of corrections for rounding errors, the percentages that are italicized differ from those reported by Barber in his tables in *The Lawmakers*.

[c] The percentage reported in *The Lawmakers*, p. 120, apparently contains a typographical error. Cf. "The Legislator's First Session," p. 333, Table F2.

Table 5. Political Background Profile

	Spectator (n=30)	Reluctant (n=17)	Advertiser (n=16)	Lawmaker (n=33)
Parents interested in politics	*53%*[b]	76%	*62%*	73%
Relatives active in politics	57	*76*	50	70
Occupation involves government contracts	30	41	75	*61*
Had held party office[a]	56	50	54	45
Had held elective office[a]	37	67	38	45
Had held appointive office[a]	37	33	62	32
Had long considered running for assembly	*47*	59	56[c]	*64*
Reports some competition for nomination[a]	56	75	*85*	*81*
Saw election chances as 50-50[a]	22	0	46	26
Considers legislative activity most important he has engaged in	80	53	38	48

[a] Indicates that percentages for the item are based on the number of respondents who answered both pre- and postsession questionnaires. Spectator n=27, Reluctant n=12, Advertiser n=13, Lawmaker n=31. All other percentages are based on postsession questionnaire respondents.

[b] Because of corrections for rounding errors, the percentages that are italicized differ from those reported by Barber in his tables in *The Lawmakers.*

[c] The percentage reported in *The Lawmakers,* p. 71, apparently contains a typographical error. Cf. "The Legislator's First Session," p. 284, Table B12.

Table 6. Personal Data Profile

	Spectator (n=30)	Reluctant (n=17)	Advertiser (n=16)	Lawmaker (n=33)
Sex: Male	60%	82%	100%	82%
Age: Over 40	67	59	25	27
Occupation:				
Housewife	30	6	0	15
Attorney	0	12	50	30
Farmer	10	6	6	3
Retired	13	24	0	9
Business	20	35	13	12
Education: Above high school[a]	*33*[b]	50	62	*74*
Income: Over $8,000	33	*25*	*62*	48
Town population: Over 5,000	40	24	*81*	*91*
Resident in town: More than ten years	80	*82*	44	*67*
Expects to remain in town	100	76	94	88
Expects more income in ten years	33	53	75	*79*

[a] Indicates that percentages for the item are based on the number of respondents who answered both pre- and postsession questionnaires. Spectator n=27, Reluctant n=12, Advertiser n=13, Lawmaker n=31. All other percentages are based on postsession questionnaire respondents.

[b] Because of corrections for rounding errors, the percentages that are italicized differ from those reported by Barber in his tables in *The Lawmakers.*

About the relative effectiveness of the four types, Barber ultimately decided that although each type seemed to contribute in some way to the successful functioning of the legislature, Lawmakers were the only type whose needs as individuals were highly compatible with the essentially task-functional nature of the institution. The personal needs of the other three types were to Barber inherently in conflict with legislative demands.[51]

Having begun, then, with a search for the determinants of legislative turnover, Barber in effect ended with a theory of the ideal legislator. Accordingly, he attempted, in conclusion, to ascertain how to assure the recruitment and retention of Lawmakers.

A Critique of Barber's Methodology. From legislators' activity during the session and from personality terms such as ascendance, dominance, initiative, and achievement, Barber derived the construct "activity." From legislators' willingness to return to future sessions and from a consonant concept in social psychology, he derived a second construct, "attitude toward the group."[52] And from interview responses he inferred two other constructs, "strategy of adaptation" and "self-esteem." Each of these is one of the four "subconstructs" that make up the prototype of his character construct (schematized in Figure 1).

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[51] In *The Lawmakers,* pp. 247–50, Barber discusses the contributions of each type. He concludes that the contributions of legislators who are not Lawmakers are "partial and minor" in comparison with the disfunctional effects of these legislators' presence. (See especially p. 250.) His conclusion is even more baldly stated in "The Legislator's First Session," pp. 241–42.

For purposes of analysis, the subconstructs can be placed into two groups (activity with attitude and strategy of adaptation with self-esteem) on the basis of four characteristics: the type of variable; the level of trait; the manner of definition; and whether measured on a sample or on a population. First, the joint configuration of activity and attitude is Barber's dependent variable. Self-esteem and strategy of adaptation, on the other hand, are his paramount explanatory variables. Second, activity and attitude compose the phenomenology of the prototypal construct; self-esteem and strategy of adaptation compose the dynamics. Third, activity and attitude are defined operationally; strategy of adaptation and self-esteem, however, are simply assessed in interviews. Fourth, every legislator in the population is "measured" on activity and attitude, whereas only interviewees are "measured" on self-esteem and strategy of adaptation.

To establish the empirical basis of the prototypal character construct, at least three questions about these subconstructs must be answered affirmatively: (1) Are Barber's measures reliable and valid? (2) Do the data support Barber's inferences? (3) Can Barber generalize from his samples?

The Reliability and the Validity of Barber's Measures. Establishing the reliability and the validity of measures is at times an onerous responsibility. It can be left unfulfilled, however, only at the expense of the empirical bases to which any conclusion could be ascribed. It is therefore remarkable that Barber failed even to discuss the validity and reliability of his measures. He attempted to tie "activity" to constructs in other personality studies; and he derived from social psychology the nature of a second construct—"attitude toward the group"—which he had partially inferred from his data. But by pointing out, in effect, that others had used similar labels, he neither assured the similarity of what was labeled nor said anything about the reliability and the validity of the measures.

What exactly were these measures?

"Activity" was a composite constructed by summing a legislator's scores on four variables:

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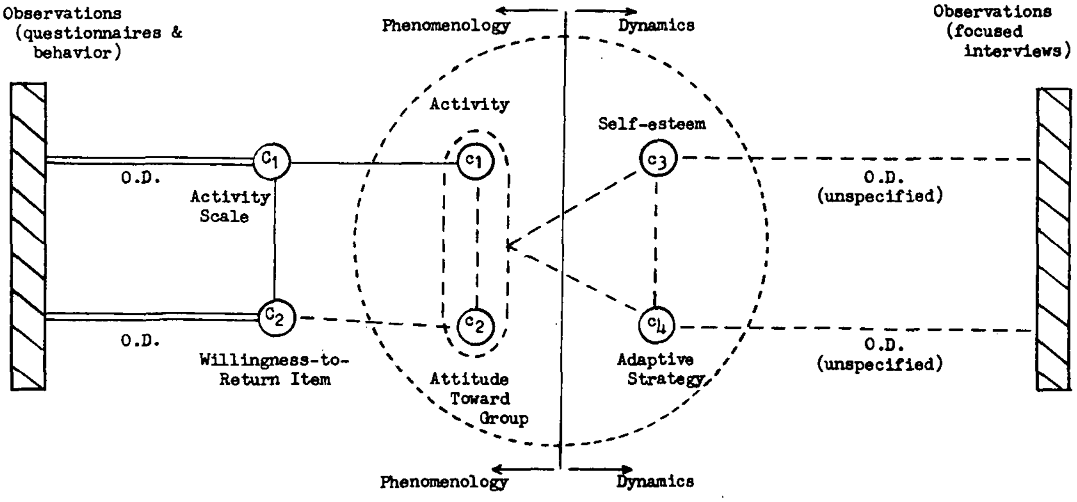
[52] *The Lawmakers,* pp. 18–19.



O.D.: operational definition
C_i: construct defined operationally
c_i: construct defined constitutively
Double solid lines: operational definitions
Single solid lines: postulated relationships that receive empirical support
Broken single lines: postulated (or implied) relationships that are not supported empirically
Note: A further statement concerning the lines linking the constructs is on p. 206.

Figure 1. Barber's Prototypal Character Construct

the number of bills the legislator introduced, the number of comments he made in his own committee, the number he made in other committees, and the number of transcript lines he spoke from the floor. Legislators who scored overall relatively high Barber classified as "actives"; those who scored relatively low he classified as "passives."[53]

A legislator's attitude, on the other hand, was measured by the legislator's willingness to return to three future sessions. As Barber interpreted it:

> This expression of willingness to return taps, I think, the general attitude the individual has toward his membership in the group. It probably should be taken less as an expression of practical intention than as a current orientation toward the office. Those who give positive answers to this question appear to be saying (and here I judge in part from the interview replies) that on balance, considering all the pros and cons, they find the office attractive, satisfying, an experience they might very well want to repeat several times.[54]

The more difficult question, of course, is, How reliable and valid were these measures?[55] Unfortunately, because of limitations in Barber's methodology and because his data cannot be completely reconstructed, calculation of reliability coefficients is impossible, with one exception. Examining those features of each measure that affected its reliability and those that affected its validity should, however, produce an accurate answer.

Activity. Barber's activity measure appears, even in a cursory examination, to be more reliable and valid than the attitude measure. Nevertheless, the activity measure exhibits a striking weakness: it lacks stability.

Stability is the invariance of an individual's scores, or classifications, over time. Because individuals may actually change, however, variance must not be indiscriminately attributed to instability, or measurement error. In distinguishing true change from instability one can be partially guided by the construct being measured.[56]

The activity subconstruct implies stability, as page after page of *The Lawmakers* documents and as Barber's subsequent treatment of the subconstruct attests. But the activity measure Barber employed is so specific to the 1959 session that it guarantees that legislators' classifications are unstable. To begin with, the measure is tied to the overall size of the 1959 legislative program, as Table 7 makes clear. Moreover, since the major bills in a program determine which committee will be more active, the measure is also dependent on the nature of the 1959 program, simply as a consequence of the component variable "comments in own committee."

The implications of these two features of the measure become apparent if we imagine an application of the measure to these same legislators serving in some other session. In a less active session with a record built on, say, agricultural problems and land use control—rather than on reorganization of the courts,

Table 7. Scoring on Variables Composing Barber's Activity Index[a]

	Variables			
Score[b]	Bills Introduced	Comments in Own Committee	Comments in Other Committees	Lines Spoken from Floor
0	0	0	0	0
1	1-5	1-10	1-5	1-20
2	6-10	11-40	6-10	21-100
3	11-15	41-80	11-20	101-250
4	16 or more	81 or more	21 or more	251 or more

[a]This table is constructed from tables in Barber, *The Lawmakers*, pp. 272–73.

[b]A legislator received a score on each of the four variables. These scores were summed to give an overall activity score for that legislator.

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[53] *The Lawmakers*, pp. 272–73.

[54] *Ibid.*, p. 19. The reader will note that this interpretation differs from the one Barber initially employed; see above, p. 189, and "The Legislator's First Session," p. 37, for Barber's original interpretation of the item; and see the section on Attitude, below, for a discussion of the difference.

[55] Explanations of "reliability" can be found in Jum C. Nunnally, Jr., *Introduction to Psychological Measurement* (New York: McGraw-Hill, 1970), p. 108; and in Richard I. Lanyon and Leonard D. Goodstein, *Personality Assessment* (New York: John Wiley & Sons, 1971), p. 120. Explanations of "validity" can be found in Fred N. Kerlinger, *Foundations of Behavioral Research* (New York: Holt, Rinehart and Winston, 1964), pp.444–62;and in Claire Selltiz et al., *Research Methods in Social Relations*, rev. 1 vol. ed. (New York: Holt, Rinehart, and Winston, 1959), pp. 154–66.

[56] On distinguishing true change from instability see Jum C. Nunnally, *Psychometric Theory* (New York: McGraw-Hill, 1967), pp. 216–17.

enactment of a uniform commercial code and authorization of major highway construction (the basis of the record in the 1959 session)[57]—the measure would probably place Barber's legislators in other than their 1959 categories.[58]

In addition, the measure may even be specific to the legislators who made up the freshman class. Barber does not disclose his cutting point for distinguishing "actives" from "passives." If he used the median aggregate score, however—which seems the likely criterion—then a freshman's classification was also dependent on the "mix" of his colleagues.[59]

In sum, a legislator's classification on this dimension, rather than simply reflecting a social trait, reflected as well the nature of the bills considered during the session, the extent of the legislative package, and ultimately even the electoral good fortunes of the legislator's fellow freshmen.

In correlating significantly with other indicants of legislative activity (see Table 8), the measure does manifest a characteristic that is necessary for a valid measure. Nevertheless, these associations also evoke the question of why Barber selected out the variables he did as the components of his measure of "activity." By taking these as his measure, he a priori inflated their importance. Whether legislators' activity scores would change if the other eleven activity variables were taken into account is an empirical question that should be answered before "activity" is considered to have been validly defined.

Attitude. Barber's single-item measure of the subconstruct "attitude toward membership in the group" is egregiously unreliable. This is easily shown, for Barber gives both the presession and the postsession responses to the item. Depending on the source from which the data are taken, the result of computing the phi coefficient is an estimated reliability of either .49 or .43 (see Table 9). Since it has been suggested that measures whose coefficients are below .80 are suspect,[60] what must we conclude about a measure whose coefficient is only a little over half that large? The answer is not

Table 8. Legislative Activity Profile

	Dimensions			
	Attitude (positive-negative)		Activity (active-passive)	
	Tau[a]	Fisher's Exact[b]	Tau	Fisher's Exact
Originated action to get nomination	.001		.149	$\underline{**}$
Attended many meetings in campaign	.028	$\underline{*}$	.158	$\underline{**}$
Likes campaigning very much[c]	.014	*	.000	
Attributes election to own campaign efforts	.003		.014	*
Often introduced self to others at first of session	.002		.065	$\underline{**}$
Took an active part in major negotiations	.002		.116	$\underline{**}$
Was frequently sought for advice	.000		.099	$\underline{**}$
Achieved leadership or important committee post[c]	.001		.211	$\underline{**}$
Considers self energetic rather than easy-going	.006		.028	$\underline{*}$
Considers self more influential than others	.000		.021	*
Self-rating as a legislator: superior or excellent	.006		.073	$\underline{**}$

*$p \leqslant .20$, direction predicted; $\underline{*}p \leqslant .20$, direction not predicted; **$p \leqslant .05$, direction predicted; $\underline{**}p \leqslant .05$, direction not predicted.

[a] Goodman and Kruskal's Tau is easily interpreted in the 2x2 case; it is equal to phi^2 and r^2.

[b] Ample justification for using tests of significance to distinguish potentially important from probably unimportant differences in a population has been supplied by Hubert M. Blalock, *Social Statistics* (New York: McGraw-Hill, 1960), p. 270.

[c] Computed on respondents answering both questionnaires, n=83. All others computed on respondents answering postsession questionnaire, n=96.

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[57] *The Lawmakers*, p. 163.

[58] Eighteen of the 20 attorneys who answered Barber's postsession questionnaire were "active" in the legislature (8 were Advertisers; 10 were Lawmakers). "The Legislator's First Session," p. 276, Table A3. In comparison with other legislators, attorneys were no doubt much more "at home" reorganizing the state's entire minor court system and enacting a uniform commercial code.

[59] Though Barber does not report the distribution of legislators on the activity scores, he does note that his dividing line distinguished 79 "passive" legislators, whose scores ranged from 0 through 7, from 71 "active" legislators, whose scores ranged from 8 through 16. This asymmetry does not necessarily preclude the median as the cutting point. The distribution could have arisen because Barber categorized as "passives" those legislators with the median activity scores.

[60] Nunnally, *Introduction to Psychological Measurement*, p. 127.

Table 9. Reliability of Barber's Attitude Measure

	Postsession		
Presession	Willing to return	Unwilling to return	Total
Willing to return	48	10	58
Unwilling to return	7	15	22
Total	55	25	80[a]
phi = .491			

[a]This table is constructed from "The Legislator's First Session," p. 336, Table F6; here n=80 rather than 83 because one Spectator and two Lawmakers did not answer the willingness-to-return item on the presession questionnaire. If constructed from *The Lawmakers*, pp. 26, 70, 120, and 167, n=83 and phi= .428, because Barber fails to take these three legislators into account, in effect including them with legislators whose answers had changed.

hidden in the statistic; it is readily apparent in the fact that 21 per cent of those who had answered the item on the first questionnaire changed their minds by the time they answered the second questionnaire.

Expecting stability in a legislator's professed willingness to return may seem highly unrealistic to the reader. After all, with a session of experience to draw on, some legislators, upon rethinking their earlier commitments, are bound to arrive at different decisions. Therefore, changes in these legislators' responses are not necessarily due to "unreliability" in the measure.[61]

Unconsidered in this argument, though, is the relationship between construct and measure. The two must be congruent. A *stable attitude* measured by an instrument that indicates *substantial change* is a contradiction. But that is precisely what Barber offers. He posits a stable attitude and relies on a measure that shows that attitude to be unstable. Why? A brief reexamination of the course of Barber's research supplies the answer.

Barber predicated the attitude subconstruct on what he perceived to be an absence of change during the session in legislators' willingness to return. Proceeding circularly, he then took this single item as his measure of the subconstruct. This would not necessarily have proved pernicious if Barber's conclusion that there was no change had been correct. Unfortunately, though, Barber reached that conclusion by committing a fundamental error. In interpreting his cross-tabulation of respondents' pre- and postsession willingness to return, he took into account only the marginals of the cross-tabulation; that is, he failed to take into account offsetting changes, which were not reflected in differences in pre- and postsession totals. In "The Legislator's First Session" he writes:

> Before the session, for example, twenty-seven per cent indicated that they "probably" or "definitely" would not be willing to return for three or more future terms. Some seven months later, thirty per cent of this group gave these answers. Contrary to what was expected, the experience of the legislator's first session appeared to have little or no effect on his willingness to return.[62]

As Table 9 makes clear, rather than the three legislators Barber perceived, there were seventeen who changed their minds.

To reiterate: Central in Barber's research were the initial query, "What were the determinants of willingness to return?"[63] and the related question, What does "willingness to return" indicate? Because Barber perceived no change in legislators' willingness, he rejected "actual legislative experience" as a possible answer to the first. For the same reason he rejected as an answer to the second the conclusion that a legislator's willingness to return is an expression of the legislator's practical intentions.[64] Instead Barber decided to seek determinants in a "more personal, older and less superficial set of characteristics" and to interpret a legislator's willingness to return as an expression of the individual's attitude toward the group.

But since substantial change actually did take place during the session, each of Barber's interdependent decisions is without at least this particular basis. A legislator's experience in the session obviously has more possibility as a determinant than Barber recognized. Accordingly, there is less reason for seeking determinants solely in "more personal" characteristics. And without the constraint of having to seek this kind of determinant, one can more readily regard a legislator's professed willingness as an "expression of practical intention." This interpretation is even supported in Barber's data. Table 10 shows that if one predicts positive correlations between the willingness-

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[61]On the durability of attitudes and traits, see Calvin S. Hall and Gardner Lindzey, *Theories of Personality*, 2d ed. (New York: John Wiley and Sons, 1970), pp. 263–68; M. Brewster Smith, Jerome S. Bruner and Robert W. White, *Opinions and Personality* (New York: John Wiley and Sons, 1956), pp. 7–47; David Krech, Richard S. Crutchfield, and Egerton L. Ballachey, *Individual in Society* (New York: McGraw-Hill Book Co., 1962), pp. 103–34 and 215–69.

[62]"The Legislator's First Session," p. 43.

[63]*Ibid.*, p. 37.

[64]*The Lawmakers*, p. 19.

Table 10. Political Ambitions Profile

	Dimensions			
	Attitude (positive-negative)		Activity (active-passive)	
	Tau	Fisher's Exact	Tau	Fisher's Exact
Identifies self as politician	.002		.029	$\underline{*}$
Has considered seeking full-time elective office:				
Presession response[a]	.010		.057	$\underline{**}$
Postsession response[a]	.029	$\underline{*}$	.086	$\underline{**}$
Had considered full-time state appointive office[a]	.002		.013	
Was interested in district or state party office[a]	.000		.051	$\underline{**}$
Was interested in assembly leadership position[a]	.020	*	.189	$\underline{**}$
Willingness to return for three or more future sessions:				
Presession response[a]	.183	$\underline{**}$	.002	
Postsession response[a]	1.000	$\underline{**}$	.001	
Ran again for nomination or election to assembly:				
In 1960	.018	*	.000	
In 1962	.012	*	.021	*
Ran for or served in some government office (including Assembly) after initial session	.005		.001	

*$p \leqslant .20$, direction predicted; $\underline{*}p \leqslant .20$, direction not predicted; **$p \leqslant .05$, direction predicted; $\underline{**}p \leqslant .05$, direction not predicted.

[a]Computed on those respondents who answered both pre- and postsession questionnaires, n=83. All others computed on respondents answering postsession questionnaire, n=96.

Table 11. Political Background Profile

	Dimensions			
	Attitude (positive-negative)		Activity (active-passive)	
	Tau	Fisher's Exact	Tau	Fisher's Exact
Parents interested in politics	.004		.007	
Relatives active in politics	.000		.000	
Occupation involves government contracts	.012	*	.098	$\underline{**}$
Had held party office[a]	.000		.004	
Had held elective office[a]	.010		.001	
Had held appointive office[a]	.016	*	.003	
Had long considered running for assembly	.000		.010	
Reports some competition for nomination[a]	.013		.051	**
Saw election chances as 50–50[a]	.000		.037	$\underline{*}$
Considers legislative activity most important he has engaged in	.030	$\underline{*}$	.065	$\underline{**}$

*$p \leqslant .20$, direction predicted; $\underline{*}p \leqslant .20$, direction not predicted; **$p \leqslant .05$, direction predicted; $\underline{**}p \leqslant .05$, direction not predicted.

[a]Computed on those respondents who answered both pre- and postsession questionnaires, n=83. All others computed on respondents answering postsession questionnaire, n=96.

to-return item and items registering whether the legislator ran again in 1960 and in 1962 (predictions that are consistent with the wording of the measure), the correlations between the items and the measure are significant, Barber's contrary interpretation notwithstanding.[65]

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[65] In an interview by Jerry Tallmer, "A Presidential Primer," *New York Post,* 1 September 1972, Barber states:

> I was writing my first book, *The Lawmakers,* about state legislators, and was puzzled by the fact that so many people fight for public office and then drop out. I figured those who most enjoy it wouldn't drop out. . . . I was going over and over the indices of who stayed at the job and who enjoyed it—and I held up the two codes and the correlation was zero, absolute zero. . . . When that happens you either commit suicide or you try to figure out the answer. And the basic answer was that you can get people who are as active as hell in politics and suffering under it.

Table 12. Personal Data Profile

	Dimensions			
	Attitude (positive-negative)		Activity (active-passive)	
	Tau	Fisher's Exact	Tau	Fisher's Exact
Sex: Male	.050	$\underline{**}$	.057	$\underline{**}$
Age: Over 40	.001		.141	$\underline{**}$
Occupation:				
Housewife	.063	$\underline{**}$	.023	*
Attorney	.028	$\underline{*}$	.160	$\underline{**}$
Farmer	.000		.008	
Retired	.000		.029	$\underline{*}$
Business	.010		.029	$\underline{*}$
Education: Above high school[a]	.000		.103	$\underline{**}$
Income: Over $8,000[a]	.001		.047	**
Town population: Over 5,000	:022	*	.304	$\underline{**}$
Resident in town: More than ten years	.009		.056	$\underline{**}$
Expects to remain in town	.021	*	.001	
Expects more income in ten years	.004		.143	$\underline{**}$

*p⩽.20, direction predicted; $\underline{*}$p⩽.20, direction not predicted; **p⩽.05, direction predicted; $\underline{**}$p⩽.05, direction not predicted.

[a]Computed on those respondents who answered both pre- and postsession questionnaires, n=83. All others computed on respondents answering postsession questionnaire, n=96.

All of this is not to deny that Barber's measure may tap "attitude toward membership in the group" and that personal characteristics can influence a legislator's willingness to return. But the former is only one of a number of factors the measure may tap, and the latter is only one of a number of determinants of willingness to return.

Self-esteem and adaptive strategy. Because Barber alone assessed the interviewees' levels of self-esteem and adaptive strategies, one can arrive only indirectly at judgments about the reliability and the validity of the focused interviews he used. In general, then, how reliable and valid are focused interviews when used for purposes similar to Barber's? The best answer seems to be "not very."[66] The focused interview is among diagnostic procedures that generally result in judgments of low consistency among clinicians. Differences in interviewers' styles, in the wording of questions, and in the type of probing are obvious sources of error in this technique. Moreover, interviewers' diagnostic skills certainly affect their inferences from interviews. We may expect, therefore, that Barber's diagnoses would not be duplicated by another interviewer. Accordingly, since focused interviews cannot be valid if they are not reliable, I believe we must also view with healthy skepticism Barber's conclusions about the self-esteem and accentuated adaptive strategies of his interviewees.

The Data Configuration. Even if the measures of the subconstructs were undoubtedly reliable and valid, Barber's data would not necessarily support the character construct. Therefore, the configuration of the interviews should be ascertained and the value of the questionnaire responses as corroborative evidence considered.

The interview data. Barber's inferences are quite complex. Barber argues that each interviewee employs several adaptive strategies but accentuates one, that accentuated strategies are associated with levels of self-esteem, and that for each of the types a different accentuated strategy is modal. As a consequence, supporting these inferences with data is difficult, and not only because a larger, more evenly distributed sample than the one Barber relied on is required (see Table 13). In effect, beginning with inter-

Table 13. Distribution of Interviewees by Types

Attitude toward legislature	Activity	
	Active	Passive
Positive	10 (Lawmakers)	3 (Spectators)
Negative	6 (Advertisers)	6 (Reluctants)

Derived from Barber, "The Legislator's First Session," pp. 59, 103, and 171; and *The Lawmakers,* p. 121.

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[66] Lanyon and Goodstein, *Personality Assessment,* pp. 107–10, and 176; Nunnally, *Psychometric Theory,* p. 489.

view responses, Barber must identify adaptive strategies, establish which strategy each interviewee accentuates, independently ascertain interviewees' levels of self-esteem, correlate levels of self-esteem with accentuated strategies, and, finally, determine each type's modal accentuated strategy.

Fundamental to these inferences are responses that unequivocally evince particular strategies. Therefore, adducing such responses is the first step; it is one that Barber is rarely able to accomplish. Careful inspection and comparison of responses across types suggests that the responses of many of the interviewees are quite ambiguous.

In some cases, having discovered an interviewee whose responses exemplified a particular adaptive strategy, Barber seems to have begun discerning the same strategy in other interviewees' more ambiguous responses. In a striking example, Barber lengthily quotes an Advertiser who communicates in a most aggressive vernacular.[67] But the responses that Barber offers to establish the aggressiveness of other Advertisers are equivocal. Just how equivocal becomes evident when these responses are compared with those he relies on to show the "activity" of Lawmakers. One Lawmaker shows merely his activity in his "demand that others 'wake up and see' "; and another evinces only his activity in his statement about an " 'apathetic' town 'revitalized.' " Likewise, Lawmakers manifest simply their activity in their "irritation" with "the 'do-nothings,' those who 'follow and follow and follow,' the 'seat-warmers,' those who 'want to be ruled,' . . . the 'rubber-stamp legislator,' the 'status quo guy,' 'the man that can't think for himself,' those in whom 'there's no rebellion.' "[68] But an Advertiser is manifesting aggression by being exasperated with his legislative colleagues who are lawyers—whom he sees as simply " 'speaking for a little publicity' "; by being annoyed that though he and some fellow members " 'have questions they want to raise' " in a committee, other members only want him to " 'follow the lead' "; or by relishing the discomfiture of the opposing leadership after the passage of several items.[69]

Dissimilar treatment of patently similar responses is also found in Barber's efforts to establish ancillary differences, even though there is no immediately evident exemplar to whom one might attribute a biasing influence. I have mixed together Spectators' expressions of their pleasure at being in the legislature[70] (pleasure that Barber describes as "superficial and temporary" and "vague and clichéd")[71] and responses that, he argues, show that Lawmakers have positive feelings for the legislature (feelings in which the "enthusiasm rings true").[72] The reader may wish to try to distinguish which type of interviewees offered which responses: (1) " 'Well, so far I love it.' " (2) " 'Personally, it's been tremendously enjoyable.' " (3) " 'I've enjoyed every minute of it.' " (4) " 'Very educational and enjoyable.' " (5) " 'I have enjoyed it.' " (6) " 'I would say that I would like to come again and again to the legislature.' " (7) " 'It's been a wonderful experience so far.' " (8) " 'I'm quite fascinated with it.' " (9) " 'I've found that it's tremendously interesting.' " Spectators offered responses numbered three, seven and nine; Lawmakers offered the others. We can only wonder why in Chapter IV " 'wonderful' " and " 'interesting' " and cognates of "enjoy" are clichéd and vague while in Chapter VII " 'wonderful' " and " 'fascinating' " and cognates of "enjoy" ring with enthusiasm.

And the reader may also wish to try to distinguish between the following responses: (1) " 'Let me say this: I haven't done anything that I would be ashamed of, as far as I, subjectively, am concerned.' "[73] (2) " 'Yeah, I think I've done all right. I . . . I'm a man with . . . my parents endowed me with a very strong conscience and when I do something wrong it bothers me for a long time. And I don't, I haven't done anything in the legislature yet that I really regret, that I think has been basically wrong.' "[74] The latter response—by an Advertiser—Barber sees as evincing guilt, which he suggests is attributable to the social unacceptability of aggression[75] (although he never demonstrates that this particular interviewee manifests aggression). But the former response—by a Lawmaker—Barber interprets as showing that the interviewee is "aware of himself as a unique person, with his own standards" and aware of "the relevance of his own standards for his behavior."[76]

The preceding paragraph introduces a serious inadequacy in Barber's presentation of the responses. It is an inadequacy that not only makes the responses more ambiguous but also, insofar as it reflects the manner in which Barber analyzed the data, vitiates his conclusions. Each quotation is a disembodied statement, the response of a legislator about whom Barber does not offer social, economic, political or other personal information. Moreover, except for several sections where Barber quotes more extensively, he does not even indicate which statements were made by the same interviewee. But the assessment of a legislator's personal needs, of course, demands the profile of that individual, not a congeries of disembodied responses that includes some by fellow legislators.[77] For example, assessments of the Advertiser whose response Barber interprets as manifesting guilt would surely differ, depending on whether the interviewee was the Advertiser whom I earlier dubbed the "exemplar" of the type or the exemplar's antithesis. From Barber's presentation and analysis, however, one can only infer that his conclusions are arrived at by ascribing the characteristics of any individual member of a type to the type itself and, in turn, any of those of the type to any of its members.

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[67] "The Legislator's First Session," pp. 108–10.

[68] *Ibid.*, p. 176.

[69] *Ibid.*, pp. 110–11.

[70] *Ibid.*, pp. 59–60.

[71] *Ibid.*, p. 78.

[72] *Ibid.*, pp. 187–88.

[73] *Ibid.*, p. 189.

[74] *Ibid.*, p. 115.

[75] *Ibid.*, pp. 112–14.

[76] *Ibid.*, p. 189.

The second step in Barber's reduction of responses to modal strategies is establishing which strategy each interviewee accentuates. Barber supposedly accomplishes this step simply by quoting responses he regards as manifestations of the strategy.[78] But "accentuated" is a comparative; it can only be empirically established in the contexts from which Barber completely removes the responses. At best, an interviewee's response evidences that the interviewee employs a particular adaptive strategy, not that he accentuates it.

Ascertaining interviewees' levels of self-esteem is the third step. As is the case, however, with responses that evince adaptive strategies, responses that univocally support Barber's conclusions about interviewees' self-esteem are rare. Indeed, of the Spectators, Advertisers, and Reluctants whom Barber interviewed, only one—a Spectator whom Barber identified as "Sam Thompson" in *The Lawmakers*—offered responses that are fairly convincing evidence of lack of self-esteem.[79]

This ambiguity is not without effect on Barber's analysis, as the metamorphosis in his interpretation of the Reluctants' motives shows. In "The Legislator's First Session" Barber attributes the Reluctant's legislative service not to an attempt to compensate for low self-esteem but rather to "old-fashioned" character,[80] to a desire "to be of service to the community"[81] in which the Reluctant is "so well adjusted."[82] In *The Lawmakers,* however, although Barber still attaches importance to "old-fashioned" character,[83] he introduces and emphasizes the enhancement of self-esteem as the Reluctant's motivation to service.[84]

The fourth step in the reduction is that of convincingly associating accentuated strategies with levels of self-esteem. The absence of persuasive evidence of the strategies that interviewees accentuated and of the degrees to which interviewees esteemed themselves makes this fourth step difficult, of course, and causes at least one tumble into anomaly. Subsequent to "The Legislator's First Session," Barber discusses only four of the eight logically possible combinations of adaptive strategies and levels of self-esteem. Thus, to the reader familiar with only Barber's later work, it is no small surprise that in "The Legislator's First Session," Barber links achievement and low self-esteem, producing a fifth combination.[85] But Barber, it seems, is at least as incredulous at the association. For in the course of several pages in "The Legislator's First Session," he lays groundwork for the conviction that achievement that is compensatory is not achievement at all. In analyzing the Advertisers he writes:

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[77] In the analysis in *The Lawmakers* Barber does connect the responses of the pseudonymous interviewees he uses as examples. For several reasons, however, this analysis does not give the reader any clearer sense of the characteristics of the interviewees: (1) Barber discusses only three interviewees of each type—a total of only half the number he interviewed. (2) His "examples" are not *average* interviewees but rather *exemplars* of the inferred patterns. (3) In analyzing the Advertisers Barber does not even treat the examples equally. "Mike Jackson"—the tough-talking, Advertiser exemplar—dominates Chapter 3 of *The Lawmakers.* Barber cites him more frequently than he cites both the other Advertisers. Thus for the 16 Advertisers in Barber's study the supposed sample of six interviewees becomes little more than a sample of one.

[78] In "The Legislator's First Session" Barber does not even attempt to establish the accentuated strategy of each of his interviewees; rather from the responses he adduces he merely arrives at a judgment of the modal strategy of a type through some unspecified inferential process. In *The Lawmakers* Barber initially appears to make more of an effort to establish the accentuated strategies of each of his examples. Closer examination reveals, however, that he is actually considering alternative *modal* strategies for each *type.* See *The Lawmakers,* pp. 43–54 (Spectators), 90–104 (Advertisers), 145–54 (Reluctants), and 191–205 (Lawmakers). Moreover, in *The Lawmakers* the effort has the cast of a justification for conclusions already arrived at.

[79] "The Legislator's First Session," pp. 66–68.

[80] *Ibid.*, p. 132.

[81] *Ibid.*, p. 141.

[82] *Ibid.*, p. 227.

[83] *The Lawmakers,* p. 117; see also p. 140.

[84] *Ibid.*, pp. 128, 228–29, and 232.

[85] "The Legislator's First Session," p. 119–20.

> These interests in making legislation and knowing how the legislature works may give the Advertiser a feeling of accomplishment. But a closer look at the quotations above and at some of the other questionnaire answers appears to show that the Advertiser tends to be interested in legislation mainly as a *means* by which he can gain a sense of achievement and unload some of his hostility in the process, rather than as an end in itself. Undoubtedly Advertisers contribute to the substantive work. But it is *his* participation more than the legislation itself which seems to be important.[86]

Of course, gaining a sense of achievement is the essence of employing an achievement strategy compensatorily. Rejecting that interpretation, Barber is subsequently free to arrive at the relatively neater conclusion ("neater" in the sense that he has one strategy wholly related to high self-esteem) that Advertisers employ an aggressive—but subtly aggressive—adaptive strategy. And we never again read empirical evidence of compensatory achievement.[87]

Determining the modal accentuated strategy of each type is the final step in the reduction. Barber begins the step in "The Legislator's First Session" and completes it in *The Lawmakers.* But since the step depends especially on the identification of the strategy each interviewee accentuated, and since Barber is not really able to establish these accentuations, it is probable that Barber's association of modal strategies with types is arbitrary.[88]

A comparison of Barber's discussion of strategies in "The Legislator's First Session" with his discussion in *The Lawmakers* produces two solid pieces of evidence that the association is indeed arbitrary. First, Barber concludes in *The Lawmakers* that Spectators are submissive.[89] But in "The Legislator's First Session," four of the six specific strategies that he lists as having been employed by Spectators fall within the scope of the more general strategy he later describes as "withdrawal." Moreover, only two of the no-fewer-than-sixteen different responses with which he supports these six specific strategies are offered as evidence of Spectators' "submission."[90] Second, Barber argues in *The*

	Sociability (Low)	Sociability (High)
Dominance (High)	Analyzes Criticizes Disapproves Judges Resists	Advises Coordinates Directs Leads Initiates
Dominance (Low)	Evades Concedes Relinquishes Retreats Withdraws	Acquiesces Agrees Assists Cooperates Obliges

Krech et al.'s Summary of Gough's Predictions

	Attitude (Negative)	Attitude (Positive)
Activity (High)	Aggression	Achievement
Activity (Low)	Withdrawal	Submission

Barber's Four Types and their Associated Strategies

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[86] "The Legislator's First Session," pp. 119–20.

[87] In *The Lawmakers,* pp. 43–45, Barber still describes achievement as a potentially compensatory strategy; but in his discussion of Advertisers (p. 97ff.) he deemphasizes—in comparison with his earlier treatment—the degree to which they manifest achievement and echoes the earlier interpretation (quoted above) which in effect precludes compensatory achievement.

[88] Barber's choice of modal strategies may depend on external influences as much as on the responses of the legislators themselves. In the bibliographical appendix to *The Lawmakers,* pp. 261–63, Barber lists Krech, Crutchfield, and Ballachey, *Individual in Society,* p. 114, as a source on "patterns of interaction." There, in illustrating the effects of the interaction of interpersonal response traits, the authors cite H. G. Gough's speculation about the behavior caused by the interaction of "dominance" and "sociability" (H. G. Gough, *Manual for the California Psychological Inventory* [Palo Alto: Consulting Psychologists Press, 1957]). The authors produce the first table (below) to summarize Gough's predictions. Barber's modal strategies and their respective types are summarized in the second table.

[89] P. 214.

[90] The six specific strategies are listed below in the order Barber discusses them. I have noted in parentheses the number of interviewee responses he offers in support of each. I am not suggesting that all one need do is compare the number of responses to determine which is the group's modal strategy. These 16 responses, after all, are given by only three interviewees; moreover, there is still the problem of identifying each interviewee's *accentuated* strategy. However, attaching significance to the amount of evidence offered does seem to have some merit. As is evident, the first four strategies fall much more readily in the "withdrawal" category.

Spectators' Strategies

(1) withdrawal of "one's emotions from (or avoid[ance of] investing them in) one's experience," p. 79 (no responses)
(2) "*concealment:* the Spectator avoids as far as possible the revelation to others of his supposed inadequacies," p. 79 (3 responses)
(3) "avoid[s] thinking much about matters beyond the immediate situation," p. 80 (4 responses)
(4) "keep[s] his social relationships superficial," p. 82 (6 responses)
(5) "submission," p. 84 (2 responses)
(6) "distraction," p. 85 (1 response)

Lawmakers that Advertisers are typically aggressive. But in "The Legislator's First Session" we discover that three of the Advertiser interviewees—exactly half of the Advertisers interviewed—employed what Barber himself sees as an achievement strategy[91] and that they did so not remotely but to such a degree that Barber at one point describes achievement as being among their typical adjustive techniques.[92] Thus, if achievement was the accentuated strategy of each of these three interviewees, no other strategy could be modal for Advertisers.[93] In contrast, the responses of only one Advertiser interviewee independently evinced aggression. (A comparison of quoted responses suggests that this interviewee is the Advertiser "exemplar," the pseudonymous "Mike Jackson" of *The Lawmakers*. [See note 77.])

The questionnaire data. Although Barber primarily relied on the focused interviews, he consulted the questionnaire responses to corroborate inferences he drew. His references to the questionnaire items are scaled down in *The Lawmakers;* but this revision is purely stylistic, for he propounds basically the same characterizations in *The Lawmakers* as in "The Legislator's First Session." Thus, the quality of this corroborative evidence, or more precisely Barber's interpretations of the evidence, deserves a few comments.

A careful examination of the responses Barber adduces to support his interpretations reveals three closely related errors. The frequency of these errors diminishes the value of the items as supporting evidence. First, Barber attaches significance to trivial differences. Second, he ignores items that contradict his inferences. Third, to support inferences he relies on items that are inconsistent with other inferences.

Attaching significance to trivial differences is abetted by Barber's use in "The Legislator's First Session" of "most," "more," "less," or "least" to characterize a type's relative likelihood of responding in a particular way on an item. These adjectives permit a smoother style but cloak the size of the differences to which they supposedly refer.

Thus, Barber offers the fact that Advertisers are "most likely to choose a 'forceful personality' as an important characteristic for a legislator"[94] in support of his conclusion that Advertisers have a "basically *hostile and exploitive attitude toward others.*"[95] Because two out of 16 Advertisers, but no other legislators, did choose "forceful personality" as one of the three characteristics they thought most important on a list of 13, Advertisers certainly are "most likely."[96] But Barber's interpretation is only a single member away from ascribing the characteristics of any one member of a type to all its members.

Similarly, Barber relies on trivial differences to show the absence of a characteristic. He infers that Advertisers' political activity has not been engendered by their family backgrounds, since in contrast to the families and relatives of Lawmakers, those of Advertisers are "politically uninterested."[97] (This inference supports his explanations of the political activity of Advertisers as compensatory and of Lawmakers as directly motivated by political interest.) Of the Lawmakers, 73 per cent did have politically interested parents; but so did 63 per cent of the Advertisers.[98] Thus, although ten of the 16 Advertisers had politically interested parents, Barber concludes that Advertisers' parents are "politically uninterested"! (Pity these ten: their political activity would have been natural, even healthy, if only two of their colleagues had reported politically interested parents.)

The preceding assumption that the same percentages would have received the same interpretations may not even be put forward with confidence, however, for in some instances the same configurations of responses are not consistently interpreted by Barber. Barber's egregious treatment of the age of Reluctants is an example. Moving from "The Legislator's First Session" to *The Lawmakers,* he more pronouncedly characterizes Reluctants as "elderly."[99] In the latter he even discusses their old age and concomitant decline in self-esteem as a motivation to enter political life. But Spectators are at least as old as Reluctants, as is evident in Table 14; and nowhere does Barber even comment on the age of Spectators. In addition, since only three Reluctants out of 17 are more than 60 years old, characterizing the group as "unusual oldsters," as being even close to senility, is a gross misrepresentation.[100]

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[91] "The Legislator's First Session," pp. 118–19.

[92] *Ibid.*, p. 124.

[93] Again, "accentuated" strategy refers to that strategy on which an individual most frequently relies. "Modal" strategy refers to the strategy which is most frequently "accentuated" by legislators of a type.

[94] "The Legislator's First Session," p. 113.

[95] *Ibid.*, p. 112.

[96] *Ibid.*, p. 298, Table D1.

[97] *Ibid.*, p. 223.

[98] *Ibid.*, p. 278, Table B1.

[99] *Ibid.*, pp. 136–38; see also *The Lawmakers,* pp. 128–29 and 153.

[100] In discussing the Reluctant's "temptation to withdraw," Barber writes: "Given his relatively modest abilities and his flagging energies, the most fundamental temptation he encounters—not only in

Table 14. A Comparison of the Ages of Spectators and Reluctants[a]

	Age							
	20–30	31–40	41–50	51–60	61–70	71–80	Total	(N)
Spectators	3%	30%	27%	20%	20%	0%	100%	(30)
Reluctants	6%	35%	12%	29%	18%	0%	100%	(17)

[a] Constructed from Barber, "The Legislator's First Session," p. 275, Table A2.

Two cases typify the second error, or disregarding items that contradict inferences. (1) Barber argues that Advertisers seek legislative office to enhance their professional careers and to open new opportunities for themselves. He contends that, in contrast, Lawmakers seek office out of an intrinsic interest in political activity and public policy. But Barber fails to point out in this context that Advertisers, in giving reasons for their candidacies, proved "most likely" to cite an interest in legislation. He ignores the fact that Lawmakers were "most likely" to attribute their candidacies to the potential that legislative service has as a stepping stone to higher office. Nor does he note that Lawmakers were "more likely" to attribute their candidacies to the contributions that legislative service might make to success in their regular occupations.[101] (2) Advertisers are supposedly characterized by a dissatisfaction with their regular occupations; but in the questionnaires only one Advertiser reported that he was dissatisfied.[102]

The third error—relying on items that do not consistently support inferences—produces equally graphic examples. (1) Barber construes the fact that Advertisers were "most likely" to have introduced themselves to others at the beginning of the session as corroboration of his inference that Advertisers are hostile and exploitative of others. Advertisers truly were the "most likely" to introduce themselves to others, but only if one combines the responses "very often" and "often."[103] Looking solely at the "very often" responses, one surprisingly finds that Reluctants—those legislators whose adaptation Barber characterizes as *withdrawal*—were "most likely" to have introduced themselves to other members. (2) As another example, consider Barber's interpretation of responses to the item "I am a person with an extraordinarily demanding conscience." Barber regards the fact that Advertisers were "more likely" to agree as corroboration of his inference that Advertisers feel guilt as a consequence of their aggressive reaction to frustration. But "most likely" to agree were Lawmakers![104]

On Generalizing to Populations. By relying on questionnaire *and* interview responses in analyzing the types, Barber implies that characteristics of the interviewees can be ascribed to the legislators who compose the types. But his generalizations are vitiated by an error: the samples from which Barber infers are not samples of the populations to which he generalizes. There are two reasons for this discrepancy.[105]

The first becomes evident through a system-

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politics but in life in general—is the longing for peace, the desire not to be bothered, the temptation to quit trying to perform like a youngster. When this desire becomes intense and general, senility begins, first with an overall slowdown and then with a flickering out of interest and activity in one sphere after another of his life.

"Compared with most people in their age group, Reluctant legislators probably are better able to resist these temptations. They are unusual oldsters in some important respects: they are habituated to hard work and social intercourse, and they have consciences that place a special emphasis on being useful and sociable." In conclusion he finds them still "far short of senility," but the age connotations remain indelible throughout his analysis. *The Lawmakers,* p. 153.

[101] "The Legislator's First Session," p. 285, Table B14.

[102] *Ibid.,* p. 281, Table B6.

[103] *Ibid.,* p. 326, Table E7.

[104] *Ibid.,* pp. 114–16 and 291, Table C2.

[105] The surface simplicity of Barber's methodology belies the formidable array of populations and samples that this methodology necessarily defines. Reconstructing these populations and samples is no mean task, for the information Barber supplies is incomplete and in places even contradictory.

The Lawmakers does not disclose that some of the samples and populations Barber reports include senators, that one of the 27 interviewees whom Barber portrays as members of the lower house (p. 15) was a senator, and that the 94 first-term legislators who were willing to be interviewed (p. 274) included some freshman senators.

"The Legislator's First Session" supplies this information; but it leaves unresolved which of two contradictory figures for those who were not willing to be interviewed is correct. (Cf. *The Lawmakers,* p. 275; and "The Legislator's First Session," p. 79.) Nor can it fix the number of legislators who responded to the postsession questionnaire. (Cf. *The Lawmakers,* Appendix Table 2, p. 275; and "The Legislator's First Session," Appendix C passim.)

atic examination of populations and samples that are defined by Barber's methodology. In Table 15, items C, E, and H are populations that are proper subsets of B, D, and G, respectively. Moreover, C, E, and H each contain members who are also in one or both of the others. Because the interviewee population (D) is composed of freshmen who answered the presession questionnaire (B), the population of freshmen who were willing to be interviewed (E) is a proper subset of C—i.e., all legislators who are in E are also in C. On the other hand, H is not a proper subset of either E or C, and vice versa. H contains some members who are neither in E nor in C; and E and C contain some members who are not in H. Rather the three simply intersect. Thus, if one infers from F—from those representatives whom Barber interviewed—one may generalize only to $E-\epsilon$, the population of which F is a sample.[106] But Barber generalizes to H, the population comprising those freshmen representatives who answered the postsession questionnaire.

To be sure, respondents to the postsession questionnaire might have held particular interest for Barber. In that case he could have generalized to those in H who initially had been willing to be interviewed $[(E-\epsilon) \cap H]$. He could have inferred, however, from only those interviewees who had answered the postsession questionnaire $(F \cap H)$.

Figure 2 is a Venn diagram of the relationships between these samples and populations. The symbols are consistent with those in Tables 15 and 16. The area that is lined comprises those interviewees and questionnaire respondents who are linked by Barber's generalizations from samples to populations. The area that is only *horizontally* lined comprises interviewees and questionnaire respondents who are invalidly linked. These are interviewees who did not answer the postsession questionnaire $[F-(F \cap H)]$, respondents to both questionnaires who were not willing to be interviewed $[(C-E+\epsilon) \cap H]$, and respondents to only the postsession questionnaire $(\tilde{C} \cap H)$. The area that is *vertically and horizontally* lined comprises those who are validly linked. These are inter-

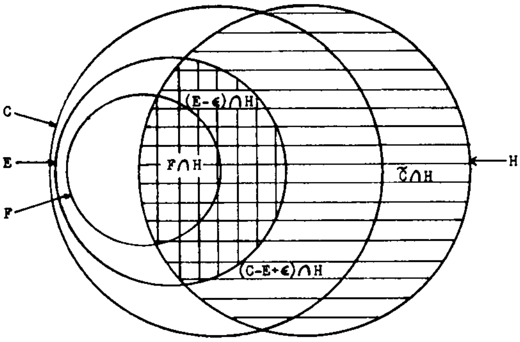

Figure 2. Relationships Between Populations and S Samples in *The Lawmakers*

Table 15. Populations and Samples in The Lawmakers[a]

Description	Relationships[b]
A. Connecticut state legislators: 279 representatives and 36 senators	
B. Legislators who answered presession questionnaire: 184 representatives and 20 senators	$B \subset A$
C. Freshman representatives who answered presession questionnaire: 106 in number	$C \subset B$ $C \cap \tilde{H} = 23$
D. Legislators who were willing to be interviewed: 94 freshmen from B—senators as well as representatives	$D \subset B$ $D \cap C = ?$
E. Freshman representatives who were willing to be interviewed: unspecified number	$E \subset D$ $E \subset C$ $E = D \cap C = ?$
F. Freshman representatives who were interviewed: 25 in number[c]	$F \subset E$ $F \cap H = ?$
G. Legislators who answered postsession questionnaire: 162 representatives and 20 senators	$G \subset A$ $G \cap B = ?$
H. Freshman representatives who answered postsession questionnaire: 96 in number	$H \subset G$ $H \cap C = 83$ $H \cap \tilde{C} = 13$

[a] The information in this table is from "The Legislator's First Session," Chapter 3; and *The Lawmakers,* Appendix C. Items C, E, F, and H have senatorial counterparts that Barber did not report because of his focus on the lower house.

[b] A question mark indicates that the numerical value of the equation cannot be derived from the information Barber supplies.

[c] Barber actually interviewed 26 representatives; but one of these he deleted because extenuating circumstances resulted in the erroneous classification of the interviewee.

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[106] See Table 16 for the meaning of ϵ.

viewees who answered the postsession questionnaire ($F \cap H$) and respondents to both questionnaires who were willing to be interviewed [$(E-\epsilon) \cap H$]. Only the number of legislators comprised by one of these five areas can be determined from the information supplied by Barber: $\check{C} \cap h$ contains 13 members.

The second reason for this discrepancy becomes evident only upon partitioning C, E, F, and H according to legislators' intentions about continued legislative service. The reader will recall that those representatives who responded to the presession questionnaire (those in C in Table 15) were classified by Barber according to their willingness to return. This partition in effect produced two populations, C_1 and C_2 in Table 16. It also divided those legislators who had indicated that they were willing to be interviewed (E in Table 15) into two populations (E_1 and E_2 in Table 16), which were distinguished by differences in the willingness of their members to return. It was from these two populations that Barber randomly sampled his interviewees (F_1 and F_2 in Table 16).

Now, the reader will also recall that the willingness of some representatives to return to future sessions was not stable over the course of the session. Barber accordingly reclassified these legislators, shifting them to "Advertiser" or "Lawmaker" or to "Spectator" or "Reluctant." The procedure would have been innocuous if these legislators had all been reclassified in the same manner. The interviewees, however, were reclassified according to the intentions they expressed during the interviews—conducted approximately halfway through the session[107]—whereas the other respondents were reclassified according to their postsession responses. ($^*F_1$ and $^*F_2$ are therefore not necessarily even proper subsets of H_1 and H_2 respectively.) As a consequence, characteristics that Barber ascribed to questionnaire respondents to whom he might otherwise have validly generalized—$(E-\epsilon) \cap H_1$ or $(E-\epsilon) \cap H_2$—were inferred from interviewees some of whom, it is likely, were no longer even of the same type as the respondents.[108]

The difference in the times at which changes in willingness to return were identified is irrelevant to the validity of Barber's generalizations *only if* all the changes in interviewees' presession intentions took place before Barber

Table 16. Populations and Samples in The Lawmakers (Partitioned According to Representatives' Intentions)

Description	Relationships	
C_1 Unspecified sum of those in C who indicated that they "definitely would" or "probably would" return	$C_1 \subset C$	
C_2 Unspecified sum of those in C who indicated that they "definitely would not" or "probably would not" return	$C_2 \subset C$ $C_2 \cap C_1 = \emptyset$	$C_2 \cup C_1 = C-3^a$
E_1 Unspecified sum of those in E who indicated that they "definitely would" or "probably would" return	$E_1 \subset E$	$E_1 \subset C_1$
E_2 Unspecified sum of those in E who indicated that they "definitely would not" or "probably would not" return	$E_2 \subset E$ $E_2 \cup E_1 = E-\epsilon^a$	$E_2 \subset C_2$ $E_2 \cap E_1 = \emptyset$
F_1 A random sample of E_1; size ranged from 15 to 20	$F_1 \subset E_1$	
F_2 A random sample of E_2; size ranged from 5 to 10	$F_2 \subset E_2$ $F_2 \cap F_1 = \emptyset$	$F_2 \cup F_1 = F = 25$
$^*F_1$ 13 of F who, *at the time of the interview,* were willing to return	$^*F_1 \subset F$ $^*F_1 \cap F_2 = ?$	$^*F_1 \cap F_1 = ?$
$^*F_2$ 12 of F who, *at the time of the interview,* were unwilling to return	$^*F_2 \subset F$ $^*F_2 \cap F_2 = ?$ $^*F_2 \cap {}^*F_1 = \emptyset$	$^*F_2 \cap F_1 = ?$ $^*F_2 \cup {}^*F_1 = F = 25$
H_1 63 of H, the sum of those who "definitely" or "probably would" return	$H_1 \subset H$ $H_1 \cap C = 58$	$H_1 \cap C_1 = 55^a$
H_2 33 of H, the sum of those who "definitely" or "probably would not" return	$H_2 \subset H$ $H_2 \cup H_1 = H$ $H_2 \cap C = 25$	$H_2 \cap C_2 = 25^a$ $H_2 \cap H_1 = \emptyset$

[a] Equation takes into account the failure of three freshman representatives to indicate their intentions on the presession questionnaire. See Table 9. ϵ is a correction for any of these three who may have been willing to be interviewed.

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[107] Presession questionnaires were mailed in December; interviews were conducted March through June; postsession questionnaires were mailed in August. "The Legislator's First Session," chap. 3.

[108] That is, it seems unlikely that $^*F_1 = {}^*F_1 \cap H_1$ and that $^*F_2 = {}^*F_2 \cap H_2$.

conducted his interviews. And there is no reason to assume that changes were restricted to that preinterview period.

Barber's reasons for typing interviewees according to willingness expressed during the interviews rather than on postsession questionnaires can, in conclusion, only be surmised. He may have done so simply to retain in his analysis any interviewees who did not answer the postsession questionnaire [F–(F∩H)].

The Validity of the Prototypal Character Construct. In summary:

(1) The measure for attitude is unreliable and invalid; there is prima facie evidence of the unreliability and invalidity of the measure for activity; and the measure for the two explanatory constructs, self-esteem and strategy of adaptation, is generally unreliable and invalid when used as Barber uses it.

(2) Rarely do the interview responses that Barber adduces unambiguously reveal strategies of adaptation and levels of self-esteem. And these responses never establish the accentuation of a strategy. Together these deficiencies prevent any real association of levels of self-esteem with "accentuated" strategies. Moreover, since Barber separates responses from respondents and even disconnects responses from one another, the responses do not even constitute interpretable individual profiles. Ultimately these failings combine to insure that Barber's ascription of modal strategies to types is arbitrary.

Compounding these problems are others, which are associated with Barber's interpretation of the data from his questionnaires. The value of the questionnaire items as corroborative evidence is attenuated by Barber's acceptance of any difference as significant, by his neglect of contradictory evidence, and by his reliance on items that do not consistently support his inferences.

(3) Barber's generalizations to populations are invalid because the populations to which he generalizes do not necessarily contain the interviewees from whom he infers.

From an evaluation of these criticisms en masse emerges the conclusion that the empirical basis of Barber's prototypal character construct exists only in the eyes of the perceiver. Figure 1 depicts the statuses of the relationships, as determined in the course of the critique.

Fixing the Extraempirical Assumptions. Presented in the foregoing discussion of the interview data was some evidence that even in Barber's earliest research his judgments of an individual's level of self-esteem and accentuated adaptive strategy were interdependent: an achievement strategy was not an achievement strategy if it was compensatory. This evidence, in combination with Barber's characterization of Spectators, Reluctants, and Advertisers as "employing strategies symptomatic of relatively low self-esteem,"[109] suggests that the relationship between these two subconstructs is an implicit prior assumption and, of course, prompts the query, "Predicated on what basis?"

A plausible answer is that the relationship between adaptive strategy and level of self-esteem is a long-recognized one, which Barber simply takes as a theoretical primitive. The writings of Karen Horney, which have substantially influenced Barber's work, suggest otherwise, however. An individual's self-esteem cannot be inferred merely from the adaptive strategies the individual employs (or vice versa), quite simply because adaptive strategies are also culturally determined.[110] Thus: (1) a strategy should not be construed as invariably compensatory or noncompensatory across cultures; and (2) culture should be taken into account in evaluating the behavior and personality of an individual.

A more likely answer is that Barber arrived at his conception of the relationship by initially considering the implications of a strategy from a perspective that emphasized the legislature as a task group and then combining this with the rationale he offers for the conclusion that legislators, relative to nonlegislators, manifest either higher or lower levels of self-esteem. The rationale is as follows: Since "the move into political candidacy" disrupts a person's "daily routine" and does not receive "unambiguous normative support in the general culture," it is most likely to be undertaken by "those who have such *high* self-esteem that they can manage relatively easily the threats and strains and anxieties involved in this change; and those who have such *low* self-esteem that they are ready to do this extraordinary thing to raise it."[111]

Regardless of the answer, however, the point remains that in conducting even his earliest research Barber implicitly treated as axiomatic the relationships between adaptive strategies and levels of self-esteem.

That these relationships can be regarded neither as axiomatic nor as having been empirically established in Barber's research contributes to a larger enigma. Why in the first place did Barber accord self-esteem and strategy of adaptation such importance as an explanation of activity and attitude when there were more tempting, less elusive explanations. For instance, Tables 6 and 12 show that on the activity dimension, at least, the personal profiles of the four types differ substantially. The most powerful single explanatory variable for a freshman's activity is the size of his home town. Barber's activity measure correlates more highly with this variable than with even the activity variables in Table 8. Other characteristics on which there are salient differences are occupation (attorney), age, education, income expectation, and present income.

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[109] *The Lawmakers*, p. 214.

[110] Karen Horney, *The Neurotic Personality of Our Time* (New York: W. W. Norton, 1937), chaps. 6 and 10.

[111] *The Lawmakers*, p. 224. For Barber's full discussion, see pp. 219–25.

But Barber appears to take these variables into account.[112] He describes the typical Spectator and the typical Reluctant, who were recruited in noncompetitive small towns, as, respectively, "a middle-aged, lower status housewife of modest achievements, limited skills, and restricted ambitions"[113] and "an elderly, infirm, retired person, of modest achievements."[114] And he describes the typical Advertiser and the typical Lawmaker—both from larger, more-competitive districts—as, respectively, "a young, upward mobile lawyer experiencing occupational difficulties"[115] and "like Advertisers, young and mobile, but with deeper and more varied political roots and much more interest in full-time elective office."[116] Moreover, he recognizes that these variables might be considered as satisfactory explanations of legislators' scores on the variables that define the types.[117] For what reason then does he emphasize self-esteem and adaptive strategy?

His rationale is clearly stated in "The Legislator's First Session":[118] Some social and economic variables do correlate with willingness to return and activity. But these variables do not really explain the phenomena, for not all legislators with the same cluster of social and economic characteristics are of the same type. Likewise, these social and economic variables do not explain why, in the first place, some people, out of all those in each district who undoubtedly have the same social and economic characteristics, choose to run for the legislature. Another explanation must therefore be sought. Perhaps that explanation lies in legislators' personal needs that are differently satisfied by legislative service. Consulting his interviewees' responses, Barber concludes that for each type except Lawmakers there is a need for enhancing self-esteem and that the need is met by relying on a characteristic adaptive strategy.

Severe problems are associated with this rationale, however. First, it is in essence a demand for an explanation in the form of a sufficient condition. But establishing such an explanation is infeasible: simply assuring that the prerequisite distribution has been achieved entails examining the universe of individuals—in Barber's study, all citizens of Connecticut who had met the constitutional requirements to run for a seat in the lower house.[119]

Second, the supposed adequacy of Barber's alternative to meet the demands of his rationale simply derives from Barber's faulty manipulation of several abstract conditions and their manifestations. The conditions are *resources*, *opportunities*, and *motivations*. Barber considers each to be necessary and only the three together to be sufficient for the recruitment of a legislator.[120] (Or we can say that given any two, the third is necessary and sufficient for recruitment.) Logically it follows, then, that if one of two individuals with identical *resources* and identical *opportunities* pursues a political career, the determinative difference is in their *motivations*. The approach works as well, of course, to establish *motivations* as the determinant of adaptation to the legislature. If the approach is used in explaining both recruitment and adaptation, it produces the same cause: *ceteris paribus*, *motivations* distinguish nonlegislators from legislators and ultimately even the legislators of each type from those of the other three types.

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[112] See his initial consideration of them in his preliminary efforts to find correlates of "willingness to return," in "The Legislator's First Session," p. 40.

[113] I have taken this and the three following quotations from Barber's summary chapter in *The Lawmakers* because his characterizations there are more succinct. Corresponding pages in "The Legislator's First Session" are cited to facilitate comparison. *The Lawmakers*, p. 214; "The Legislator's First Session," p. 64ff.

[114] *The Lawmakers*, p. 215; "The Legislator's First Session," p. 138.

[115] *The Lawmakers*, p. 215; "The Legislator's First Session," p. 101ff.

[116] *The Lawmakers*, p. 216; "The Legislator's First Session," p. 170ff.

[117] See, for example, "The Legislator's First Session," pp. 64–65.

[118] *Ibid.*, pp. 65 and 102–3.

[119] Perfect association—"strong monotonicity" in the sense explicated by Herbert F. Weisberg, "Models of Statistical Relationship," *American Political Science Review*, 68 (December 1974), 1638–55—is a prerequisite for the explanatory characteristic to be a necessary *and* sufficient condition. Likewise, for the explanatory characteristic to be a sufficient condition, those who exhibit the explanatory characteristic must constitute a proper subset of those who exhibit the dependent characteristic; and for the explanatory characteristic to be a necessary condition, those who exhibit the dependent characteristic must constitute a proper subset of those who exhibit the explanatory characteristic.

[120] *The Lawmakers*, pp. 10–15.

The logical neatness of the argument does not, however, extend to real variables. For example, if, in the preceding contingency about recruitment, "identical positions in local political parties of like structure and fortune" is substituted for "identical *opportunities*" and "identical social and economic characteristics [specifically those Barber rejects]" for "identical *resources*," it does not follow that the determinative difference is in the individuals' levels of self-esteem, their personal needs, or even their *motivations.*[121] For the more specific variables do not exhaust the ranges of the abstract conditions. Nor does it seem possible that any tractable set of variables ever could.[122] Every explanatory variable offered—including self-esteem—could be dismissed on the same grounds Barber used to dismiss the social and economic variables. Thus, we can ask about his Spectators, What distinguishes these legislators from other individuals whose preferred party is of like structure and fortune, who have the same social and economic characteristics, and who have low self-esteem, but who do not choose to run for the legislature?

Third, the rationale fosters an a priori evaluation of the relative importance of *motivations,* on the one hand, and *resources* and *opportunities,* on the other. This evaluation ultimately serves to fix the relative importance of the determinants in Barber's analysis. In the argument containing the three conditions, the controlling *ceteris paribus* clause is formally mute about their relative importance. Nevertheless, *motivations* is made prominent simply by virtue of its appearance in the consequence. Of course, one can make another condition prominent instead, and yet retain a valid argument, simply by rearranging the conditions. For instance, if one were to take *motivations* as a given in the place of *resources,* then one might conclude that the determinative difference is in legislators' and nonlegislators' *resources,* that *resources* is the necessary and sufficient condition and, according to this calculus, the cause for recruitment. Obviously, then, the prominence of the condition appearing in the consequence is not necessarily a reflection of the relative importance of that condition in the real world.

But "prominence" is readily misinterpreted as "primacy," especially when (as in Barber's analysis) the argument appears only in one form, which places the same condition *(motivations)* in the consequence. If the context beguiles only the reader and if his misinterpretation of the a priori argument can quickly be set right by an analysis of unbiased data, the error is not important. The researcher himself, however, is not necessarily immune to being mislead by the connotations of relative significance that he in effect bestowed on the conditions. When *he* is mislead, the error is important. Thus, in drawing the following conclusions about the effect of *motivations* on legislators' adaptation, Professor Barber even loses sight of one of his three conditions: "Insofar as most of the real environment is common to all members of an institution, it is likely that differences in reactions are attributable to differences in the needs an individual brings to his new experience."[123] To an eye that has not fixed itself on *motivations,* it is obvious that even *insofar* as legislators experience a common legislative environment, their reactions to the environment will be determined by their *resources* as well.

In such an error the harm is not in the illogic but in whatever bias that illogic introduces into the analysis. In Barber's analysis the bias is in the heightened degree to which Barber saw expressions of personal needs in interviewees' responses. These responses may certainly contain expressions of personal needs, but determining what exactly is an expression of a personal need—what, in this case, was an expression of low self-esteem—is not straightforward.[124] Diffidence and expressions of a sense of inadequacy in a legislative role, for example, do not necessarily indicate low self-esteem. Especially in the maiden legislative session, they may simply be accurate reflections of a legislator's more limited political and social experience. Before attributing them to low self-esteem, one must consider them in the light of reactions that should be expected from an individual with the particular level of experience. Barber's failure to take into consideration experience and other manifestations of *resources* can be explained by the fact that they appear largely in the form of social and economic characteristics. Having discounted these and having even lost sight of the importance of *resources* in general in his search for a sufficient condition, he apparently failed to recognize the necessity of taking them into account when he attempted to determine interviewees' personal needs.

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[121] Cf. George, "Assessing Presidential Character," p. 253.

[122] Thus, *motivations* are not certainly reducible to either productiveness that is functional for the institution (in the case of Lawmakers) or aggrandizement of self-esteem that is disfunctional (in the case of the other three types). Cf. Barber, *The Lawmakers,* pp. 13–14.

[123] *Ibid.,* p. 213.

[124] For example, in "The Legislator's First Session," pp. 66–73, Barber infers a "sagging sense of self-esteem" (p. 73) from "direct confessions of self doubt . . . in situations of exposure to others" (p. 72).

In conclusion, then, though Barber's rationale did not imply anything about the composition of each type of data, his use of the rationale ultimately heightened the degree to which he saw expressions of personal needs in interviewees' responses. In contrast, the rationale did imply prerequisites for empirical support of his explanation; these were a comparison of the responses of interviewees of different types who had the same opportunities and resources and a comparison of these interviewees with similar nonlegislators. But these comparisons Barber did not even attempt. Rather he judged each interviewee's responses against all other interviewees' responses and simply assumed the relative self-esteem of legislators and similar nonlegislators.[125]

Explaining Recruitment and Adaptation: A Most Curious Hiatus. As we have seen, in attempting to explain a legislator's willingness to return and activity during the session, Barber placed his eggs in the basket of personal needs. But given (1) the faultiness of his perception of an absence of change in legislators' willingness to return, (2) the paucity of evidence for the needs he attributed to legislators of each type, (3) the methodological problems in his research, and (4) the logical fallacies in his quest for a sufficient condition, other possible explanations—such as social and economic characteristics he chose to deemphasize—should be considered further. Of course, that young lawyers from large towns are more active in the legislature than less-educated, politically inexperienced, middle-aged housewives from small towns is not the stuff of which ground-breaking theory can be made; but as important indicants of *resources*, these characteristics are as reasonable as "personal needs," or *motivations*, as an explanation for patterns of adaptation.

A problem that is equally as severe as Barber's misattribution of some explanatory importance that rightfully belonged to *resources* is his neglect of the importance of *opportunities*. Because the political environment was a discriminating provider of opportunities, it especially should have been examined. But Barber gave short shrift to the most important condition determining the environment of legislative politics: *he failed even to identify the party affiliations of the legislators.* If he had been studying Mississippi legislators such an omission would be neither important nor puzzling. But Connecticut had a vigorously competitive two-party system.[126]

Moreover, this system had a couple of distinctive features that together with the 1958 election made it likely that party affiliation in the session Barber studied was even more important than usual as a determinant of the patterns of recruitment and adaptation. First, because Republican and Democratic loyalties were reinforced, respectively, by small-town, old-Yankee and large-town, new-immigrant identifications, and because each legislative district tended to be homogeneous, the competitiveness of the parties was for the most part at the state rather than at the local level.[127] Nevertheless, each party maintained an organization in virtually every district and regardless of the prospects for electoral success, attempted at every election to fill a slate—if only with *pro forma* candidates in districts where the party was severely disadvantaged.[128] Second, because legislative districts had not been altered since 1876, the dominantly Republican small towns, though containing only ten per cent of the voters, controlled more than 50 per cent of the seats.[129] Accordingly, observers foresaw little chance that without a redistricting of seats Democrats could gain control of the legislature.

But in the 1958 election, Democratic candidates for the lower house were able to ride the coattails of a very popular gubernatorial candidate. Effecting an important juncture in the political history of the state, voters wiped out the 219 seat advantage they had given Republicans in the previous election and gave Democrats control of the House by one seat.[130] The unprecedented surge in votes for Democrats no doubt swept many *pro forma* candidates into the legislature. The surge also altered the very atmosphere of the House: the party that had been in opposition for 75 years took control; since it comprised few experienced legislators, freshmen assumed leadership positions; this inexperience and an exceptionally large legislative program combined to produce an unexpectedly difficult session.

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[125] Barber, "The Legislator's First Session," p. 73.

[126] See Duane Lockard, *New England State Politics* (Princeton: Princeton University Press, 1959), p. 228.

[127] *Ibid.*, pp. 233–42.

[128] *Ibid.*, p. 231.

[129] *Ibid.*, pp. 271-77.

[130] *Ibid.*, p. 269 n. See also, The Council of State Governments, *The Book of the States 1958–1959* (Chicago: The Council of State Gov'ts., 1958), p. 35; and *The Book of the States 1960–1961* (Chicago: The Council of State Gov'ts., 1960), p. 37.

It is obvious, then, that both the direct and the indirect effects of this surge on Democratic freshmen were different from the effects on Republican freshmen. It follows that in analyzing recruitment, Barber should have attended to legislators' party affiliations; and that in analyzing adaptation, he should have attended to party affiliation and to the working conditions of the legislature, which he had early on chosen not to study.[131]

Dusting Off an Old Caveat. Barber's failure to take party affiliations and legislative working conditions into account adds to the previously discussed problems and, of course, to reservations about his conclusions. At the time he wrote *The Lawmakers* he was aware of some of the general problems. In the very first chapter he writes: "Suffice it to say that the techniques of data-gathering and analysis used do not permit us to consider the results as more than suggestive hypotheses for further research."[132] And in the appendix on methodological details he cautiously advises that "the interpretations reported here, then, should not be taken by those unfamiliar with problems of questionnaire surveys, interviewing, statistical analysis, and the like as definitive empirical findings."[133] This critique specifies why Barber's conclusions are speculative. It also justifies reinstating Barber's initial caveats, which he in effect set aside by treating these "suggestive hypotheses" as assumptions in his presidential research. Finally, it raises as well the question of whether Barber could have appropriately generalized his prototypal character construct to presidents even if he had empirically established that construct in *The Lawmakers.*

The Lawmakers as Prologue

Situation-Specific Measures and Inappropriate Constructs. A president qua president does not introduce bills in the Connecticut House, comment in the committees of the House, or speak from the floor of the House chamber. Asking where he might fall on Barber's activity scale, then, is as nonsensical as asking whether he would be willing to return to future legislative sessions.

Related to this problem of measures that are inappropriate for Barber's research on presidents is the problem of a subconstruct that is equally unsuitable. "Attitude toward the group" fits presidents no better than does its measure, willingness to return. "Affect," the subconstruct that Barber substitutes, is similar to but obviously not synonymous with its supposed counterpart.

Thus, in order for Barber to treat the character construct as axiomatic in his study of presidents, he would initially have had to establish the relationships between the measures he used in classifying legislators and the corresponding assessment criteria he used in judging a president's affect and activity. Second, he would have had to specify the theoretical relationship between "attitude" and its presidential counterpart, "affect." His failure to do either only accentuates the emptiness of his axiomatic treatment of character, an emptiness that initially derives from the absence of demonstrated empirical support for the construct.

Atheoretical Insight and the Prediction of Crisis. The question of course arises, How was Barber able to predict accurately the tragedy of Richard Nixon if the theory sustaining *The Presidential Character* is not true? One response is simply to deny that Barber's prediction proved accurate. George, for example, questions both whether Watergate is the kind of tragedy that Barber predicted and whether the occasions Barber offered as examples of Nixon's rigidification really show Nixon to be rigid.[134] A second response is to grant the correspondence of the prediction with reality but to take issue with the premise that Barber should somehow be given credit for a correspondence to which fortuity has just as much claim. A third response is neither to deny nor to question these premises. It is simply to note that a qualified prediction of tragedy, a prediction like the one Barber offered, does not fly in the face of reason that reckons on nothing other than an understanding of the office, a comprehension of Nixon, and a projection of the crises recurrent in Nixon's political history. All three are factors in Barber's analysis; together—without the theory—they can produce the prediction.[135]

Barber's Theory in Perspective. *The Presidential Character* is not only a collection of well-written, interesting biographies but also the medium for a predictive theory that provides continuity and promises practicability. Acquiring sufficient information to make the judgments demanded by the theory is not as simple, however, as the reader may perceive. Except for candidates long in the political limelight, campaign "puff pieces" may be the only references.[136] Consequently, even if doubts about the truth of the theory were nonexistent, the practicability of Barber's predictive technique would still be illusory. The availability of necessary information is certain only after the fact.

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[131] See above, p. 190; and "The Legislator's First Session," p. 45.

[132] *The Lawmakers,* p. 16.

[133] *Ibid.,* p. 271.

[134] "Assessing Presidential Character," pp. 239 and 262–63.

[135] Reservations about the truth of Barber's character construct do not constitute a conclusion that Barber's descriptions of Nixon are necessarily less perceptive than others' descriptions. Cf. Bruce Mazlich, *In Search of Nixon* (Baltimore: Penguin Books, 1973); Garry Wills, *Nixon Agonistes* (Boston: Houghton Mifflin, 1969); and Hargrove, "Presidential Personality and Revisionist Views of the Presidency."

To point this out is not to argue that a candidate's personality is unimportant, or that studying a candidate's personality is not worthwhile. It is only to state that typing a candidate is difficult; that the accuracy of the conclusion is uncertain even with the right kind of information; and that without that information, typing a candidate in the manner Barber prescribes can be expected to produce accurate conclusions no more frequently than typing at random.

Again, however, this last paragraph assumes a true theory. If, in the end, the truth of Barber's theory can only be determined empirically, then the process will be lengthy, spanning some minimum number of cases multiplied by a factor of four years. Of course, in assessing the personality of a presidential candidate, a student of politics well may employ the theory. But he should do so only with caution, aware of the speculative nature of the theory and of the distortions that may result from deductions about a candidate's personality in the absence of information adequate for an independent determination of the fit of the theory in the particular case.

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[136] Cf. Barber's discussion of his sources, *The Presidential Character,* pp. v–vii.

Comment: Qualls's Nonsensical Analysis of Nonexistent Works

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Any brave soul who has plowed his way through the preceding must be wondering how in the world such a piece found its way into the *APSR*. After all, this *Review* has a backlog literally years long (as this is written, a year before publication) and a severe space problem. Surely only the cream of the political science crop can be accommodated. Mr. Qualls's piece must, you would suppose, have survived the most rigorous of editorial screening processes.

Not so. The system is meant to be different by the time you read this, but when Mr. Qualls got his piece accepted, *APSR* was put together in comic opera fashion. The managing editor did not presume to *read* articles he decided to print. He relied instead on referees he chose. Not having read the instant piece, all the editor could have had to go on were the title and the abstract, either of which might have led him to believe, erroneously as it turns out, that Mr. Qualls was dealing mainly with my work on presidents. Or, who knows? Perhaps the editor simply consulted chicken entrails. In any case, two referees were selected and their report determined the choice entirely—so much for the enormous hurdles standing between an author and these pages. They amount to two readers; if they say yes, in it goes. If *one* of them says no, out it goes. This discovery was as amazing to me as to all those authors who have puzzled over the supposedly intense competition for *APSR* space, on the one hand, and the poor quality of so much of what has appeared in these pages. *The American Political Science Review* was not edited at all. The articles were selected entirely by pairs of referees picked in ignorance of what they were to judge.

No doubt the managing editor will want to explain this nonfunction in the pages of the *Review* he has arrogated for his personal use.

In this case, fifty per cent of the referees was a fellow who had already advanced similarly vague and lengthy attacks on *The Presidential Character*. The editor may not have known that, being generally too busy to read journals. Nor would the editor have known that Mr. Qualls spends page after page in fervent praise of—this Referee #1 himself!

Now it is not hard to imagine the torment of soul this potential referee *must* have experienced (if we may indulge in long-distance analysis) as he struggled with the decision whether or not to act as impartial judge (and, as it turns out, half the jury) in this case. His judgment of the work in question was already pre-formed and published. Shouldn't he send back the Qualls paper with a note disqualifying himself, recommending that some other reader be found? Not only that: here were those fulsome paragraphs in which Mr. Qualls pours perfume on this potential judge's head. Indeed, the struggle must have been a severe and protracted one, but at last this referee took the bit in his mouth and decided on publication. As far as I know, the other referee, if he read the manuscript, had no such problem.

Once Mr. Qualls's flattery had its effect and his paper was mechanically slated for publication, he was free to tone down his praise of Referee #1 in a revised version. It took truly heroic editorial efforts (though not by the managing editor) to rewrite the piece for Mr. Qualls, draining the swamp of his original prolix, wandering, error-polluted version to reveal the deteriorating stumps of his reasoning. This process deprives you, reader, of several interesting facts about Mr. Qualls's record. He has omitted the fact that he failed to develop predictions of the performance of candidates in 1972,[1] perhaps because that would cast doubt on his standing as a critic. He eliminates reference to the one prediction he made in Version One of this paper: That "heightened recognition of the possible consequences of organizations run amuck [*amuck?*—wonderful image!] has been instrumental in producing at least a few surface changes in White House operations and even seems to have insured against the use of one particular form of campaign organization by that most prominent prospective presidential candidate—Gerald Ford." If you follow that, it means Ford would avoid Nixon's type of campaign organization. Alas, in the event Ford didn't so avoid. He too set up a separate non-White House, nonparty outfit. It is true he did not call it CREEP.

Mr. Qualls thus approaches his critical task innocent of any positive achievement in this or any other field of political science.[2] He did not have the courtesy to send me a copy of his critique to comment on, despite his professor's advice to do so. At the cost of a good deal of

[1] I tried that in "The Question of Presidential Character," *Saturday Review* (September 23, 1972), pp. 62–66.

[2] However, now that he has "published in the *APSR*," he can of course be taken as an expert.

delay in his dissertation work, he plodded ahead with this negative project, year after year, forcing me, lest his cockeyed version of my work be allowed to stand, to use my dwindling time correctly. The version printed above has obviously been changed from the version accepted for publication to give Mr. Qualls the advantage of the reply I wrote previously, thinking I was replying to what would be printed. This nitwit procedure could go on *ad infinitum*—my trying to teach at long distance a student whose interest is not in the truth of the matter but in hanging onto this fortuitous and surprising chance for publication.

It is a mystery to me why Mr. Qualls interrupted his graduate studies for such long and useless labors. But he has at least offered to his fellow graduate students a lesson in how to get into these pages: attack someone, praise the potential referees to the sky, and go on at such length that a response is provoked. Then, because you made the first move, space must be provided, in this topsy-turvy system, for a "rejoinder" by you—as if you had advanced some original argument.

As anyone at all familiar with *The Lawmakers* and/or *The Presidential Character* knows immediately, neither book is offered as some mathematical, mechanical, or definitive treatment of the subject. I have no concern that people who have read these books will be misled by Mr. Qualls. But others may too easily fall into his simpleminded frame of reference—the easiest form of criticism yet invented—by which he builds an exaggerated picture of the books' claims and then takes credit for destroying his own exaggerations. His first error, however, is the opposite; he prepares for his later hyperbole by a trivialization.

These preliminary comments are necessary because I am 98.6 per cent sure very few readers will have waded through the above swamp of distortions, yet very many will casually assume that there must be some fire under all that smoke. Not so. The smoke is there by a mere accident of procedure which happened to couple with a weak code of professional ethics. Underneath is sheer gas.

Qualls's First Mistake: Trivializing the Nixon Prediction. Qualls's many small mistakes are not worth bothering with. He begins with an attempt to summarize the approach I use in *The Presidential Character*. Here we get the first of a long and ballooning series of miswordings, as the critic strains for originality at the expense of conveying, clearly and straightforwardly, what I said, until at last my mouth is full of such Quallsian inventions as "subconstructs" and "prototypal constructs" and the like. None of this is very significant in itself, just sloppy perceptions, and strawmanning, culminating in a simply silly imaginary account of my "research strategy." I am glad to invite comparison with the account in the book.

But then we get to Mr. Nixon and in a few fragmentary paragraphs I am head-patted for predicting, in the early Spring of 1972, the development of the Nixon presidency. Mr. Qualls is not committing himself, but thinks "most readers of *The Presidential Character* will undoubtedly deem the prediction accurate."[3] I appreciate even that begrudged compliment, but not the gross underestimation of what was actually predicted in *The Presidential Character*. There was a good deal more to it than several scattered items and a generality. By the time these words appear in the *APSR*, a new edition of the book, with a follow-up of the Nixon predictions, may already be available. For now, it is no good to pretend to deal with Barber's theory without exploring seriously its main empirical test to date. Qualls finds it convenient to skim over that as quickly as possible.

Just what was predicted about Nixon? Well, first the continuity of his character, despite all sorts of contemporary nonsense about "new" Nixons. The data and theory led me to predict that Nixon would continue as an active-negative character type, which I describe as follows:

> Active-negative: The contradiction here is between relatively intense effort and relatively low emotional reward for that effort. The activity has a compulsive quality, as if the man were trying to make up for something or to escape from anxiety into hard work. He seems ambitious, striving upward, power seeking. His stance toward the environment is aggressive and he has a persistent problem in managing his aggressive feelings. His self-image is vague and discontinuous. Life is a hard struggle to achieve and hold power, hampered by the condemnations of a perfectionistic conscience. Active-negative types pour energy into the political system, but it is an energy distorted from within.[4]

On the basis of close study of biographies of three other active-negative presidents, I thought

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[3] "Barber's Typological Analysis of Political Leaders," *APSR*, 71 (March, 1977), 184.

[4] *The Presidential Character: Predicting Performance in the White House* (Prentice-Hall, 1972), pp. 12–13. Quoting such summary passages out of context, without the mass of evidence on which they are based, sharply decreases their impact. But at least the meaning is clearer than the little phrases Mr. Qualls extracts. For readers unfamiliar with the book, *The Presidential Character* devotes most of its 479 pages to biographical material.

Nixon's second term (if any) would confirm that he shared their characteristics:

> The active-negative type is, in the first place, much taken up with *self-concern.* His attention keeps returning to himself, his problems, how he is doing, as if he were forever watching himself. The character of that attention is primarily *evaluative with respect to power.* Am I winning or losing, gaining or falling behind? It is, secondarily, *evaluative with respect to virtue.* In the struggle, am I being a good or bad person?
>
> The active-negative's perfectionistic conscience lends to his feelings about himself an *all-of-nothing quality.* He waivers between grandiosity and despair. Similarly there is little incorporation of a sense of the self as developing in time, progressively growing through experience; rather, there is a *now-or-never quality.* Similarly, the perfectionism imposes unclear guidelines for achievement; one is supposed to be good at everything all the time. Therefore there is a resistance to self-definition, a lack of clarity in the person's commitment to shared loyalties and to particular sequences of achievement building toward special goals.
>
> The demands of conscience also impose a felt necessity for the *denial of self-gratification.* The active-negative not only behaves so as to suffer in fact—by working to exhaustion, for example—but also insists on explaining his behavior to himself and to others, as self-sacrificing rather than self-rewarding.
>
> The power emphasis is reflected in the active-negative's concern with *controlling his aggression.* He will tend to view himself as restrained, holding back, reining in his anger, patient despite much provocation, and so on. By building up a view of his anger as monumental, he strives for approval from conscience as a reward for the effort and suffering it costs him to hold it in.
>
> These two themes—the denial of self-gratification and the struggle to control aggressive impulses—come together in the active-negative's perennial *temptation to fight or quit.* Images of breaking out, attacking, releasing free anger compete with fantasies of abandoning effort for quiet, relaxation, ease—even death. These are experienced as temptations in a double sense: one might get at others by striking at them or by abandoning them, and one might give in to self-gratification by removing the falling barriers to aggression or by wallowing in weakness. These tempting fantasies help the person bolster his feelings of strength and virtue as he resists them.
>
> The active-negative lives in a *dangerous world*—a world not only threatening in definite ways but also highly uncertain, a world one can cope with only by maintaining a tense, wary readiness for danger. The prime threat is other people; he tends to *divide humanity into the weak and the grasping,* although he may also, with no feeling of inconsistency, idealize "the people" in a romantic way. In struggling to understand social causality, he restricts the explanations to *conspiracy or chaos,* fluctuating between images of tight, secret control and images of utter disorder. He strives to resolve decisional conflicts by invoking abstract principles in order to render manageable a too complex reality.
>
> The active-negative's political *style is persistent and emphatic.* That is, he shows a stylistic specialization more markedly than other presidents do (as in Wilson's oratory, Hoover's homework, Johnson's interpersonal relations), and he tends to inflexibility in shifting his stylistic repertoire. Furthermore, he is likely to *extend his primary stylistic emphasis into his total style,* to treat all occasions as if they were amenable to mastery by means of his main political habit pattern.
>
> While the active-negative's character is taken up with his own performance, he continually seeks confirmation of his self-esteem from other people; in this sense he is highly *dependent upon positive response from the environment.* He feels confirmed in his expectations by vigorous opposition, but is disconcerted by and strongly threatened by ridicule, contempt, or personal denigration. His tendency over time is to *focus anger on a personal enemy,* usually an opponent who treats him, he feels, with condescension.
>
> But the most pervasive feeling in the active-negative's makeup is *"I must."* He is a man under orders, required to concentrate, to produce, to follow out his destiny as he sees it. At any given moment, he feels bound by what he has already undertaken, already promised, already committed. The central conflict between virtuousness and power-seeking is never resolved, but is massively denied in the feeling that whatever one does, one has no choice. The tragic sacrifice in such a personality is the sacrifice of will. Not only others, but the man himself is reduced to an instrument. He finds it hard even to see alternatives to the course he "must" follow, much less to change that course when it proves unproductive.[5]

In addition to these continuing characteristics, Nixon as an active-negative was likely to follow out a pattern the other presidents of that type had pursued. I call it rigidification: "a common pattern, the essence of which is this: The President defeated both his own purposes and the nation's purposes by adhering rigidly to a line of policy long after it had proved itself a failure."[6] Nixon like the presidents in the other active-negative histories would eventually appear "as a man unable to see what, eventually, nearly everyone else around him sees: that the line of action simply is not working—that, for whatever reason, the costs of persevering in it are far too high."[7]

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[5] Barber, *Presidential Character,* pp. 96–97.

[6] *Ibid.,* p. 42.

Nixon like the other active-negatives would develop certain themes in the process of rigidification, themes described for the type as follows:

> *The Fight Against "Giving In."* The President experiences a sense of temptation. The issue is highly moralized, a matter of principle, not prudence. Therefore to compromise, to move toward any easy "panacea," is seen not only as mistaken but evil. In moral terms the cost of compromise is simply too high for the President as a person: He would have to sacrifice his own integrity, his stance as being responsible to his conscience. Furthermore, he sees the conflict as one between strength and weakness. He himself, he feels, must be tough—and others should also show their mettle, their fiber, their manhood. The soft way out is closed to him, for his fight against giving in is a double fight, against both sinfulness and weakness.
>
> *The Answer in Effort.* The President experiences severe depression as the personal implications of his policy unfold. In an attempt to overcome his practical problems he turns to work, which he progressively transforms into grueling labor in which his effort is more and more narrowly focused. He loses himself in his task, concentrates on repetitious plunges into the most difficult labors. Throughout his working, he suffers—and reminds himself, again and again, how much pain and self-sacrifice he is experiencing.
>
> *The Long Struggle.* Increasingly as the issue develops, the President feels that the responsibility for success depends entirely on him. The policy becomes his policy, inexorably welded to his person; its defeat means his defeat. The reason he must bear the burden alone is, as he feels it, that others who might help him keep deserting him. In a way he would prefer to welcome them as followers, but bitter experience has taught him not to trust them when the chips are down. The result is a President isolated from independent voices in his own camp.
>
> *The Appeal to Faith.* The President increasingly uses opportunities for persuasion as opportunities for exhortation. The policy problem is transformed from a matter of calculation of results to a matter of emotional loyalty to ideals. The President appeals for faith in himself as bearer of the nation's promises. Because the cause is so tremendously important, the President concentrates on whatever rhetorical tactics are necessary for his grand strategy; *his* view of reality must be accepted, else the cause fall apart.
>
> *The Emergent Enemy.* The President experiences intense anger as he meets the frustrations imposed upon him. That anger moves gradually away from him, outward to targets in the environment. The beginnings may be mere irritation kept in the privacy of his staff, but it extends and deepens to include whole categories of weak and immoral people on the outside. As the process of rigidification moves along, the President finds among his enemies an individual who, to him, personifies the threat. That person becomes the focus for the President's aggression; the critical task becomes defined as defeating this emergent enemy at all costs. The alternative is defeat at his hands, surrender.[8]

Nixon also, if he followed out the pattern of the other active-negatives, would experience the rigidification process in this fashion:

> From the inside, then, the active-negative type generates tremendous energies for political domination. From the outside, he seems at first extraordinarily capable and then extraordinarily rigid, becoming more and more closed to experience, including the advice of his ardent allies. Over time, he has a powerfully disillusioning effect, because so much was expected from him when he started, but these expectations have been disappointed continually as the man stubbornly adheres to his course and waxes so moralistic in its defense.
>
> This temporal process as seen from the outside is matched, I think, by a regular development in the active-negative's own character. He sees himself as having begun with a high purpose, but as being continually forced to compromise in order to achieve the end state he vaguely envisions. Battered from all sides with demands that he yield yard after yard of his territory, that he conform to ignorant and selfish demands, he begins to feel his integrity slipping away from him. In doubt about his personal strength, he experiences compromise as a steady diminution of "the most powerful man in the world" to a mere clerk, ordered about by his supposed subordinates. At the same time, he is being harassed by critics who, unaware of the problems he faces, attribute his actions to low motives, adding insult to injury. At long last, after enduring all this for longer than any mortal should, he rebels and stands his ground. Masking his decision in whatever rhetoric is necessary, he rides the tiger to the end.[9]

To these characteristics general to the active-negative type, Nixon added (as did every president) individual peculiarities. If all of the above could be predicted from type characteristics, even more could be predicted from Nixon's variations within the type. Again on the basis of detail after detail—not on the basis of vague speculation—Nixon emerges as a man whose conscience—his politically relevant ideal self—

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[7] Barber, *Presidential Character*, p. 43.

[8] *Ibid.*, pp. 56–57.

[9] *Ibid.*, pp. 97–98.

> remains obscure as long as we try to find it in ideology, philosophy, allegiances to groups, places, and roles. Yet he is obviously a highly moralistic man, constantly preaching about how one should arrange his feelings. I think it is the "howness" rather than the "oughtness" of his interior life that gives the clue to understanding, at a different level, the Nixon self-image. In psychological terms, Nixon substitutes technique for value. His energies are taken up with the struggle to resolve certain fairly continuous conflicts among character forces. The model is not that of progressively approaching an ideal, but of managing, controlling, and molding the tensions among competing drives. These conflicts are externalized in ways which give an impression, when viewed from without, of inconsistency or vagueness. But when one looks at the right place—at conflict management rather than goal attainment as the chief problem—there is a more coherent structure to the Nixon personality. For him, the task of raising self-esteem is less a matter of positive development than of success in warding off dangerous tendencies he discovers within himself. In this light, his constant attention to the arrangements of his inner life represents the role of conscience as protector, as self-maker, as climbing guide picking a characterological path up the mountainside.
>
> Viewing Nixon as self-monitor and self-manager highlights his search for signs that he is performing correctly. Those signs are inherent in the management process itself, not in some external determinant or consequence of the process. Nixon is seen as a man on the run, watching himself run, criticizing his form as he runs. He *tends* himself.
>
> Consequently Nixon's criteria for judging his performance are apt to be formal rather than substantive. How does he know when he is doing well? From what he has said and written, one criterion stands out: the experience of difficulty confirms the moral worth of action.[10]

The Nixon character's particular versions of the struggle for power are described and predicted as part of his active-negative pattern.

Then because, contrary to Mr. Qualls's reading, there is more to the theory than character, I go into Nixon's world view and style, in a series of predictions I consider quite important. His view of human nature would continue to be a dark one:

> Nixon sees people as essentially untrustworthy ... Nixon has never had many close, personal friends ... Nixon goes on and on about the ingrates and disloyalists in politics, ending up with his recommendation that "those who reach the top ... develop a certain tough realism as far as friendships and loyalties are concerned." ... "Frankly, most people are mentally and physically lazy." ... People are undependable in part because they lack the steely strength to control themselves.[11]

Then, following an analysis of data on Nixon's view of history and his role in it, I summarize these continuing themes:

> When we combine these sentiments (and there are more such expressions) regarding fate, destiny, luck, and uncontrollable change with Nixon's emphasis, seen earlier, on the critical moment, the key move, and the right step, it is possible to derive a picture of causality temporally considered. For Nixon there is "up there" an unknown flow of fate rolling on into eternity; "down here" is man, crouched and waiting, prepared through effort to catch the sudden sign for action, to show, through his success in meeting the critical moment, that fate and the self have intersected. There is a *tendency* in Nixon to see the world in this way, eliminating from consciousness the middle-ground progressions, leaving unfilled the gap between the present and eternity. In the midst of this he holds to the view that he is one of those marked by destiny. He need not invest all his need for success in his own ambitious climb, because (a) it is impossible to know precisely when and where destiny will point its finger at him until the moment comes and (b) he has faith that somehow that must happen. These are the themes of predestination, straight from the Reformation. Their implication is not for passive watching and waiting, but for intense preparatory effort and then fast risk in the key event.[12]

After reviewing changes in Nixon's vision of the tightness of the "Communist conspiracy," I concluded that the conspiratorial thinking was more significant than any of its temporary objects:

> Nixon's world view is a psychology, not an ideology. He is prepared himself to act and to urge action in the face of an unknown fate and despite the unreliability of mankind. That vision—of human activity as preparatory, as a labor aimed at justifying man's claim to salvation at the crucial moment—fluctuates with another, in which what makes the world move is all too clear, the vision of the implacable enemy subtly and secretly spreading his poison beneath the surface.[13]

Nixon's style, his history indicated, would continue to center in rhetoric. Well before Watergate, I wrote that

> Nixon's antipathy to the press is evident. He writes of their "suspicion," "hostility," skepticism, inaccuracy, unjust "interpretations," and "savage and sadistic" reports about which

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[10] Barber, *Presidential Character*, pp. 360–361.

[11] *Ibid.*, pp. 375–376.

[12] *Ibid.*, p. 379.

[13] *Ibid.*, p. 380.

> "I would become so infuriated . . . that on more than one occasion I slammed the paper or magazine into the fireplace."[14]

Again before Watergate, the evidence made it clear that

> another specialized feature of his rhetorical style, one significant for his Presidency, is his use of secrecy and surprise. Part of being in control of a situation, for Nixon, is controlling the extent and timing of revelations. Surrounded by hostile watchers and a gullible public, the politician must be very careful what he lets them see and when. From the start, Nixon has been an aficionado of the well-kept secret suddenly revealed . . . In the Hiss case there is much of this: "We should not delay the *confrontation. Only the man who was not* telling the truth would gain by having additional time to build up his case."[15]

Even before Watergate, Nixon's rhetorical

> approach has an effect in narrowing the mental vision to the immediate. One is taken up with the internal relationships among features of the given. The situation or problem at hand constricts the possibilities, sets boundaries around the task. The "innuendo" comes about in part because all conceivable factors in the situation are seen as linked by a necessary integument to a key detail. A variety of tunnel vision may develop, in which the focus is strongly on the problem-as-stated in the situation-at-hand.
>
> A result of this style of thinking has important implications for the Nixon rhetoric. He repeatedly shows the tendency to respond and react to the immediate situation he faces, rather than to the larger and longer range implications.[16]

And his rhetorical specialization made him feel that

> once having spoken, the words must be defended. They tend to become, for the rhetorical type, no longer symbols more or less representative of an objective reality, but realities in their own right. Particularly when someone points it out to him, a Nixon is likely to treat his rhetorical ploys as serious commitments.
>
> The danger of such a tendency is obvious.[17]

A peculiar Nixon pattern with important implications for his future, as of Spring 1972, was his "classic crisis," as I called it. Problems were bound to come to him.

> When there is one among these problems which he perceives as making him *seriously vulnerable to public exposure of personal inadequacy*, he is likely to move into his crisis pattern. The threat of this situation is multiplied when it is combined with a *serious risk of losing power.* These themes are linked because he believes his power to be dependent upon his public reputation as an able, reliable, and righteous person. But in order for his crisis syndrome to take hold, the threat must reach to the question of personal adequacy, not just to some policy position. And it must threaten *public* exposure of inadequacy; Nixon has passed through a great deal of more or less private rejection (as in the Eisenhower administration) without its triggering a crisis.
>
> This threatened feeling may come to him in several forms. Perhaps the most damaging is ridicule. His compulsive "fighter" stance makes him expect, and enables him to endure, direct opposition. But being laughed at throws him off balance, relegating him to the position of a joke, someone unworthy of serious combat. Other forms of severe threat include attacks on his moral worth—especially his honesty—and his perception that he is in a situation in which the public may well expect him to fail.[18]

The Nixon crisis pattern consisted of four stages: "Fastening. At this stage Nixon is aware of a threatening situation and must decide whether or not to take it on as a crisis."[19] "Tensing. Having committed himself to a crisis, Nixon goes into a period of focused tension, of concentrated hard emotional labor directed toward meeting the particular emergency."[20] "Release . . . This is the execution time, the moment toward which all the effort and worry have been directed."[21] "Letdown . . . The main crisis event has been endured. A natural feeling would be relief and relaxation. But this is a snare and a delusion."[22]

"The primary danger of the Nixon Presidency," the data indicated in early 1972, "is that the frustrations and erosions of self he experiences will accumulate and that the process of rigidification, triggered by a serious threat to his power and his moral confidence, will show him a way to rescue, as he sees it, his Presidential heroism."[23] Reasoning from the evidence, I predicted as follows:

> So far his crises have been bounded dramas, each apparently curtailed with the end of the last act. The danger is that crisis will be transformed into tragedy—that Nixon will go from a dramatic experiment to a moral commitment, a commitment to follow his private star, to fly off in the face of overwhelming odds. That type of reaction is to be expected when and if Nixon is confronted with a severe threat to his power and sense of virtue.

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[14] Barber, *Presidential Character,* pp. 382–383.

[15] *Ibid.,* p. 383.

[16] *Ibid.,* p. 384.

[17] *Ibid.,* p. 385.

[18] *Ibid.,* p. 387.

[19] *Ibid.,* p. 388.

[20] *Ibid.,* p. 389.

[21] *Ibid.,* p. 391.

[22] *Ibid.,* p. 292.

[23] *Ibid.,* p. 418.

Nixon's is a special variant of the active-negative character. With his remarkable flexibility regarding issues and ideologies, Nixon can be "defeated" any number of times on specific questions of policy without feeling personally threatened. His investment is not in values, not in standing fast for some principle—although, if he were to stand fast, his doing so would certainly be rationalized in terms of principle. His investment is in himself, and Nixon's self is taken up with its management. As Margaret Mead has noted, "The President thrives on opposition. It is a form of stimulation for him." Thus he will court the strains of political resistance, finding in them yet another confirmation of his virtue.

But let the issue reach his central concern, the concern of self-management, and the fat may go into the fire. Threats to his independence in particular—the sense that he is being controlled from without because he cannot be trusted, because he is weak or stupid or unstable—will call forth a strong inner response. For Nixon, the prime form of the active-negative command "I must," is this: *"I must make my own way."* Only when a crisis gathers around him, one he cannot escape by moving on to some alternative crisis, and he experiences a sense of entrapment is he likely to move toward the classic form of rigidification.

The key variable here is time. "Time running out," the President wrote on his pad before Cambodia. As the clock ticks forward, Nixon confronts his future in two stages. First is 1972. As the election approaches, Nixon's Presidential fate will clarify itself. If the uncertainties fade in the light of the polls, and the probability of a defeat for Nixon rises sharply, this President will be sorely tempted to do what he feels he must do before it is too late. The loss of power to forces beyond his control would constitute a severe threat. That would be a time to go down, if go down one must, in flames.

Nixon victorious in 1972 would face a second clock. Then he would know exactly when his time would run out. Short of impeachment, he would be unremovable until 1976; and then would be removed for sure. No longer accountable to an electorate, the Nixon pragmatism would have to take a different focus, one not restrained by calculations of electoral popularity. Then the salience of Presidential virtue would rise in Nixon's mind, as power faded. Accountable then to God and history, Nixon would have before him the models of heroes.

Two heroes he has often expressed admiration for are Wilson and Churchill, both men who marked the world and then were rejected by their people. Their rhetorical styles and dramatic lives appeal to the theatrical in Nixon. But conceivably he could come to find an example in another man of independence, unheroic Harry Truman, who drew upon inner strengths he hardly knew he had to move beyond toughness to achievement.[24]

Now that, in truncated form, is what *The Presidential Character* predicted for Nixon. There will be other predictions for other new and used Presidents, but so far, this scheme's main test has been in this particular set of predictions based on highly detailed evidence too extensive to quote here. Mr. Qualls simply ignores all that, instead dismissing the whole matter with a little fingertip applause. Even that fades away by the end of his piece (he may have forgotten by then where he started) when he reduces the specifics to a mere "prediction of tragedy" which any *noodnik* could have come up with.[25] But where was Mr. Qualls when we needed him? Failing to develop predictions in 1972, according to his own account.

Surely Mr. Qualls in the first place and the pair of referees in the second place should take what the book claims and deal with that directly. The main claim of *The Presidential Character* is a claim to prediction, and the book develops the single predictive case of Nixon in more than a hundred pages. All that is treated as if it were trivial.

Qualls's Second Mistake: Qualls Reviews Qualls. Mr. Qualls is not what you would call a data man. His favorite word is "seems." He writes in a fog of seemingnesses, alludings, indicatings, evincings, and adducings, like a drunk in the dark feeling for his socks under the bed. When he wants to describe my research strategy, he passes by an article I wrote on the subject, "Strategies for Understanding Politicians,"[26] and goes instead to quote Alexander George on the subject. Mr. Qualls imitates George's review in continually vaporing about imaginary problems rather than facing, fairly and squarely, the evidence presented and the interpretations advanced.[27] At least in one case, that of the influence of Herbert Hoover's world view on his presidential behavior, George got around to a question debatable on the evidence; Qualls never reaches that point.

The point is the simplest one in the world: if you insist on throwing around charges of biased assessments, strained interpretations, and the like, then let us have your unbiased and unstrained ones—and give us the evidence to support your alternative conclusion. Name-calling is easy and irrelevant. The task is to argue what is, from the facts, not to guess at what seems to be, from the peak of your fantasy. Reading this piece of Mr. Qualls one would never guess that both *The Presidential Character* and *The Lawmakers* are jam-packed with accounts of the words and actions and histories and hopes of real human-being politicians.[28] The way to beat that, if one is so inclined, is to disprove it on the merits of the evidence. Of course, that can be harder.

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[24] Barber, *Presidential Character*, pp. 441–442.

[25] "Barber's Typological Analysis," p. 210. Mr. Qualls now has the title of Bruce Mazlish's book *(In Search of Nixon)* right. Qualls cites no evidence at all that Nixon was correctly predicted by others. others.

[26] "Strategies for Understanding Politicians," *American Journal of Political Science*, 18 (Spring, 1974), 443–467.

[27] Whenever Qualls exhausts his own vocabulary of synonyms, he quotes George quoting Barber.

But if my assessments are so terribly biased, this ought to be easy enough for Mr. Qualls to prove. Does he think I have placed President X in the wrong category? Very well, what evidence can he find to contradict the evidence I present? Not being given to Quallsian flights, I will not "suggest" that the lack of such arguments here means it cannot be done. Considering how hard it was for Mr. Qualls to find the guilty bias, it just may be the bias is what does not exist. In any case, assertion proves nothing. If Mr. Qualls has facts, let's have them.

We can get an idea of how Mr. Qualls deals with facts by a case study. Beginning on p. 186, he slurs at my "Coding Scheme for Presidential Biographies," a mimeographed outline, used at an early stage of the research on presidents and subsequently sent to anyone who would pay the cost of production—including, must be, Mr. Qualls. Even in what he presents above, his folderol is perfectly clear. Distorting step #1 is to insert in brackets after the items in the coding scheme certain phrases he has picked up from *The Presidential Character,* particularly "[Low self-esteem]" and "[High self-esteem]". Now: having put together the little paragraphs from the coding scheme with the labels he imports from *The Presidential Character,* Mr. Qualls asserts that "Barber simply *assumed* rather than *proved* the relationship between the four particular strategies of adaptation and the levels of self-esteem he had associated with the strategies in his legislative study. Barber adjoined the criteria for each adaptive strategy with the criteria for the level of self-esteem he had associated with it in the earlier study."[29]

On the face of it, that's wrong. Qualls puts together A from one scheme and B from another and then damns Barber for putting A and B together. Barber didn't; Qualls did.

But suppose we give him a looser rein. Qualls is casting the broad aspersion that the Coding Scheme biases the coding toward confirming the clusterings I hypothesized. Unlike Qualls, I can prove his bias from the evidence.

First, it is not so that "the possibility of a president who, for example, emphasized 'service, principle, and responsibility' and esteemed himself quite highly was thus precluded."[30] Nor was this my "assumption in the guise of an empirical conclusion,"[31] except in Mr. Qualls's mind. Presidents were categorized according to activity and affect toward the role—two long *separate* and *independent* sections of the coding scheme, preceding those Mr. Qualls chooses to quote. Having placed a president tentatively, the researcher checks the evidence to see whether or not it supports the placement. Of course he could do that stupidly or blindly, but there is nothing in the Scheme that reinforces such tendencies. If he finds a president high on "service, principle, responsibility" he is to note that fact—whatever the concurrent evidence about self-esteem. By going on and on about "assessment," Mr. Qualls obscures the fact that these categories were for the compiling of evidence. Maybe the particular President would stress "service" but not "principle," maybe "responsibility" but not "service," maybe none of these, but elements from other categories. The graduate student assistants took the task seriously, as posed: code the *data* wherever they lead you. In the event, that's what the students did.

Having got the categorization scheme wrong, Mr. Qualls neglects to tell readers that the coders were also instructed to "review the entire work for evidence contradicting the major emergent themes."[32] This results, for example, in the paragraph on page 350 of *The Presidential Character* recounting occasions when Nixon was happy, according to the biographical sources. This category was much used to illustrate all sorts of exceptions to the developing rules.

Mr. Qualls read that item. He sees how directly relevant it is to his argument. And he leaves it out. In like manner, in his assumption that the codes I formulated biased the research, Mr. Qualls forgets this passage:

> XVI Interpretive notes.
>
> Items to be coded in this category are references to the codes above rather than to the

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[28] *The Presidential Character* is about William Howard Taft, Woodrow Wilson, Warren G. Harding, Calvin Coolidge, Herbert Hoover, Franklin D. Roosevelt, Harry S. Truman, Dwight D. Eisenhower, John F. Kennedy, Lyndon B. Johnson, Richard M. Nixon, and presidents yet to be.

[29] "Barber's Typological Analysis," pp. 188–189.

[30] *Ibid.*, p. 189.

[31] *Loc. cit.*

[32] James David Barber, "Coding Scheme for Presidential Biographies," (mimeographed, January, 1968), p. 6.

> pages of biographies. After a thorough review of all of the material you have coded, note here any ways in which you feel such a compilation of individual items may be especially misleading in any individual code. Give your own interpretation of how such errors should be corrected. The point is, that listings of individual items may leave an impression contrary to the truth. For example, by the casual interpretation of frequency as indicating intensities.[33]

Similarly he passes by "Residual Categories" in the Coding Scheme, in which are to be placed items that don't fit anywhere else, so that totally new and different themes and categories and colorations could be noted. The Coding Scheme is 39 pages long. Even without the "Residual Category," there was a lot of room for taking note for the complexities, and no reward at all for the gross oversimplifications Mr. Qualls accuses me of.

Long after the graduate student coders had gone on to greater glories, I went back over the biographies many more times, with all sorts of different ideas in mind. So they should not be tarred with the brush of my sinful nature. The book stands on its own bottom, but if Mr. Qualls wanted to get into that one old and incomplete part of the methodology, he might at least have handled it accurately. Had he been one of the student coders, Mr. Qualls would have been required to *read*, closely and carefully, and to *report* what he read accurately and completely.

Having demonstrated his partiality as a reader, Mr. Qualls goes on to demonstrate his ability as a writer, in constructing all sorts of fancy labels, charts, and distinctions, pretending they are Barber's, and then attacking them for the simplemindedness he has put into them. I doubt that even his godfather George would go along with the array Qualls calls "A Summary of Selected Criticisms." *I* surely would not. The labels have nothing at all to do with what I wrote. For example, clearly "style" in my scheme is a set of behaviors and also a set of determinants—but all examples would do, as the chart is simply an invention bearing no relationship to what it claims to summarize. Later Mr. Qualls wants to call worldview and style "companion constructs" supposedly to indicate that they are derived from character. They are not. The label is incorrect. Then we have "subconstructs," power situation and climate of expectations. In my scheme, they are not "sub" anything. The label is incorrect.

Table I, "Outline and Development of Barber's Theory of Presidential Character," is simply a way of pulling out a quote or two and reinforcing the reader's sense that dumb Barber thought that was all there was to it.

Mr. Qualls reaches the apex of his flight of chart-making fancy in Figure 1. I firmly believe he constructed this one by attaching phrases (some his, some mine) to darts and flinging them while blindfolded at the wall. Suffice it to say that the placement of my phrases and of the various solid, hatched single and double dotty lines has nothing in particular to do with the much more direct and straightforward presentation in the materials I wrote. The source is not Barber but Qualls himself. Qualls cannot get along without his "joint configuration," "paramount explanatory variables," "phenomenology of the prototypical construct,"[34] etc. That Figure 1 is a morass of confusion is no accident, being derived, as Mr. Qualls confesses, from this prose. One hopes he will learn to write and to draw better before he emerges with his Ph.D.

Throughout his diatribe, Mr. Qualls invents successive "Barbers" available, like voodoo dolls, for him to stick pins into. This is a way, all you potential publishers in the *APSR*, to "seem" original without having to work at it.

Quall's Third Mistake: Double-Barreled Reductionism. At least as early as page 183 of his piece, Mr. Qualls starts down the reductionist path. He is going to show how I reduce everything to character. Then he is going to show I am wrong to do that. Now what Barber actually says is that character type is "the most important thing to know about a President or candidate."[35] Barber's problem, in the face of various determined reductionists, is to keep that clear: "most important"—not *all*—important.

The Qualls buildup of character as the Rosetta stone of the Barber scheme begins with his noticing that character is accorded "heightened importance relative to style and worldview," a matter on which, despite the above, Mr. Qualls thinks I am "not explicit."[36] Now here is what Barber says:

> Character provided the main thrust and broad direction—but it does not *determine*, in any fixed sense, world view and style. The story of development does not end with the end of childhood. Thereafter, the culture one grows in and the ways that culture is translated by parents and peers shapes the meanings one makes of his character. The going world view gets learned and that learning helps channel character forces. Thus it will not necessarily be true that compulsive characters have reaction-

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[33] Barber, "Coding Scheme for Presidential Biographies," p. 39.

[34] "Barber's Typological Analysis," p. 193.

[35] Barber, *Presidential Character*, p. 6.

[36] "Barber's Typological Analysis," p. 183.

> ary beliefs, or that compliant characters believe in compromise. Similarly for style: historical accidents play a large part in furnishing special opportunities for action—and in blocking off alternatives. For example, however much anger a young man may feel, that anger will not be expressed in rhetoric unless and until his life situation provides a platform and an audience. Style thus has a stature and independence of its own. Those who would reduce all explanation to character neglect these highly significant later channelings. For beyond the root is the branch, above the foundation the superstructure, and starts do not prescribe finishes.[37]

Nevertheless, Reductionist Qualls charges on. By page 185 he has me in danger of committing what Father George has defined as the "cardinal sin" of *"psychological reductionism"*! By page 186 "Barber errs" in assigning a "critical role" to character. And by page 209, the process is complete: "Barber placed his eggs in the basket of personal needs." This is, purely and simply, nonsense. Character—and world view and style—are all dealt with at length for each of the eleven presidents, as important and predictable features of their performance.

Right there in front of you, on pages 7 through 11 of *The Presidential Character,* you can see a typology of styles, a typology of worldviews, a typology of climates of expectation, a typology of power situations, and even a typology of stages of character development—in addition to the character types. And it is plain foolish to accuse me of slighting worldview and style when I go on and on about Wilson's moralism, Harding's bloviating rhetoric, the "Johnson Treatment," Hoover's individualism and heavy speeches, Roosevelt's experimentalism, Nixon's isolation, and so on and so on.

Similarly Reductionist Qualls wants to get me in the box of neglecting the environments politicians experience—their situations—in favor of their characters. He will go so far as to say I "appear to take these variables into account," on page 207, but very shortly he lapses into paragraph after paragraph of silly sophistry regarding motivations, opportunities, and resources, as usual distorting a very simple and straightforward idea. In case Mr. Qualls has convinced anyone that I am fool enough to ignore environments, here is what I wrote, a dozen years ago, on this point:

> The particular strategies an individual employs depend on the special needs he brings to his political experience and the availability in that environment of satisfactions for these needs. If the needs are intense and the environment includes important sources of satisfaction, the pattern of adjustment will be modified or surrendered only in exceptional cases. To the extent that either of these factors is weak or absent, the pattern will tend toward flexibility and change. In turn, these patterns will have important effects on the legislative process insofar as they support or interfere with certain central functions that the legislature is called upon to perform.
>
> The word "environment" has a special meaning here—the subjective environment. Although an individual will not be totally unaware of the most obvious facts of the world around him, or perceive objects which are in reality totally absent, he does pay special heed to some things and neglects others. Furthermore, he tends to react emotionally to objects he encounters, attaching positive and negative signs to things. The particular selective and value-attaching practices a person employs amount to his reactions to the environment. Insofar as most of the real environment is common to all members of an institution, it is likely that differences in reactions are attributable to differences in the needs an individual brings to his new experience.[38]

So I did not take the personalistic approach Mr. Qualls attributes to me. Nor can he ignore, if he's honest, page after page in which legislators tell how they are relating to their environs. The same for presidents. *The Presidential Character* develops for each and every one of the eleven presidents on account of the power situation and the climate of expectations he confronted, how he related to those elements, how the interaction affected his approach, etc., etc. To say as Mr. Qualls does that Barber "dismissed" the "social and economic variables" regarding legislators is, again, not the case. Indeed I may go on too much about these matters; in *The Lawmakers* each type is discussed in these terms and the book's concluding chapter develops an extensive argument about the effects of community and occupational changes on the recruitment of legislators.

The next of Mr. Qualls's series of unfounded criticisms is that "the political environment," especially state party politics, "should have been examined" by Barber.[39] He goes on to lecture me about the Connecticut political system, with which I can claim some familiarity. What is the truth on this point?

Although *The Lawmakers* is described in its very first sentence as "a study of politics as a personal experience of the politician,"[40] Mr. Qualls is simply wrong in saying I ignored the political context. Thus:

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[37] Barber, *Presidential Character,* p. 11.

[38] James David Barber, *The Lawmakers* (New Haven and London, Yale University Press, 1965), p. 213.

[39] "Barber's Typological Analysis," p. 209.

[40] Barber, *Lawmakers,* p. vii.

> The legislators examined here shared significant characteristics. They were nominated and elected in the Connecticut political culture, with its relatively energetic, clean, responsible, and competitive parties, its peculiar blend of Yankee traditionalism and ethnic ticket-balancing, and its comparatively progressive record of social legislation. They entered a legislature in which party leadership is strong and party unity is the rule. Overcoming one of the worst cases of malapportionment in the nation, Connecticut's Democrats won a majority of the lower house in 1958 for the first time in 80 years. From this historical and political context the legislators drew a set of common attitudes and expectations.[41]

In every chapter, as nominations are discussed, and all through the concluding chapter, the pull and play of politics are there.

Nor does Mr. Qualls *seem* to have discovered, in his several years of attention to my work, that I wrote a whole complete article on party politics from these data: "Leadership Strategies for Legislative Party Cohesion."[42]

No, neither *The Lawmakers* nor *The Presidential Character* is vulnerable to these reductionist ploys. When Mr. Qualls makes such accusations, he is simply beating a nonexistent horse, wasting energies he might have devoted to some positive achievement.

Qualls's Fourth Mistake: Quantomania. Not even Mr. Qualls could mistake *The Presidential Character* for a statistical book, as there is not a table in it. *The Lawmakers,* however, does have some little profiles of simple characteristics at the first of each main chapter, six pages of questionnaire results back in the second of three appendices at the end of the book, and a few statistical footnotes. A great deal of Mr. Qualls's tirade is directed at these innocent numbers, as if they were the sum and substance of the 314-page book. Of course, they are not. In the book itself I said that they were not, and Mr. Qualls has, I suppose, read the very passages in which this truth is announced. Yet he plods on for page after page, berating the numbers and subjecting them to various tests and calculations, as if they were the realest thing available.

Since this whole line is out of kilter with the nature of the book, I am again forced back to quotation:

> I have explored such events primarily by asking some state legislators to tell me about them. It has helped, of course, to know a good deal about the character of the constituencies, the nature of the office, and the shape of the intervening party system. Statistics from official records and questionnaire replies have been compiled and interrelated in various ways. These materials provide a background (as it has been observed and recorded by others) and a numerical check of sorts (based, to be sure, on questionable questions). But I have emphasized the long, loosely structured interviews in which the legislators themselves discuss political events in their pasts, presents, and futures.[43] . . . Suffice it to say that the techniques of data-gathering and analysis used do not permit us to consider the results as more than suggestive hypotheses for further research. I shall not belabor this caution in the chapters to follow, but it should be borne in mind.[44]

Readers were reminded at the presentation of each of the profiles and of the statistical appendix that the numbers on which percentages were based are very small in some cases. And I did not require any Mr. Qualls to point out the speculative nature of the data. When he was still learning Platonistic ontology I wrote in *The Lawmakers:*

> The aim of this study has been to interpret a large mass of data bearing on the recruitment and adaptation of new members to legislative life. The results are speculative generalizations, not verified hypotheses. Among the many sources of error the reader should be aware of the following: Only 27 of 150 freshman legislators were interviewed, and only 83 and 96 respectively returned presession and postsession questionnaires. When these responses were divided into subcategories, some very thinly populated cells resulted. Neither the interview questions nor those in the questionnaires were pretested. The interviews were conducted in an exploratory fashion; thus no two were precisely alike. On the other hand, most of the questionnaire items were of the forced choice, check-mark type which permit the respondent no way to express his interpretation of them. The session during which the study was conducted was obviously an unusual one (see Chapter 5 for some of its peculiarities), and the Connecticut General Assembly contrasts in important ways with many other state legislatures (generally, Connecticut is a high income, urban, Eastern, industrial state with a strong, competitive two-party system at the state level). The selection of passages for quotation involved the writer's judgment, which may have erred considerably in estimating the typicality of responses. Evidence is presented below that the more active new members are overrepresented in the data, and no information on more experienced members is used. The interpretations reported here, then, should not be taken by those unfamiliar with problems of question-

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[41] Barber, *Lawmakers,* p. 17.

[42] "Leadership Strategies for Legislative Party Cohesion," *Journal of Politics,* Vol. 28 (1966), 347–367.

[43] Barber, *Lawmakers,* p. 15.

[44] *Ibid.,* p. 16.

> naire surveys, interviewing, statistical analysis, and the like as definitive empirical findings. There is little danger that the reader familiar with these arts will miss the methodological loopholes.[45]

Many of the questionnaire items Mr. Qualls puts so much stock in were crudely dichotomized answers to questions which, individually, could be interpreted in several different ways; collectively, I thought, they suggested (no more than that) some complements and supplements to the interviews. But the bald fact is that by the time I got around to publishing them (unlike the 17-year-old dissertation), I put all but the most obvious reports back in an Appendix to provide "a numerical check of sorts."

No, *The Lawmakers* is written primarily from interviews. The reader can perhaps judge whether he learns anything, beyond the little questionnaire replies, from such passages as the following:

> Well, of course, my father lived in this town all his life, and the town has been good to him and, well, he's been good to the town. And I thought to myself—of course, father's dead and all that—but I said to myself, Dad would say, "You've got the time, you ought to do it." So that's about the way I felt, that I was doing what was really set out for me to do, that I should do. I felt a, well, I felt duty bound to it, that's all. It wasn't a great hankering that I had . . . I thought I owed it to, to the years back, not—I know Dad would have told me to have done it, said, "You go ahead and do it," Dad would say, "Well, I'll get you by anyway." And he got me by, but I come by, thank the Lord, I got by myself.[46]

For the particular purposes I had in mind (and said I had in mind), the interview material was better treated narratively than statistically. If someone wants to break up those phrases and sentences into meaningfully countable units, I have no objection, as long as he does it fairly.

As for all Mr. Qualls's (unoriginal) scholasticism about validity and reliability, that, too, is simply vaporous posturing. Like his mentor, Qualls is given to "requiring" abstract criteria neither he nor any other researcher into these matters has ever met. Such hypocrisy is all too common in political science among those who avoid doing research by criticizing and proposing research for others to do.

The legislator types in *The Lawmakers* are similar to the presidential types in *The Presidential Character*. Of course this fact, borne out by the evidence, makes the rational reader more confident in the reality of the types than if the types were found in one population rather than in both populations. That is about all one need say about the matter—a modest claim of similarity, based on the evidence. All the fancy footwork Mr. Qualls displays cannot build this into a cause of controversy in any significant sense, nor can the poor Mr. Nunnally he quotes, whose system of "construct validity" is conspicuous by its absence in actual social research.

In the simple assessments of the degree to which legislators were active in legislative work (described plainly in the text), it is perfectly evident that high and low activity ratings are relative. Thus—lo and behold!—in a legislature or session of generally very high activity, the median divider would shift upwards; in lethargic legislatures and/or sessions, downward. Therefore it would be silly to stick to the same precise array of scores, session to session; *of course*, a 25 in one legislature might become a 45 in another. Only a quantomaniac would freeze onto the number itself as a particularly revealing talisman. In any legislature (probably in any group), motives, opportunities, and resources interact to produce action, and the participants sort themselves around the group mean, as high or low activists, in ways consistent with their personalities.

Again on the activity measure, Mr. Qualls, having no evidence of anything, wonders why I used comments on the floor, comments on committees, and bills introduced as indication of activity, neglecting such other variables as self-reported behind-the-scenes activity. Because, Mr. Qualls, the measures I used were the best available—dependent neither on my own nor on the legislators' selective perception. As your own Table 8 shows, self-reports of activity bear little relationship to the facts of activity as measured in my behavioral indices.[47]

Mr. Qualls is disturbed by the fact that there is some change, during the first session, in new legislators' intentions about returning to future sessions, and he thinks that shows instability in the measure. But of course. The words may sound the same when you ask a member, *before* he has had any experience at all, whether he wants to keep on with it, and when you ask him that after he has been laboring away at the job for several months—the words may stay the same but the meaning is profoundly different. It is something like asking you, Mr. Qualls, before you have done any original research, whether you think that will be your cup of tea, and asking you the same thing after you have tried your hand at real research.

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[45] Barber, *Lawmakers*, pp. 271–272.

[46] *Ibid.*, p. 141.

[47] That is about all Table 8 shows. Why Mr. Qualls would expect answers to the activity questions to correlate significantly with passive-negative affects (what he wants to call "attitude") is beyond me. But beyond that, the matter is absurd. Tau and Fisher's Exact must blush to be applied in this fashion. Tables 10, 11, and 12 are equally mysterious. I should have been very much surprised to find convincingly high correlations in this table. What Mr. Qualls is doing is giving the impression that I made predictions I did not make. We have a word for this in North Carolina.

Similarly in the interviews, when a respondent says X, and does not say Y or Z, then I think it is all right to say he *accentuates* X, rather than Y or Z.

And when a Lawmaker type advocates activism for others, he is expressing a value he very probably applies to himself.

And a response which in the context of one character pattern indicates aggression may in another pattern very well indicate nonaggressive activism. Similarly for the other little items Mr. Qualls lists and mocks as meaningless: You have to interpret the particular little phrases people use, Mr. Qualls, in the context of the rest of what they say. Otherwise you will leap so fast into quantomanic manipulation that you won't be able to *hear* what anybody is trying to tell you.

And: cutting back and forth between my Ph.D. dissertation and *The Lawmakers,* as it suits his convenience, Mr. Qualls has backed off from his earlier accusation that I was "ascribing any individuals' characteristics to the type and, in turn, any of those of the type to any of its members." Now he says that with some exceptions I do not indicate who says what. To get it clear: anything quoted as said by a respondent of a type was said by a respondent of that type. Anything quoted as said by one of the "pseudonymous" named individuals was said by him or her.[48]

And: amid all sorts of "appears to," "seems," "suggests" language, Mr. Qualls picks over what he wants to call an "achievement strategy" in the Advertiser interviews. How can I call talk touting achievement genuine for Lawmakers and roughly the same kind of talk spurious for Advertisers? Well, because Lawmakers are forever talking about the actual content and results of legislation; Advertisers respond like this:

> INTERVIEWER: Have you introduced bills?
> MIKE: Yeah.
> INTERVIEWER: What do you think their prospects are, for getting through this time?
> MIKE: I haven't even considered it. Not much interested in it. I think it was about fifteen bills I introduced. Some of them I was in accord with. There are two bills, three bills that I am really interested in, on X, Y, and Z. The others I don't care.[49]

And so on and so on. Mr. Qualls demonstrates, on page after page of such niggling nonsense, that he has not the slightest interest in the reality of the matter, but only in emerging as the destroyer of a study he cannot even read, much less understand. He is determined to transform plainness into mystery, as when he takes the simple and direct account of who returned questionnaires and who was interviewed in *The Lawmakers* and turns it into the terribly systematic-looking but actually trivial Tables 15 and 16 and Figure 2. Here is the little passage in the methods appendix of *The Lawmakers* that gives rise to all this speculation:

> 4. Selection of interview subjects. On the presession questionnaire, 94 first-term members indicated they were willing to be interviewed. These new members were divided into three categories: those answering "Definitely would"; "Probably would"; and "Definitely would not" or "Probably would not" to the questionnaire item on willingness to return. Ten potential interviewees were selected from each of these categories (numbering 37, 33, and 24 respectively) by random sampling within the category. Three of the prospective subjects were unable to arrange time for an interview, despite repeated efforts. In some cases, an interview was held after as many as four postponements. Insofar as possible, interviews were scheduled in series of three—one from each category—from late March through late June. In each of the four main chapters above, three interviews are explored in depth. These were selected by the writer to illustrate most clearly certain themes that appeared to typify the category. Quotations from the other interviews appear occasionally in the text or footnotes.[50]

I see this as a simple, commonsense approach any one-member graduate student research project might well adopt. For that is what "The Legislator's First Session" was. I was pleased when the Yale professors awarded it "distinction" as a dissertation, and more pleased when Yale University Press decided, on the basis of several outside evaluations, to publish it. But it was not the be-all and end-all, and it certainly was not the only strand leading, through years of further study and reflection, to *The Presidential Character.* Little did I suppose that some humorless junior Cromwell like Qualls would care to devote such inordinate energies to such off-the-mark criticisms.

As he huffs and puffs nearer and nearer the end of the track, Mr. Qualls's qualifiers drop away. Barber's approach "vitiates his conclusions" (p. 200), and his "construct exists only in the eye of the perceiver" (p. 206), and his "conclusions are speculative" (p. 210), and, at last, his manner of research will "produce accurate conclusions no more frequently than typing at random" (p. 211–not a bad description of what Qualls *appears* to have been doing).

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[48] As his paper progresses, Qualls grasps at weaker and weaker straws. He is not even above relying on an obvious misquote from an inexperienced newspaper reporter who gets the matter garbled in his scrambled eggs at a breakfast interview.

[49] Barber, *Lawmakers,* p. 99.

[50] *Ibid.,* p. 274.

Mr. Qualls is even prepared to "suggest" that this whole lousy theory of mine traces back to H. G. Gough's scheme, table-ized in Qualls's footnote 88–and thus that the theory is not only bad but copied! Elsewhere he says I owe a lot to Karen Horney. He might have said, if he were interested in conveying the truth of the matter, that these are two of *twenty-nine* very specific references to writings, in a wide variety of fields, elaborating similar and/or related types–not including the ancient and honorable theory of the four humors.

In the end, though, Mr. Qualls tells you outright that these two fat books of mine are merely "speculative."[51] That *seems* to say that they do not exist or that they are works of fiction. Well, there they are; the facts in them, including what the politicians have said, do exist and cannot be wished away.

What is sad about this matter is not so much the accusations Mr. Qualls makes. They appear here only as a result of an accident of prejudice and neglect. No honest and attentive reader of these books is likely to be swayed by Mr. Qualls's fulminations. Nor is it–I hope!–to be a continuing practice of this journal to waste its precious space on articles such as this. The process that is harder to turn around is the grim trivialization of political science by naive technicians. The behavioral impulse was much needed, after the Second War, to urge political science out into the field again, armed with the best methods we could find, to attack the basic problems of actual political life. That impulse continues to have good results, because most practitioners of the craft care about the improvement of government and bring to their labors a realistic sense of how research might contribute to that cause.

But scattered among us, in no intended conspiracy, are little backwaters of blindness in which political know-nothings paddle around in circles. These are the apolitical scientists who, enthralled with technique, cannot see past technique to the values it might serve. When they produce anything, it is the elegant demonstration of useless propositions. More often, the naive technicians, urged along by less competent gurus who want to see themselves as standard-setters, thrash about in the critical mode, creating nothing but a false sense of the possibilities of perfection, and in the process, wasting a lot of valuable time.

What is happy about this matter is that, in addition to the many political scientists and other scholars who have so generously appreciated my efforts in these lines of research and have been so helpful in correcting and advancing it, now an increasing number of important political actors are using those ideas to assess and predict candidates. For example, such thoughtful and effective journalists as David Broder, Anthony Lewis, Hugh Sidey, and Bill Moyers have found *The Presidential Character* worth hard pondering, and more important, have drawn on their own special talents and sources to apply and extend its argument. As this is written, the presidential primaries are in full swing; more and more of the writing within and about that process includes attention to the relations between life histories and the presidential task. All of this may have produced in Mr. Qualls nothing more than "great sighs." In the rest of us, it is producing the hope of building a bulwark against tyranny.

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[51] "Barber's Typological Analysis," p. 211.

An Optimal Control Model of Arms Races*

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Before presenting his model of an armaments race, Lewis Frye Richardson argued briefly with a hypothetical critic who was concerned with the plausibility of a science of foreign policy. Responding to the critic's questions, Richardson explained the limitations of his model: "The equations are merely a description of what people would do if they did not stop to think. . ."[1] Although frequently misunderstood, Richardson's response to his critic highlights one of the central difficulties with his arms race model: it does not contain any decision calculus. While Richardson's equations can be interpreted as describing how nations will react to one another in an arms race, they do not explain how the nations arrived at such a strategy. Thus Richardson's equations are not, as some have misinterpreted, a model of irrational behavior; instead, as Rapoport, Alker and others have pointed out, the model is a-rational in the sense that it does not explain how nations come to adopt their particular reaction equations. Thus to answer critics like Rapoport and Alker, it is necessary to extend the Richardson model in such a way that a decision calculus involving national goals becomes part of the theoretical structure.[2]

The work of several scholars has pointed toward incorporating national goals into the Richardson process model.[3] Brito was probably one of the first to provide a general mathematical statement of national goals used in conjunction with the Richardson model.[4] Brito demonstrated that the Richardson equations could be derived from a generalized quadratic function representing the goals of a nation. Subsequently Brito and Intriligator expanded on Brito's earlier analysis by incorporating technological and strategic weapons considerations of a nuclear war in a generalized form derived from Intriligator's previous work on strategic questions of rates of fire and targeting.[5] Simaan and Cruz extended the Brito-Intriligator model by considering an arms race as a differential game between two arming nations, each having national objectives of a general quadratic form.[6]

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*Support for this research was supplied by the National Science Foundation, Research Grant GS-36806. The authors wish to express appreciation to Professors Jose B. Cruz, Jr. (University of Illinois), Dagobert Brito (Ohio State University), Michael Intriligator (University of California, Los Angeles), and I. W. Sandberg (Bell Laboratories) for their helpful suggestions and valuable assistance. A previous draft of this paper was presented at the 1974 meetings of the Midwest Political Science Association, Chicago, Illinois.

[1] Lewis Frye Richardson, *Arms and Insecurity* (Pittsburgh: Boxwood Press, 1960), p. 12.

[2] Hayward R. Alker, Jr., "The Structure of Social Action in an Arms Race" (paper prepared for delivery at the 1968 Conference of the North American Peace Research Society (International), Cambridge, Massachusetts, November, 1968); Anatol A. Rapoport, "Lewis F. Richardson's Mathematical Theory of War," *Journal of Conflict Resolution*, 1 (September, 1957), 249–299, and *Fights, Games and Debates* (Ann Arbor: University of Michigan Press, 1960), pp. 15–30.

[3] Numerous scholars have attempted to examine the degree of empirical fit between the Richardson model or modified Richardson models and armaments races or have suggested reformulations of Richardson's basic model. See, for example: N. Z. Alcock and Keith Lowe, "The Vietnam War as a Richardson Process," *Journal of Peace Research*, 6 (April, 1969), 105–112; Avron A. Blumberg, "Model for a Two-Adversary Arms Race," *Nature*, 234 (November 19, 1971), 158; William R. Caspary, "Richardson's Model of Arms Races: Description, Critique, and an Alternative Model," *International Studies Quarterly*, 11 (March, 1967), 63–88; P. E. Chase, "The Relevance of Arms Race Theory to Arms Control," *General Systems Yearbook*, 13 (1968), 91–98, and "Feedback Control Theory and Arms Races," *General Systems Yearbook*, 14 (1969), 137–149; Manas Chatterji, "A Model of Resolution of Conflict between India and Pakistan," *Peace Research Society Papers*, 12 (1969), 87–102; John Ferejohn, "On the Effects of Aid to Nations in Arms Races" (paper delivered at the Annual Meeting of the American Statistical Association, December, 1970); Mats Friberg and Dan Jonsson, "A Simple War and Armament Game," *Journal of Peace Research*, 5 (July, 1968), 233–247; Michael D. Intriligator, "Some Simple Models of Arms Races," *General Systems Yearbook*, 9 (1964), 143–147; Yu. P. Ivanilov, "A Model of Competition between Two Countries," *Engineering Cybernetics*, 1 (Jan.-Feb., 1969), 19–30; Robert H. Kupperman and Harvey A. Smith, "Strategies of Mutual Deterrence," *Science*, 176 (April 7, 1972), 18–23; John C. Lambelet, "A Dynamic Model of the Arms Race in the Middle East, 1953–1965," *General Systems Yearbook*, 16 (1971), 145–167, and "Towards a Dynamic Theater Model of the East-West Arms Race," *Journal of Peace Science*, 1 (Autumn, 1973), 1–38; Knut Midgaard, "Arms Races, Arms Control, and Disarmament," *Cooperation and Conflict*, 5 (February, 1970), 20–51; Kendall D. Moll, *The Influence of History Upon Seapower, 1865–1914* (Menlo Park, Cal.: Stanford Research Institute, September, 1968); Barry O'Neill, "The Pattern of Instability among Nations: A Test of Richardson's Theory," *General Systems Yearbook*, 15 (1970), 175–181; George W. Rathjens, "The Dynamics of the Arms Race," *Scientific American*, 220, no. 4 (April, 1969), 15–25; Paul Smoker, "A Mathematical Study of the Present Arms Race," *General Systems Yearbook*, 8 (1963), 61–76, "Fear in the Arms Race: A Mathematical Study," *Journal of Peace Research*, 1 (Febru-

The work of Brito, Intriligator, and Simaan and Cruz has been of considerable importance in meeting one of the obvious and important objections to the original Richardson model.[7] These analyses, however, have been very general. Brito did not examine specific goals that nations might have, but rather investigated the general mathematical form that such goals might take. Such a general form can of course represent a large number of different types of national goals. Similarly, the Simaan-Cruz solutions to their differential game formulation are for a general quadratic function. Thus these previous analyses provide a framework within which we can begin to interpret specific national objectives once more specific statements of objectives are formulated. Indeed, until such formulations are provided we cannot begin to explore the specific consequences of models for the nations involved in armaments races and the international system as a whole. Furthermore, we cannot begin to determine whether the models provide any sort of explanation for the empirically observable world of international politics and foreign policy. The purpose here, then, is to consider a particular national goal—a goal very typically expressed in the foreign policy literature—and to determine the consequences of this goal in terms of the armaments policy a nation would pursue to satisfy the goal. A comparison of the policy derived from the model with the policies actually pursued by nations involved in armaments races will then provide an indication of the extent to which the model is relevant to the empirical world. It will also be possible to examine certain properties of the model, such as the point at which the rate of change in the armaments policy of a nation will be steady (steady state equilibrium) and the extent to which the policy pursued is stable through time.

In the next section we will present the general defining ingredients of an optimal control model. These somewhat abstract and mathematical concepts must be presented before working with the Richardson model in order that the reader can understand how the Richardson arms model has been altered. Having discussed the main characteristics of the optimal control approach and sketching the form of its solution, we will present the modified Richardson arms model, derive its solution and analyze some of the properties of the solution. In the remaining two sections we will empirically assess the model using arms expenditure data from the United States, the Soviet Union, the North Atlantic Treaty Organization (NATO) countries, the Warsaw Treaty Organization (WTO) countries, Israel, and the Arab states. Finally we will discuss the equilibrium and stability properties of the model.

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ary, 1964), 55–64, "Trade, Defense and the Richardson Theory of Arms Races: A Seven Nation Study," *Journal of Peace Research,* 2 (April, 1965), 161–176, "Nation State Escalation and International Integration," *Journal of Peace Research,* 4 (February, 1967), 61–75. "The Arms Race As an Open and Closed System," *Peace Research Society Papers,* 7 (1967), 41–62; D. L. Wagner, R. T. Perkins and R. Taagepera, "Fitting the Soviet-U.S. Arms Race Data to the Complete Solution of Richardson's Equations" (paper presented at the 1974 Meetings of the Western Regional Peace Science Society); Dina A. Zinnes and John V. Gillespie, "Analysis of Arms Race Models: USA vs. USSR and NATO vs. WTO," in *Modeling and Simulation,* vol. 4, ed. William G. Vogt and Marlin H. Mickle (Pittsburgh: Instrument Society of America, 1973), pp. 145–148; Murray Wolfson, "A Mathematical Model of the Cold War," *Peace Research Society Papers,* 9 (1968), 107–124. Some scholars have suggested goals for nations involved in various specific aspects of armaments races. See Martin C. McGuire, *Secrecy and the Arms Race* (Cambridge, Mass.: Harvard University Press, 1965), and Thomas L. Saaty, *Mathematical Models of Arms Control and Disarmament* (New York: John Wilay and Sons, 1968).

[4] Dagobert L. Brito, "A Dynamic Model of an Armaments Race," *International Economic Review,* 13 (June, 1972); Dagobert L. Brito and Michael D. Intriligator, "Some Applications of the Maximum Principle to the Problem of an Armaments Race," in *Modeling and Stimulation,* vol. 4, pp. 140–144, and "An N-Country Model of an Armaments Race" (mimeoed, 1974).

[5] Michael D. Intriligator, *Strategy in a Missile War: Targets and Rates of Fire* (Los Angeles: University of California, Los Angeles, Security Studies Project, Paper #10, 1967), and "The Debate over Missile Strategy: Targets and Rates of Fire," *Orbis,* 11 (Winter, 1968), 1138–1159.

[6] Marwan Simaan and José B. Cruz, Jr., "A Differential Game Formulation of Arms Race and Control and Its Relationship to Richardson's Model" (mimeoed, 1973), and "A Multistage Game Formulation of Arms Race and Control and Its Relationship to Richardson's Model," in *Modeling and Simulation,* vol. 4, 149–153.

[7] Other objections have been raised about the Richardson model. See Alker, "Structure of Social Action"; Rapoport, "Richardson's Mathematical Theory"; and Caspary, "Richardson's Model."

The Optimal Control Theory Approach

The form of analysis to be pursued here is generally placed under the rubric of optimal control theory or systems, optimization with controls, or optimization in feedback systems, or a similar title. Optimal control methods developed by mathematicians and theoretical engineers have advanced rapidly with the development of space exploration and the need for mathematical systems to solve transport and targeting problems. Control methodology is occasionally applied in economic and business analyses, but for the most part social scientists have ignored these valuable tools.[8]

The variables in an optimal control problem are either *state variables* (denoted by x, y, or z) or *control variables* (denoted by u or v). The values of the state variables are determined by the dynamic process being modeled, while the values for the control variables are selected by the decision maker with the intent of optimizing specified goals. It is generally assumed that the state variables $(x(t))$, the control variables $(u(t))$, and time compose a complete and closed system which is described by a set of differential equations of the form:

$$\dot{x} = F(x(t), u(t), t), \tag{1}$$

where F is generally some linear function and $\dot{x}$ (read dx/dt) is the rate of change in X. Given equation (1), the state and control variables at any point in time completely determine the rate of change in the system. Given the state of the system at some initial time t_o and the choice of control variables as a function of time, $u(t)$, the whole course of the system is determined for the finite time horizon $[t_o,T]$. The general statement of the differential equation given in (1) is usually known as the *plant.* It describes the dynamics by which inputs (values of the control variables) are translated into outputs (values of the state variables), often incorporating feedback.

We are interested in how the system evolves through time $[t_o,T]$. At any moment in time the system is in some *state;* that is, some particular configuration of relationships exists between the elements x, u, and t of the system. Our problem is to control the system as it moves through time.[9] Controlling the system means that through the manipulation of the control variable, $u(t)$, it is possible to make the system respond in a particular way. Thus by selecting different values for the control variables over time, a set of future "histories" or behaviors can be built for the system. The problem is to choose between the essentially infinite possible future histories which can be generated by selecting the "appropriate" values for the control variable $u(t)$. By "appropriate" it is meant that the choice of the values of the control variable should be governed by some objective or goal that the decision maker is attempting to achieve. The objective represents the desired yield from the system at each moment in time and is expressed as a function of the state variable, control variable and time:

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[8] The most useful explanations of optimal control theory are Michael Athans and Peter L. Falb, *Optimal Control* (New York: McGraw-Hill, 1966); Arthur E. Bryson, Jr. and Yu-Chi Ho, *Applied Optimal Control* (Waltham, Mass.: Ginn and Co., 1969); Stephen J. Citron, *Elements of Optimal Control* (New York: Holt, Rinehart and Winston, 1969); Jose B. Cruz, Jr., ed., *Feedback Systems* (New York: McGraw-Hill, 1972); Peter Dyer and Stephen R. McReynolds, *The Computation and Theory of Optimal Control* (New York: Academic Press, 1970); Jay C. Hsu and Andrew U. Meyer, *Modern Control Principles and Applications* (New York: McGraw-Hill, 1968); Katsuhiko Ogata, *Modern Control Engineering* (Englewood Cliffs, N.J.: Prentice-Hall, 1970); William R. Perkins and Jose B. Cruz, Jr., *Engineering of Dynamic Systems* (New York: John Wiley and Sons, 1969); Pravin P. Varaiya, *Notes on Optimization* (New York: Van Nostrand Reinhold, 1972). See also: Kenneth Arrow, *Applications of Control Theory to Economic Growth* (Stanford, Cal.: Institute for Mathematical Studies in Social Science, Technical Report #2, Stanford University, July, 1968); Robert Dorfman, "An Economic Interpretation of Optimal Control Theory," *American Economic Review,* 10 (December, 1969), 817-831; Michael D. Intriligator, *Mathematical Optimization and Economic Theory* (Englewood Cliffs, N.J.: Prentice-Hall, 1971); John V. Gillespie, "Optimal Control Theory and Comparative Foreign Policy: A Promising Approach to Future Research," in *In Search of Global Patterns,* ed. James N. Rosenau (New York: Free Press, forthcoming) and "Some Observations on Dynamic Modeling," in *Measurement and Research Design in Political Science,* ed. David Leege (Chicago: Aldine, forthcoming).

[9] Not all models in which manipulable variables appear are controllable. In general, if the desired state is specified for all time, the requirements for the existence of a control input that will generate the desired $x(t)$ are very stringent. A less ambitious but more realistic goal is to require only a partial specification of the state variables. One such partial specification is forcing the state of the system to attain a specified value at some finite time in the future. That is, given an initial time, t_o, an initial state $x(t_o) = x_o$, and a final state x_f, $t_o \leqslant t \leqslant t + T$, for some finite T, such that $s(t_o + T) = x_f$, there may or may not be any control, $u(t)$, which can force the system to attain the state x_f. By a suitable choice of error coordinates such as state variables, the problem of reaching and maintaining a specified state is the problem of matching a desired dynamic response. A system is said to be completely state-controllable if, for any initial x_o, for any initial time t_o, it is possible to generate an unconstrained control vector, $u(t)$, that will take any given initial state $x(t_o)$ to any final state $x(t_f)$ in a finite time interval $t_o \leqslant t \leqslant t_f$. For a linear system, the final state may be taken to the origin without any loss of generality. The model we have developed here is completely state-controllable.

$$J(x, u, t), \tag{2}$$

where J is the objective function to be achieved. Thus for a finite time horizon, the general statement of the optimization problem is to find a particular $u(t)$, call this $u^*(t)$, which will optimize:

$$J = \int_{t_o}^{T} L[x(t), u(t), t]\,dt, \tag{3}$$

subject to the plant equation (1) and possibly other conditions. The use of the integral in (3) indicates that the decision maker wants to optimize over the entire time period from t_o to T. The equation (3) is commonly referred to as a *performance index* or *objective function.*

Summarizing, a control problem has essentially three parts: (a) an objective function of the form given in expression (3); (b) a plant of the form in expression (1), and (c) a set of initial conditions and possibly additional constraints on the values of variables and parameters through time or at the initial or terminal time points. Generally speaking, analytical solutions can be found to control problems if the objective function is quadratic in form and the plant is a linear system. Control problems which do not satisfy these conditions can on occasion be solved, but the solutions are more complex and frequently require the use of numerical estimation techniques. Next let us consider how the quadratic-linear control problem is solved.

Drawing on the work of Bellman, the Soviet mathematician Pontryagin showed that the general problem defined by (1) and (3) could be solved by defining a function known as the Hamiltonian, named after the nineteenth-century English physicist.[10] In effect, the Hamiltonian function combines the objective function and the plant equation through use of special auxiliary variables, also functions of time.[11] These auxiliary variables are known as the *adjoint vector* and are denoted by $p(t)$. Since the Hamiltonian is a function of both the objective function and the plant equation together with the new adjoint vector, it can be written in its general form as:

$$H(x(t), u(t), p(t), t). \tag{4}$$

Specifically, the Hamiltonian is:

$$H(x, u, p, t) = L(x, u, t) + {} + p^T F(x, u, t), \tag{5}$$

where p^T is the transpose of the adjoint vector and p satisfies

$$-\dot{p} = \partial H / \partial X. \tag{6}$$

It is important to note that the adjoint vector $p(t)$, which allows for the combination of the objective function and plant equation, *must* satisfy the partial derivative in equation (6). The optimal control, $u^*(t)$, can be obtained from equations (5) and (6) by observing that from (5) we can write:

$$\frac{\partial H}{\partial u} = 0. \tag{7}$$

In summary, the procedure to be followed is to form the Hamiltonian (5), and with expressions (6) and (7) together with the initial conditions, solve for $u^*(t)$, the *optimal strategy*. Having obtained the optimal strategy, it is then possible to substitute the expression for $u^*(t)$ into the plant equation (1) to obtain the *optimal trajectory.*

We now turn to a consideration of the Richardson arms model as an optimal control problem.

The Richardson Model as an Optimal Control Problem

The Richardson arms race model is a simple coupled pair of linear differential equations in two variables:[12]

$$\dot{x} = ly(t) - bx(t) + h \tag{8a}$$

$$\dot{y} = kx(t) - ay(t) + g \tag{8b}$$

In Richardson's equations x and y are the armaments (generally measured in terms of defense expenditures) of two antagonistic nations X and Y; $\dot{x}$ and $\dot{y}$ are the rates of change through time of the armaments; and k, l, a, b, g and h are constants. Richardson reasoned that arms races could be explained by three factors: (1) threat from one opponent (the terms $ky(t)$ and $lx(t)$ where k and l are positive constants across the time horizon $[t_o, T]$); (2) economic burden and fatigue (the terms $-ax(t)$ and $-by(t)$, where a and b are positive constants); and (3) grievance (the positive constants g and h).

To apply optimal control theory to Richardson's arms race model it is necessary to reformulate the original Richardson equation given in (8). Let us assume we have some nation U (this notation is selected to avoid confusion with Richardson's model) in an arms race with another nation X. Nation X behaves as modeled by Richardson (equation (8a)). Nation U, knowing the behavior of nation X, desires to behave in a way that satisfies its objectives. In some senses we are removing equation (8b) from the Richardson model and are substituting nation U's objective function in its place. Nation U for our model is Richardson's nation Y.

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[11] By obtaining an optimal trajectory in this way we are incorporating feedback into the model. Feedback in our case will allow countries to respond to the foreign policy decisions of other countries. A solution which incorporates feedback is a closed-loop solution. It is closed-loop in the sense that the output from the model is an input at a later time in the dynamic process.

[10] Richard E. Bellman, *Dynamic Programming* (Princeton, N.J.: Princeton University Press, 1957); L. S. Pontryagin, V. G. Boltyanskii, R. V. Gamkrelidge and E. F. Mishchenko, *The Mathematical Theory of Optimal Processes,* trans. K. M. Trirogoff (New York: Wiley Interscience, 1962).

[12] Richardson, p. 16.

The objective for nation U needs to be specified. One such objective that has commonly been discussed in the literature involves the "balance of power."[13] The balance of power from the perspective of a single nation involves two components. First, the decision makers of a given nation, in this case nation U, desire to maintain a sufficient level of armaments to offset the armaments of their potential enemy, in this case nation X. Second, nation U's decision makers also desire not to overindulge in armaments to the degree that arms monopolize governmental effort, and likewise encourage nation X to increase its arms. In other words, the objective behind the balance of power from the perspective of a single nation is to maintain sufficient armaments to assure a desired level of national security *and* to restrain the armaments of the potential enemy by attempting to minimize the total armaments possessed by the two antagonists. These two component parts of the balance of power from a single nation's perspective are somewhat contradictory. If nation U increases its armaments to satisfy its desired level of national security—hence meeting the first part of its objective, it also increases the total amount of armaments possessed by the two antagonistic nations—hence not meeting the second component of its objective. Likewise if nation U reduces its armaments in an attempt to diminish the total arms held by both antagonists satisfying the second part of its objective, nation U's decision makers may endanger their perceived level of armaments needed for national security. In sum, the balance of power (or arms balance) objective argues for two somewhat contradictory goals: maintaining national security and at the same time also containing arms proliferation among the arming powers.

The first part of nation U's objective can be stated mathematically as:

$$J_1 = \int_{t_o}^{T} [(u(t) - ax(t))^2] dt, \tag{9}$$

where a is the proportion of X's arms which nation U perceives it needs to maintain national security and u and x are armaments of nations U and X respectively. In expression (9) nation U desires to select a level of armaments, $u(t)$, such that J_1 is minimized. The minimization of J_1 is achieved when $J_1 = 0$ and hence when there is a perfect arms balance between the two antagonists, nations U and X, from the perception of nation U.

The second part of nation U's objective can be stated mathematically as:

$$J_2 = \int_{t_o}^{T} [c\ (u(t) + x(t))]\ dt, \tag{10}$$

where c is the significance nation U places on the total arms held by both nations U and X. Once again nation U desires to select a level of armaments, $u(t)$, such that J_2 is minimized. J_2 is minimized when $J_2 = 0$ and hence when there is no significant arms proliferation from the perspective of nation U.

In equation (9) it might be argued that the constant a must be greater than 1.0. Such a strict constraint is unwarranted, however, because a nation's decision makers may perceive their armaments level need not be greater than that of the opposing nation. To assure a sufficient arms level for security concerns, a nation's decision makers may only require that for increases in their opponent's arms level, the response must be proportional to the opponent's increase. Especially in the world of strategic weapons and potential overkill by antagonistic nations, proportional response rather than "greater than" response, may be an appropriate strategy to assure sufficiency in armaments.

Some might argue that the constant c in equation (10) must be positive. This constant c refers to the degree of significance attached to total arms owned by the two antagonistic nations. In many arms races, however, there may be a lack of concern with arms proliferation. National decision makers may argue that as long as total armaments do not reach a certain level, the balance of power can be preserved. Likewise national decision makers may place such extraordinary emphasis on the constant a in (9) that there is little concern for arms proliferation.

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[13] See, for example: Dina A. Zinnes, "An Analytical Study of the Balance of Power Theories," *Journal of Peace Research*, 4 (July, 1967), 270–288, and "Coalition Theories and the Balance of Power," in *The Study of Coalition Behavior*, ed. Sven Groennings, E. W. Kelley and Michael Leiserson (New York: Holt, Rinehart and Winston, 1970), 351–368. See also: Inis L. Claude, Jr., *Power and International Relations* (New York: Random House, 1962), 11–93; A. F. Pollard, "The Balance of Power," *Journal of the British Institute of International Affairs*, 2 (March, 1923), 53–64; Ernst B. Haas, "The Balance of Power: Prescription, Concept, or Propaganda?" *World Politics*, 5 (July, 1953), 442–477.

A simple examination of statements (9) and (10) quickly yields the conclusion that the two goals are contradictory and this is wherein the problem lies. The decision makers of nation U have somewhat conflicting goals and their problem is how to optimally satisfy both goals in combination. To formulate nation U's problem, statements (9) and (10) can be combined as in expression (11) for the overall objective of nation U.

$$J = \int_{t_o}^{T} [(u(t) - ax(t))^2 + c(u(t) + x(t))]\,dt. \qquad (11)$$

In combining equations (9) and (10) into (11), it will be noted that the first part of nation U's objective is squared, whereas the second part is not. Since nations by virtue of the very fact of being involved in an arms race are more likely to place emphasis on sufficiency of their armaments than on arms proliferation, the first part of the expression has been squared, whereas the last part has not been. As a result, differences in armaments between nations U and X are more important to the decision making of "arms balancing" than is arms proliferation.

The solution for nation U is to find the optimal value of $u(t)$, called $u^*(t)$, such that J in equation (11) is minimized. Expression (11) is the objective function of our optimal control model. It will be noted that its structure is quadratic and that in accordance with previously mentioned requirements for finding solutions to control problems, with a linear plant, a solution should exist to satisfy nation U's objective.

Recalling the original Richardson model given in (8a) and (8b), one can see that the second equation of the model specifying the behavior of nation Y has been replaced by the objective function given in (11) and that the variable y has, in our new problem, become the control variable u. As noted above, u is a control variable because its value is chosen by the decision makers of nation U who are attempting to determine the appropriate amount of armaments given their objectives postulated in (11).[14]

To complete the construction of the control model it is now necessary to alter equation (8a). The most obvious change is to substitute the variable u for the variable y:

$$\dot{x} = lu(t) - bx(t) + h. \qquad (12)$$

Equation (12) in control terminology is the plant or dynamic system. The equation describes how the control inputs $(u(t))$ are transferred into outputs $(x(t))$. It is also important to note that equation (12) also has a somewhat different interpretation than was previously the case in the Richardson model. In the present control model, equation (12) represents nation U's perception or interpretation of how nation X behaves. In other words, (12) is a statement of how nation U believes nation X will respond to U's armaments. The combination of equations (11) and (12) can be thought of as mathematical representation of U's total decision problem: the objectives U wishes to achieve (equation (11)), given the kind of world in which U perceives it is operating (equation (12)). Since the original Richardson model was an attempt to model *in fact* how two nations respond to one another's armaments, the model (8a and 8b) can be considered to be an interactive model describing an arms race system. By constructing a control model based on Richardson's formulation, we have in fact converted the systemic model of arms races into a decision-making model involving the objectives of foreign policy decision makers.

The optimal control problem formulated here can be summarized as follows: Find $u^*(t)$ (the optimal control) which minimizes the objective function (11) subject to the plant (12) under the assumption that the initial conditions $(x(t_o))$ are known, where t_o is the initial time. It should be noted that expression (12) is a linear differential equation, thus meeting the second requirement set forth above for finding analytic solutions to control theory problems.

Because it is always helpful to construct a block diagram of control problems,[15] we depict in Figure 1 a block diagram of equations (11) and (12), showing the dynamics of the control decision-making model, including the control inputs, feedback and systems response contained in (11) and (12).

The solution to our optimal control problem can be obtained by following the steps outlined in the previous section. Forming the Hamiltonian function and suppressing time to simplify notation, we obtain

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[14] Although the variable $u(t)$ is manipulable by the decision maker, if and only if the model is completely state-controllable is it the case that a manipulable variable becomes a control variable. For our model the condition of controllability is satisfied. We are assuming that the variable $u(t)$ is completely manipulable, i.e., in principle, any values of $u(t)$ can be chosen.

[15] For block diagramming and network methods, see Perkins and Cruz, pp. 11–94.

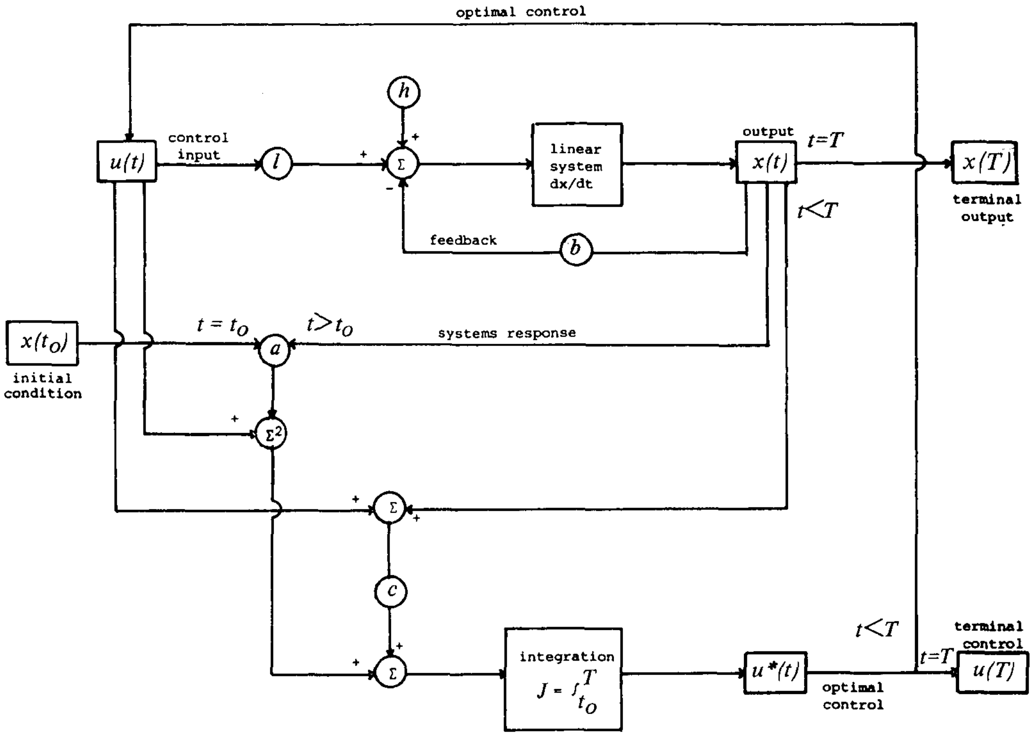
optimal control
h
control input
u(t)
l
+
Σ
+
-
linear system dx/dt
output
x(t)
t=T
x(T)
terminal output
t<T
feedback
b
x(t₀)
initial condition
t = t₀
t>t₀
a
systems response
+
Σ²
+
Σ
+
c
+
Σ
+
integration
J = ∫ T t₀
t<T
u*(t)
optimal control
terminal control
t=T
u(T)

Figure 1. Block Diagram of Optimal Control Model

$$H(x, u, p) = [(u - ax)^2 + c(u + x)] + p(lu - bx + h), \qquad (13)$$

where $p^T = p$ because the plant equation is unidimensional. Using the properties as explained above in (6) and (7), we can write:

$$\partial H/\partial u = 2(u - ax) + c + pl = 0 \qquad (14)$$

$$\dot{p} = -\partial H/\partial x = 2a(u - ax) + pb - c. \qquad (15)$$

From (14) we obtain expression (16).

$$u = (2ax - pl - c)/2. \qquad (16)$$

For the linear plant (12) and the quadratic index (11), we can solve the problem more easily by making the following assumption.[16] Let

$$p(t) = k_1(t)\, x\,(t) + k_2(t), \qquad (17)$$

which when differentiated yields (18):

$$\dot{p} = k_1 \dot{x} + \dot{k}_1 + \dot{k}_2, \qquad (18)$$

where t has been repressed for notational simplicity. Substituting (15) and (17) into (18) we obtain

$$\dot{p} = (bk_1 - alk_1)\, x + (bk_2 - alk_2 - ac - c). \qquad (19)$$

But using (12), (16) and (18) we also obtain

$$\dot{p} = (k_1 la - (k_1{}^2 l^2)/2 - bk_1 + \dot{k}_1)\, x + (\dot{k}_2 + k_1 h - (k_1 lc)/2 - (k_1 k_2 l^2)/2). \qquad (20)$$

It is true that

$$0 = \dot{p} - \dot{p}. \qquad (21)$$

Substituting from (19) and (20) for the $\dot{p}$ expressions in (21), and collecting like terms we obtain

$$0 = (2k_1 la - 2k_1 b - (k_1{}^2 l^2)/2 + \dot{k}_1)\, x + (k_2 la - bk_2 + ac + k_1 h - (k_1 lc)/2 - (k_1 k_2 l^2)\,/\,2 + c + \dot{k}_2). \qquad (22)$$

Expression (22) is of the general form:[17]

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[16] Our analysis makes use of the Ricatti equations whereby the equations allow decomposition because certain terms must equal zero. The general form of our model is the "regulator problem with a linear system and quadratic cost." See Varaiya, pp. 176–184.

[17] For equation (23)

$$A = (2k_1 la - 2k_1 b - (k_1{}^2 l^2)/2 + \dot{k}_1)x$$

$$B = (k_2 la - bk_2 + ac + k_1 h - (k_1 lc)/2 - (k_1 k_2 l^2)/2 + c + \dot{k}_2)$$

$$0 = Ax + B. \tag{23}$$

One solution which satisfies (23) is $A = 0$ and $B = 0$. Consequently we can obtain two expressions, one for $\dot{k}_1$ and another for $\dot{k}_2$ by letting $A = 0$ and $B \approx 0$ in (23):

$$\dot{k}_1 = 2(b - la)\, k_1 + (l^2/k_1{}^2/2) \tag{24}$$

$$\dot{k}_2 = (b - la)\, k_2 + (l^2 k_1 k_2)/2 + [(lc/2) - h]\, k_1 - (a + 1)\, c. \tag{25}$$

At the terminal time T, $p(T)$ must equal 0.[18] Since the auxiliary variables contain the constraints, this boundary condition must be satisfied for our problem. Hence we can write following (17):

$$p(T) = k_1(T)\, x\, (T) + k_2(T) = 0. \tag{26}$$

Examining (24) we see that one solution is to take $k_1\ (t) = 0$, k_1 being the zero function because it is not dependent on $x(t)$ and $k_2\ (T) = 0$, k_2 evaluated at $t = T$.[19]

Therefore, $k_1\ (t) \equiv 0$ implies from (25) that

$$k_2(t) = (b - la)\, k_2 - (a + 1)\, c, \tag{27}$$

which can now be solved to produce (28) evaluating $t = T$:

$$k_2(T) = Ke^{(b-la)t} + (ca + c)/(b - la), \tag{28}$$

where K is a constant composed of parameters. The boundary conditions in (26) requires $k_2(T) = 0$. Hence (28) can be set equal to zero and we solve for K:

$$K = [-(ca + c)/(b - la)] \cdot [e^{(b-la)(-T)}]. \tag{29}$$

Given expression (29) and (27) we can write the expression for $k_2(t)$ as in (30).

$$k_2(t) = [(ac + c)/(b - la)] \cdot [1 - e^{(b-la)(t-T)}]. \tag{30}$$

Remembering expression (17) and the boundary condition (26), $p\ (t) = k_2\ (t)$ since $k_1\ (t) \equiv 0$. Using expression (30) as equal to $p(t)$ and substituting into expression (16), we arrive at the solution for u^*:

$$u^* = ax - [l/2] \cdot [(ca + c)/(b - la)] \cdot [1 - e^{(b-la)(t-T)}] - c/2. \tag{31}$$

Define $g(t)$ as in expression (32):

$$g(t) = -c/2 - [l/2] \cdot [(ca + c)/(b - la)] \cdot [1 - e^{(b-la)(t-T)}] \tag{32}$$

Using (32) in (31) we have:

$$u^*(t) = ax(t) + g(t). \tag{33}$$

To express $\dot{x}$ in terms of variables not involving $u(t)$, substitute (33) into (12) and we obtain (34):

$$\dot{x}(t) = (la - b)\, x(t) + h + lg(t). \tag{34}$$

Expression (34) demonstrates that with the optimal control (33) we can obtain the behavior of nation X in terms of constants and variables not involving $u(t)$.

Examining expression (31) we can arrive at several preliminary conclusions. For discussion purposes let us rewrite (31) as:

$$u^*(t) = ax(t) - A \cdot B \cdot C - c/2 \tag{35}$$

where $A = [l/2]$, $B = [c\ (a + c)\ /\ (b - la)]$, and $C = [1 - e^{(b-la)(t-T)}]$.

First it can be noted from (35) that $u^*(t)$, the optimal control, is highly responsive to $x(t)$ as modified by the parameter a, which is nation U's perception of its own armaments needs given nation X's armaments. Hence in nation U's optimal armaments policy, it is highly responsive to nation X's armaments. Second, it can be noted that the quantity $(b - la)$ occurs in both expressions for B and C in (35). If we assume, as Richardson did, that all the parameters are positive,[20] the quantity $(b - la)$ can be either negative or positive in value and we can consider what happens under either condition.

When $(b - la)$ is negative, and t is small, making the quantity $(t - T)$ negative, the term C in (35) is also negative. Similarly B will be negative, while A is positive since all parameters are assumed to be positive. Hence $-ABC$ is negative. As t becomes larger approaching T, the quantity $(t - T)$ approaches zero so that $e^{(b-la)(t-T)}$ approaches 1 and the term C approaches the value of zero. Hence $-ABC$ approaches zero. With t small the term ABC or $g(t)$ in expression (33) serves to depress the armaments of nation U; but as t becomes larger and approaches T, the term ABC or $g(t)$ in expression (33) begins to approach zero and serves first as less of a depressant, and finally as no depressant at all, on nation U's optimal armaments policy at $t \cong T$. Hence we can conclude that if $(b - la)$ is negative, then the armaments of nation U increase through time, under the assumption that $x(t)$ is not decreasing.

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[18] Since we have not specified any "scrap value," i.e., value that must be shown by $x(t)$ when $t = T$, the adjoint variables at time $p(T)$ must be equal to zero. The adjoint variables are composed only of the state equation and contain no additional constraints. This property holds for all models of the general form of the linear-quadratic regulator problem.

[19] Because in equation (26), k_1 is dependent on $x(T)$ and in equation (24) $k_1(t)$ contains no k_2 terms, it is the case that $k_1(t)$ is a zero function. In equation (25) $k_2(t)$ is dependent on $k_1(t)$, but in equation (26) it must be the case that $k_2(T)$ is equal to 0.

[20] The assumption that all parameters are positive is a very strong assumption. It can be shown that Richardson could have obtained his results with a less strict assumption. The quantity $(b - la)$ is a very important quantity for this model as will be demonstrated below.

Now let us take the case in which $(b - la)$ is positive. When t is small, the expression $e^{(b-la)(t-T)}$ in equation (31) will be a small negative number and hence C in expression (35) will be positive. B in (35) will be positive as will A. Hence $-ABC$ will be negative, and as a result depress values of $u^*(t)$. As t approaches T, the term C in (35) approaches zero and hence $-ABC$ approaches zero. As time increases, therefore, the terms ABC in (35) and $g(t)$ in (33) become less of a depressant on the values of $u^*(t)$.

Given the above analysis we have shown that irrespective of the sign of $(b - la)$, and assuming $ax(t)$ is constant or increasing and that all parameters are positive, $u^*(t)$ will increase through time. Furthermore it can be shown that under these assumptions, the function $u^*(t)$ is a curve sloping gently away from the origin and increasing in slope to the terminal time $(t = T)$. Thus, if nation U has the objective as given in equation (11) and perceives nation X as modeled in (12), then its optimal armaments policy (best strategy) is to increase its armaments through time at an ever increasing rate.[21]

One further observation on equation (31) is important. The grievance term h in the plant (12) does not appear in (31). The grievance term only appears in the expression for nation X's armaments (34). Thus nation X's grievances, as perceived by nation U, are not directly a part of U's optimal armaments policy strategy.

Data and Data Analysis

Now that the optimal armaments policy for nation U has been derived, it is important to ask how closely the model fits with the empirical world. If we had values for the parameters a, b, l, and c, and the initial value for $x(t_0)$—the value of the opponent's armaments at an initial time point—we could predict nation U's armaments for each point in time from equation (31). A comparison of nation U's predicted values for each point in time with the actual value of its armaments for the same time points would then provide us with a test of the model's fit. If the values predicted from the optimal strategy $u^*(t)$ as given in equation (31) closely correspond (in terms of some goodness-of-fit criterion) to the actual values for a particular nation U, we could then conclude that the nation being examined behaves *as if* its objective function is as postulated in equation (11) and *as if* it perceives its opponents as given by equation (12).

The problem, however, is that we do not have estimates from decision makers of the parameter a, b, l, and c. We will, therefore, treat the problem somewhat in reverse arriving at "best" estimates of the parameters based upon optimizing measures of fit.

Equation (31) can be rewritten by rearranging terms as (36)

$$u^*(t) = ax(t) + [l/2] \cdot [(ca + c)/(b - la)] \cdot [e^{(b-la)(t-T)}] - [c/2] \cdot [(la + l)/(b - la) + 1]. \tag{36}$$

Examining (36), we can see that it has the following structure:

$$u^*(t) = \alpha x(t) + \beta e^{\lambda(t-T)} + \gamma, \tag{37}$$

where:

$$\alpha = a \tag{38}$$

$$\beta = [l/2] \cdot [(ca + c)/(b - la)] \tag{39}$$

$$\gamma = [-(c/2)] \cdot [(la + l)/(b - la) + 1] \tag{40}$$

$$\lambda = b - la. \tag{41}$$

We have now restated the estimation problem on equation (31) as equation (37) in terms of the parameters α, β, γ, and λ as defined in equations (38) through (41). Equations (39) and (40) can be more usefully stated as in (42) and (43). Using (39) and (41) we obtain:

$$\beta = l[(ca + c)/(2\lambda)]. \tag{42}$$

Using (39) and (40) we obtain:

$$\gamma = -\beta - c/2. \tag{43}$$

From equations (38), (41), (42) and (43), we can obtain expressions for the unknown parameter values, a, c, l, and b in equation (31) in terms of the unknown parameters α, β, γ, and λ in equation (37). From equation (38):

$$a = \alpha. \tag{44}$$

From (43) we obtain:

$$c = -2(\gamma + \beta). \tag{45}$$

From (42) we obtain:

$$l = (2\lambda\beta)/(ca + c). \tag{46}$$

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[21] The assumption that a nation has a given objective function should not be thought to be unnecessarily strict because the objective function used here is sufficiently general to incorporate many differing instances. It is important to note that our derivation is both necessary and sufficient.

Substituting into (46) from (44) and (45) we obtain:

$$l = -[\lambda\beta] / [(\gamma + \beta)(\alpha + 1)]. \qquad (47)$$

From (41) we obtain:

$$b = \lambda + al. \qquad (48)$$

Substituting into (48) from (44) and (47) we obtain:

$$b = \lambda - [\alpha\lambda\beta] / [(\gamma + \beta)(\alpha + 1)]. \qquad (49)$$

Equations (44), (45), (47) and (49) all express the original parameters in equation (31), the optimal strategy for nation *U*. If estimates can be found for the parameters α, β, γ, and λ in equation (37), using equations (44), (45), (47) and (49), the parameters *a, b, l,* and *c* in equation (31) can be calculated. Equations (44), (45), (47) and (49) express the parameters *a, b, l,* and *c* as functions of only the parameters α, β, γ and λ and numerical constants. As a result the values for calculating the parameters *a, b, l,* and *c* can be obtained directly from an estimation procedure on equation (37), using the equivalences in equations (44), (45), (47) and (49).

Examining equation (37) one can see that it has a similar structure to the linear regression model. The primary difference between (37) and the regression model is that the unknown parameter λ appears in the second term of the right hand side of the equation. If we treat the term $e^{\lambda(t-T)}$ as an independent variable choosing various values of λ and compute the parameter values for α, β, γ by least squares analysis for the various values of λ, we can create a function *G* which is dependent on values of λ. Call this function $G(\lambda)$. If the univariate function $G(\lambda)$ is minimized, then we can select the unique value of λ which when treated as a constant in equation (37) yields values of the other parameters α, β, γ by least-squares analysis.[22] In other words, once a value of λ is selected, the values of α, β, and γ can be obtained by treating (37) as an ordinary least-squares problem.[23] If $G(\lambda)$ is a global minimum, then our estimates will be unique.[24]

The function *G* needs to be specified. The obvious selection for *G* is $(1 - R^2)$, where R^2 is the multiple correlation coefficient. By minimizing $(1 - R^2)$ we are maximizing the percentage of variance explained. Hence our problem becomes one to select the λ which minimizes *G*, where *G* is $(1 - R^2)$ and involves the selection of values for α, β, and γ and yields that set of parameters which maximize explained variance.

While the mathematical problem of minimizing a function is simple, the empirical problem until a few years ago would have been exceedingly difficult. However, computer scientists have developed a number of algorithms for this kind of problem.[25] The algorithm best suited to our needs, developed by Powell,[26] is a variant on the method of conjugate directions developed by Davidon.[27] The algorithm simply finds a functional minimum based on an "initial" guess by moving in both directions from the guess. To assure that the minimum yielded from a first initial guess is not a local minimum (a valley in the curve separated from a deeper valley), several random initial guesses need to be made. If the results from the several guesses yield the same minimum, then there is ample evidence to choose the point of convergence as the global minimum.

The functional minimization was repeated ten times using randomly chosen initial values of λ in the range $[-5.0, +5.0]$ to counter the possibility of finding a local minimum for any single minimization.[28] For the findings reported below, no local minima were found, and there was complete convergence from all initial guesses of λ.

We will empirically assess our model by analyzing post-World War II data for six cases

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[22] The method of analysis here can be described as: Find a $G(\lambda)$ such that $F[\alpha, \beta, \gamma, G(\lambda)]$ meets some criterion.

[23] The associated measures of significance in ordinary least-squares analysis do not apply in our case. This is because we do not have, strictly speaking, a linear estimation equation. We have "linearized" equation (36) by using pseudoparameters, one of which is not chosen strictly by least-squares analysis.

[24] In any minimization one may encounter local minima (shallow valleys in a hypersurface separated by hills from deeper valleys) which may mask themselves as global minima (the deepest valley in the hypersurface). To assure that we have obtained a global minimum we have begun our analysis from several randomly selected initial values and have required convergence of final estimates from these initial values.

[25] See Richard P. Brent, *Algorithms for Minimization with Derivatives* (Englewood Cliffs, N.J.: Prentice-Hall, 1973).

[26] M. J. D. Powell, "An Efficient Method for Finding the Minimum of a Function of Several Variables without Calculating Derivatives," *Computer Journal*, 7 (July, 1964), 155–162.

[27] William C. Davidon, *Variable Metric Method of Minimization*, rev. ed. (Washington, D.C.: Atomic Energy Commission Research and Development Report, ANL-5990, 1959).

[28] Our criterion was convergence. If for several runs the analysis produced the same minimization, we assumed that a global minimum was found. The Davidon-Powell method is not constrained by the range selected for initial values. Values outside of the range are eligible as minima.

(U.S.A., U.S.S.R., N.A.T.O., W.T.O., Israel, and the Arab states) from the period following World War II. Since each nation or alliance can be considered the nation modeled (nation *U*) with the other nation or alliance as its opponent (nation *X*), we have in effect six tests of the proposed model.

Defense expenditure data were collected from the Stockholm International Peace Research Yearbooks.[29] The data were then treated in two ways. First, inflationary effects were removed transforming the data into terms of constant dollars. This step was desirable so that the estimates obtained would truly reflect defense expenditures and a true arms (defense expenditure) race. Second, because the Soviet Union experienced a governmental accounting change in 1961, adjustments were made to compensate for the post-1961 Soviet expenditures.[30] We did not adjust for such increases in expenditures that occurred in the United States during the Korean or Vietnam wars or in the Soviet Union during the Hungarian or Czechoslovakian uprisings. The NATO-WTO, USA-USSR, and Arab-Israeli data sets are in constant dollars with adjustment for accounting changes.[31]

One statistical problem that plagues almost all time-series analyses is autocorrelation.[32] Autocorrelation in data sets produces results that are not consistent and as a result are not useful for predictive purposes.[33] Two simple tests can be used to measure the degree of autocorrelation in a given estimation using ordinary least-squares analysis. They are the Durbin-Watson *d* statistic and the Geary sign test.[34] Both tests are based on the pattern of residuals in an ordinary least-squares analysis. If autocorrelation is found in a given analysis, one estimation procedure for adjusting for autocorrelated residuals is the Prais-Winsten transformation.[35] If autocorrelation is suggested by the Durbin-Watson *d* statistic and by Geary sign test, an estimate of the first-order autocorrelation parameter, $\hat{p}$, can be computed by using equation (50).

$$\hat{p} = \frac{\sum_{t=2}^{n} u_t u_{t-1}}{\sum_{t=1}^{n-1} u_t^2}, \qquad (50)$$

where u_t represents the ordinary least squares residuals for time *t*. The autocorrelation parameter can now be used as a transformation on the terms in the estimating equation following Prais and Winsten, in the following manner. Let Z be the original variable and TZ be the transformed variable where

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[29] International Institute for Peace and Conflict Research, *SIPRI Yearbook of World Armaments and Disarmaments* (Stockholm: Stockholm International Peace Research Institute, 1968 and forward). Our data sets run from 1947–1972 for the United States and Soviet Union; 1950–1972 for NATO, WTO, Israel and Arab states.

[30] See Abraham S. Becker, *Soviet Military Outlays Since 1955* (Rand Corporation Memorandum RM-3886 PR) (Santa Monica: The Rand Corporation, 1964). To adjust for the Soviet accounting change, the mean value of the four years after 1960 was compared to the mean value of the four years immediately prior to 1960. The prior-1960 mean value was subtracted from the post-1960 mean value, and this mean difference was then subtracted from all post-1960 total defense expenditures. This adjusted Soviet expenditure was used in calculating the WTO aggregate defense expenditure.

[31] The Arab nations were defined as Egypt (UAR), Syria, and Jordan. The data are expressed in terms of noninflationary dollars using mean yearly market exchange rates for determining conversion into American dollars. The rates of inflation were obtained from the Stockholm International Peace Research Institute.

[32] Autocorrelation is not the only statistical problem in time-series analysis. Multicollinearity is an additional problem. By the very nature of our model, one might expect multicollinearity. Our analyses show that there is some multicollinearity in our data but that it is not particularly disturbing. There are no agreed-upon methods for treating multicollinearity in time-series analysis. Some may challenge the need for treating autocorrelation in a model with a lagged endogenous variable. Once again there is no agreement on whether adjustments for autocorrelation are meaningful in the kind of model we have posited. See Philip A. Schrodt, *The Relationship Between Arms Races and the Preservation of Peace* (Bloomington: Indiana University, Ph.D. dissertation, 1976), chapter 7; and Marc Nerlove and Kenneth F. Wallis, "Use of the Durbin-Watson Statistic in Inappropriate Situations," *Econometrica,* 34 (March, 1966), 235–238. The conclusions from our analyses are the same, whether or not corrections for autocorrelation are made. More conservative findings are obtained when the correction is made. However, by no means do we fully endorse use of the Durbin-Watson statistic or the Geary sign test for systems with lagged endogenous variables. We have used these analyses because they do not alter the conclusions, and because they provide more conservative estimates of the parameters.

[33] See John Johnston, *Econometric Methods,* 2nd ed. (New York: McGraw-Hill, 1972), pp. 246–266 and 312–313; Arthur S. Goldberger, *Econometric Theory* (New York: John Wiley and Sons, 1964), pp. 153–155, 267–268 and 270–272.

[34] J. Durbin and G. S. Watson, "Testing for Serial Correlation in Least Squares Regression, I," *Biometrika,* 37 (June, 1950), 409–428, and "Testing for Serial Correlation in Least Squares Regression, II," *Biometrika,* 38 (June, 1951), 159–178; Hamid Habibagahi and John L. Pratschke, "A Comparison of the Power of the Von Neumann Ratio, Durbin-Watson and Geary Tests," *Review of Economics and Statistics,* 54 (May, 1972), 179–185; J. Durbin, "Estimation of Parameters in Time-Series Regression Models," *Journal of the Royal Statistical Society,* Series B, 22 (January, 1960), 139–153; D. Cochrane and G. H. Orcutt, "Application of Least-Squares Regression to Relationships Containing Autocorrelated Error Terms," *Journal of the American Statistical Association,* 44 (March, 1949), 32–61.

[35] Koteswara Rao Kadiyala, "A Transformation Used to Circumvent the Problem of Autocorrelation," *Econometrica,* 36 (January, 1968), 93–96.

$$T = \begin{bmatrix} \sqrt{1-\hat{p}^2} & 0 & 0 & . & . & . & 0 & 0 \\ -\hat{p} & 1 & 0 & . & . & . & 0 & 0 \\ 0 & -\hat{p} & 1 & 0 & . & . & 0 & 0 \\ 0 & 0 & 0 & . & . & . & -\hat{p} & 1 \end{bmatrix} \tag{51}$$

If the transform T is applied to the variables, then a new least-squares analysis (usually called generalized least-squares analysis) can be performed on the estimating equation with the transformed variables. Furthermore, if this first-order autocorrelation transformation using generalized least-squares is subjected to the Durbin-Watson d statistic and the Geary sign test, further autocorrelation may be exhibited. As a result, a second-order autocorrelation parameter, $\hat{p}$, can be computed using equation (50) and the residuals from the generalized least-squares analysis. A further transformation TT can be obtained using (51). This procedure can be repeated n times, where n is the number of observations in the time series. For our data analysis here, this procedure was repeated a sufficient number of times that neither the Durbin-Watson d statistic nor the Geary sign test led to rejection of the hypothesis of zero autocorrelation at the 5% level of significance.

Findings

Table 1 reports the generalized least squares analysis adjusting for autocorrelation on equation (37) using the Powell algorithm to minimize the function $G^*(\lambda)$,[36] where G^* is the generalized least squares $1-R^2$ meeting the condition of significance above.

The "pseudo-parameters" λ, α, β, and γ will be used later to compute the parameter a, b, l, c in equation (31). The most interesting finding shown in Table 1 is the magnitude of the generalized least squares correlation ($GLS\ R^2$) in comparison with the ordinary least squares correlation ($OLS\ R^2$). As can be seen from Table 1, considerable loss of explained variance occurs in the cases of the United States, Soviet Union and NATO when autocorrelation is controlled. Also the fairly deep order of autocorrelation for the United States and NATO reflects a behavior of incremental decision making in defense during the post-World War II era. In other words, because these nations or alliances exhibit routine increases in their expenditure behavior, our model, which calls for a more sophisticated form of calculation is not very successful in predicting the armaments behavior. The USA, USSR, and NATO seem not to abide by an arms balance strategy and because of the high autocorrelation in our results, a more naive calculation of incrementalism seems to be the mode of decision making. But in the cases of the WTO nations taken as a group, Israel, and the Arab nations, defense expenditures exhibit none or barely some autocorrelation, and the generalized least-squares R^2 is equal or about equal to the ordinary least-squares R^2 estimate. In these three cases the arms balance model fits extraordinarily well with the data.

It is significant that the Soviet Union taken separately does not follow the model proposed; however, the Soviet Union taken together with the other WTO nations does seem to follow the arms balance model. In other words, the Soviet defense expenditure policy as part of the overall defense policy of the WTO seems to reflect an arms balance calculation with the NATO countries, and not an arms balance calculus with respect to the United States. Although there is some increase in the generalized least-squares R^2 comparing the United States to NATO, this increase is insufficient to permit arguing conclusively that the United States's calculus as part of NATO follows the arms balance model. What *can* be argued is that

Table 1. Generalized Least-Squares Analysis of Equation (37)

	Cases					
Parameter	**USA**	**USSR**	**NATO**	**WTO**	**Israel**	**Arabs**
λ	−1.07	−.25	.62	.33	.25	−.13
α	.94	.18	1.21	.16	.15	.83
β	0.00	.03	−52.06	43.59	1626.02	−7.89
γ	192.92	161.56	146.85	23.49	9.36	459.20
GLS R^2	.32	.42	.58	.88	.99	.97
No. of Transformations	2	1	3	0	0	1
OLS R^2	.76	.83	.69	.88	.99	.99

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[36] In obtaining the parameter from this analysis, γ has been standardized by dividing by the autocorrelation parameter, $\hat{p}$. The reader should note that, whereas GLS provides more conservative findings, the conclusions are the same whether the analysis is completed under GLS or OLS.

when taken as part of NATO, the United States is more likely to follow an arms balance strategy than when taken separately in an adversary relationship with the Soviet Union. The results in Table 1 clearly suggest that an understanding of American and Soviet defense policy does require a full treatment of the relationships between these powers and their major alliance partners.

Table 2 presents the results for the original parameters in equation (31), the optimal strategy, using equations (44), (45), (47) and (49) for calculation from the parameters as estimated for equation (37). The parameter *a*, as formulated in the model, is the degree of difference between nation *U* and nation *X* that is tolerable to nation *U*. As can be seen from Table 1, only NATO requires that its arms level be greater than its adversary. The behavior of NATO in the post–World War II era has been to attempt to maintain an advantage over the WTO countries such that NATO arms expenditures through time are greater than the arms expenditures of WTO. The United States and the Arab nations do not require that they match one-for-one the defense expenditures of their adversaries. In both of these cases, however, the value of the parameter *a* is near 1.0, but slightly short of that figure. In the case of the Soviet Union, WTO and Israel, the degree to which arms expenditures must be equivalent with adversaries is minimal. In other words national security for these cases does not require a matching of their expenditures with those of their adversaries.[37]

The results for the parameter *c* are most interesting, if not most discouraging. Parameter *c* as formulated in the original model is the degree to which nations are concerned about the total arms shared between themselves and their adversaries. In all six cases the value for parameter *c* is negative, which implies that there is no concern, in fact a lack of concern, for arms proliferation. The greatest lack of concern for arms proliferation is shown by Israel and the Arab states. The least lack of concern is exhibited by the NATO and WTO countries taken as a group. The United States and the Soviet Union fall between the extremes. These results demonstrate that for the six cases, nations do not consider the arms proliferation in their defense calculus, and in fact encourage proliferation. Proliferation has not been a restraint on arms expenditures; it has been an inducement for increased armaments. The inducement that proliferation exhibits to armaments expenditures is greatest among those nations who have experienced repeated wars in the post-World War II era. The Israelis and the Arabs seem to desire increases in arms in part because proliferation is of no concern in determining armaments policy. The results in Table 2 demonstrate that for nations to follow the arms balance model, proliferation of arms becomes a necessity.

The parameter *l* as shown in Table 2 and as formulated in the original model is the perceived threat coefficient, i.e., the perceived threat which a nation's armaments pose to its adversary. In three cases the value of *l* is zero. In other words, the armaments for these three nations are not perceived to threaten their opponents. This result runs against the conventional wisdom in understanding the nature of arms races.[38] When, however, one additionally takes into account the values of the *b* parameter, and the perceived economic burden and fatigue imposed on one's opponent by being party to an armaments race, the results become more meaningful. In all three cases where *l* is zero, the values for the *b* parameter are negative, i.e., the arms races for these cases are not perceived as an economic burden on one's adversary, but as an economic stimulant. As a result it is very reasonable that the perceived threat imposed on one's adversary is nonexistent when it is also perceived that arms expenditures are not a burden to one's opponent, but an economic stimulant. It is interesting to note that the United States' behavior is consistent with the perception that the

Table 2. Values of Parameters in Equation (31) Based on Generalized Least-Squares

Parameter	Cases					
	USA	USSR	NATO	WTO	Israel	Arabs
a	.94	.18	1.21	.16	.15	.83
c	−385.83	−323.18	−189.58	−134.16	−3270.77	−902.62
l	0.00	0.00	.15	−.18	−.22	0.00
b	−1.07	−.25	.80	.30	.22	−.13
GLS R^2	.32	.42	.58	.88	.99	.97

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[37] Care must be taken in interpreting these results, given the measures of fit in Tables 1 and 2.

[38] Richardson and his followers have accepted the idea that all parameters must be positive. See Richardson and, for example, D. L. Wagner et al.

Soviet Union benefits greatly from arms expenditures. The degree of benefit is less for the United States from the perception of the Soviet Union.

For two cases (WTO and Israel) the perceived threat to the nation's adversaries is actually negative. In other words, arms expenditures by both the WTO nations and Israel are perceived not to be an inducement for their enemies to arm, but a constraint on their adversaries' desire to arm. This observation also runs against conventional wisdom. Richardson and his followers have generally assumed that the threat of armaments should always be perceived by opponents as inducements to arm.[39] The results for the arms balance model as formulated here demonstrate that for both the WTO and Israel, the perception of the adversary is one of "submissiveness" when faced with increased arms expenditure. This result is especially meaningful when the values of the *b* parameter, economic fatigue and expense, are interpreted. For both the WTO and Israel the perception is that their adversaries suffer some economic fatigue and expense from the armaments race. Hence, the values of the rate of change of armaments of the adversaries of WTO and Israel are not a function of threat, but are perceived to be more of a function of economic fatigue and expense.[40]

The case of NATO is the classical Richardson case.[41] In the perception of the NATO alliance, the threat imposed on its adversary is positive as is the perception of the adversary's fatigue and expense. The results in Table 2 demonstrate that the armaments of NATO are perceived to increase the threat to the WTO nations, and that in answering that increased threat, the WTO nations do suffer considerable economic fatigue.

By inserting the values displayed in Table 2 into equation (31), the optimal policy, one can draw the trajectories of the optimal defense policy for each of our six cases, as shown in Figure 2. For ease of interpretation we have drawn the curves in Figure 2 so that all nations begin at the same starting point. Adjustments for initial condition can be made by adding more curves up and down the vertical axis. Figure 2 indicates that there are three types of optimal policy behavior.

Type 1: *Linear Incremental*

In type 1 the optimal policy is to increase armaments at a constant positive

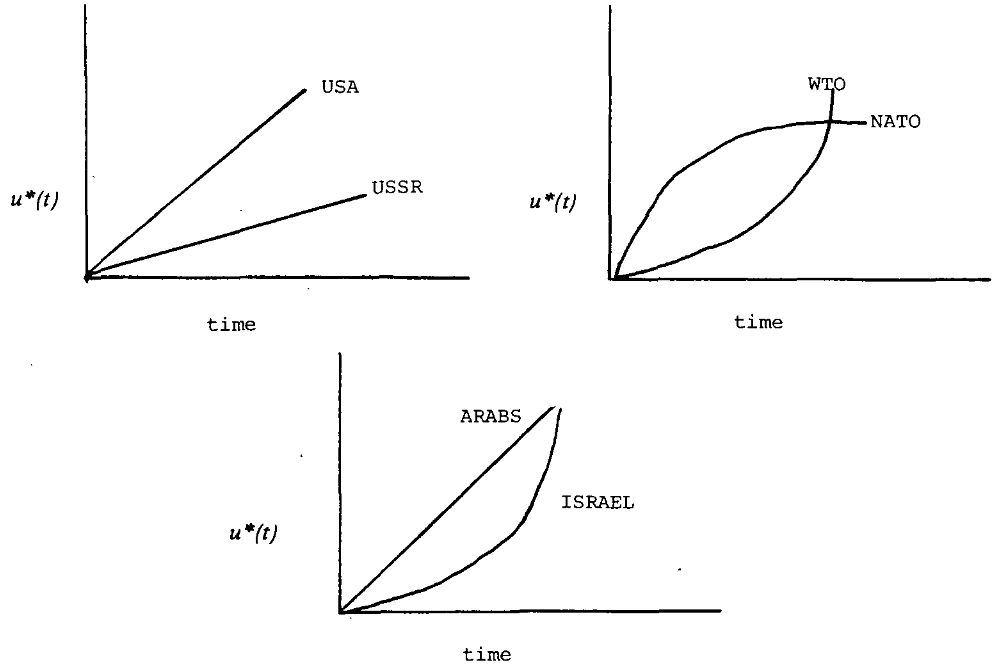

Figure 2. Trajectories of Optimal Defense Policy

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[39] See for example Smoker's work, and Caspary.

[40] It must be remembered that the parameter *b* is multiplied by a negative unity in the state equation.

[41] For NATO all the signs of the coefficients are in the direction Richardson stipulated. See Richardson.

rate. (Examples: USA, USSR, Arab nations)

Type 2: *Marginally Increasing*

In type 2 the optimal policy is to marginally increase armaments. The rate of increase in arms is always increasing. (Examples: WTO, Israel)

Type 3: *Marginally Decreasing*

In type 3 the optimal policy is to marginally decrease armaments. The rate of increase in arms is always decreasing. (Example: NATO)

The linear incremental policy (type 1) places little emphasis on the adversary's perceived threat and places major emphasis on a "steady course of growth" in arms. The marginally increasing optimal policy (type 2) is a plan of "catching-up" to the decision maker's perceived needs (objectives). It may be thought of as "delayed gratification." The notion in type 2 policies is to start slowly and then rapidly advance later. A marginally decreasing armaments policy (type 3) is a program of "gain the early advantage" and then rest on that "advantage." Such a policy calls for an early gratification of needs.

Noting that the optimal behaviors of the adversaries in arms races do differ is important. Furthermore, there are obvious implications for the pairs of antagonistic nations where optimal policies taken in combination have certain mixes of the three types of policy behavior. These mixes are most valuable in attempting to understand points in time at which changes in strategy may occur. To understand these phenomena, we must first examine the equilibrium and stability for our model.

Equilibrium and Stability Analysis

An important question to ask about any dynamic model is whether the model exhibits the attributes of equilibrium and stability, or in other words, what conditions, if any can be found, need to be satisfied if equilibrium and stability are to be achieved. Equilibrium for a dynamic model is some point or set of points such that the rates of change in the state variables equal zero, i.e., a position at which the system does not change.[42] However, if an equilibrium exists, we also want to know whether the equilibrium is stable, i.e., for some small exogenous perturbation or force driving the system from its equilibrium, will the system return to its equilibrium?

Such questions are important to ask because equilibrium and stability may or may not be desirable attributes. In a system which describes the trajectory of a manned spacecraft flight to the moon, equilibrium is not desirable. If equilibrium did exist the spacecraft would cease to move from some location between earth and the moon ("lost in space"). The goal of the space exploration could not be achieved, and the astronauts would not return to earth, let alone travel to the moon. In other systems, especially arms races, equilibrium is a desirable attribute.[43] When equilibrium is achieved, nation X as perceived by nation U is in a steady state and does not need to increase its armaments.[44]

Stability may also be desirable or undesirable depending on the problem. Stability tells us whether or not exogenous forces applied to the system can move it permanently away from its equilibrium or from its trajectory as modeled. In some systems once equilibrium is obtained or once the system is on its trajectory toward the terminal time, it is desirable to move the system to a new trajectory by applying some new force. For example, for an economy in an inflationary spiral and on a trajectory toward higher and higher rates of inflation, the introduction of some force may be desirable to move the economy away from its inflationary trajectory. In other systems, however, it is desirable for the system itself to counter disturbances by returning to its equilibrium or original trajectory. In armaments races disturbed by some international crisis or by other exogenous factors, it is of considerable importance to know whether the arming process will return to steady state equilibrium where nations will not increase armaments, or if the arms race will spiral off into a runaway proliferation of arms.

To examine equilibrium and stability we need to set T, the terminal time, to some large number, say about 40 years.[45] This is necessary because there is no assurance that an equilibrium will exist in a shorter time horizon. By making T large—for our purposes here the actual value of T is irrelevant—there is some assurance that the equilibrium, if it does exist, will occur in the larger time horizon.[46]

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[42] For readable summaries of equilibrium analysis, see Intriligator, *Mathematical Optimization*, pp. 220–257; Perkins and Cruz, pp. 72–78.

[43] Since our solution is for a steady-state equilibrium, one must be careful not to interpret equilibrium as total disarmament. Equilibrium for our analyses simply means that there the rate of change in armaments is zero, not that armaments expenditure becomes zero.

[44] In equilibrium, if $\dot{x}$ equals zero, it is not the case that $\dot{u}$ is equal to zero. The nation U is the controller in our model and may increase or decrease its armaments expenditure while nation X is in equilibrium.

[45] This method has been shown to be entirely legitimate. See ; Leonard Weiss and E. F. Infante, "On the Stability of Systems Defined over a Finite Time Interval," *Proceedings of the National Academy of Sciences*, 54 (July 15, 1965), 44–48. Normally one assumes T to be infinity, but such an assumption is not appropriate for our analysis. The actual value of T is irrelevant.

By using equation (31), the optimal policy for nation U and equation (34), the trajectory for nation X given nation U's optimal policy, the equilibrium can be obtained. Substituting equation (31) into equation (34), we obtain equation (52).

$$\dot{x}(t) = l[ax(t) - [l/2]\,[(ca + c)/(b - la)] \cdot [1 - e^{(b-la)(t-T)}] - c/2] - bx(t) + h. \qquad (52)$$

Integrating (52), we can find the optimal trajectory $x^*(t)$ given $u^*(t)$.

$$x^*(t) = [(lc - 2h)/(2(la - b)) - (l^2c(a+1))/(2(la - b)^2)] + [x(t_o) + (l^2c\,(a + 1))/(2(la - b)^2) + (2h - lc)/(2(la - b)] \cdot [e^{(la-b)t}] - [(l^2c\,(a + 1))/(4(la - b)^2)] \cdot [e^{(la-b)(T+t)}] + [(l^2c(a + 1))/(4(la - b)^2)] \cdot [e^{(la-b)(T-t)}]. \qquad (53)$$

Equation (53) provides the optimal trajectory of nation X *from the perspective* of nation U. Equation (53) tells us nation U's expectation of the optimal policy of nation X given nation U's optimal policy, $u^*(t)$. Examining equation (53) using our results in Table 2, we can obtain the shape of $x^*(t)$ as displayed in Figure 3. For ease of interpretation the curves in Figure 3 have been drawn so that all nations begin at the same starting point. Adjustments for initial conditions can be made by moving the curves up and down the vertical axis. In Figure 3 the perceiving nation (nation U) is placed in parentheses and the perceived nation (nation X) is not.

What is interesting in Figure 3 is that four of the six cases perceive their adversary as "marginally increasing," or in other words as "catching up"—(USSR as perceived by the US; US as perceived by the USSR; Israel as perceived by the Arab nations; and the Arabs as perceived by the Israelis). The degree of the "catching up" perception is especially acute in the United States's perception of the Soviet Union and the Arabs's perception of the Israelis. This "catching up" perception is the anticipated behavior of one's adversary. For the United States and the Arabs, the expectation is that the Soviet Union and Israel, respectively, are putting forth an extreme effort in accumulating armaments. For the Israelis and the Soviets there is also the perception of increased effort in armaments, of their adversaries, the Arab nations and the

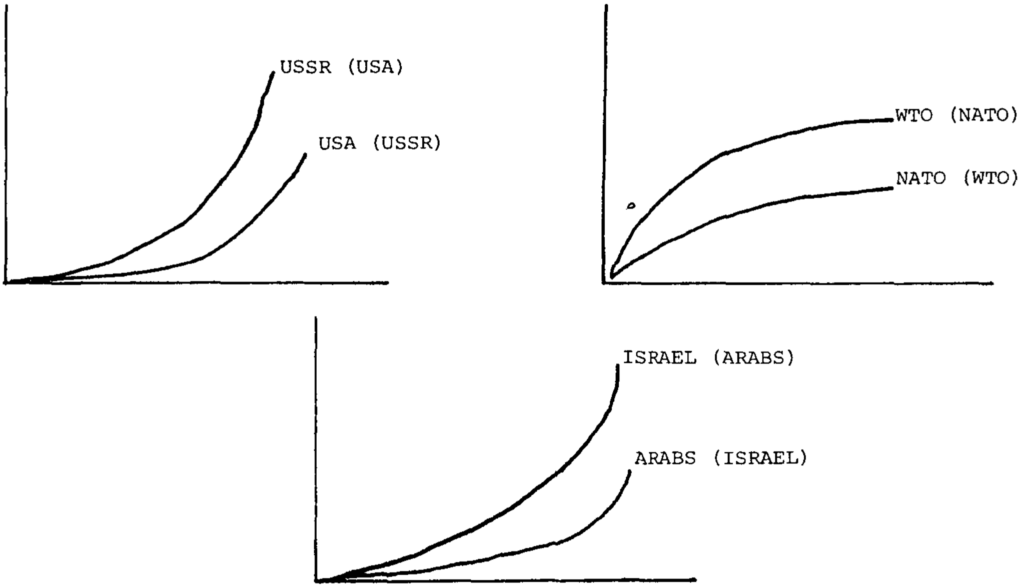

Figure 3. Perceived Optimal Trajectories of Opponent's Defense Policy

Note: Perceiving nation (nation U) is denoted by parentheses.

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[46] For the optimal control regulator problem of the form here, the assumption that equilibrium and stability exist in an infinite time horizon is incompatible with the boundary conditions used to solve for $u^*(t)$. Hence the weaker, but still legitimate, assumption of setting T equal to a large number is employed.

United States, respectively, but at a lower marginally increasing rate.

For NATO and WTO the perceptions are of a declining armaments race. Both alliances perceive each other to be decreasing the rate of armaments build up. In other words the armaments race in Europe is perceived by both sides to reach a point at which there is some steady state whereby the increments in armaments expenditure approach zero.[47]

Returning to equation (53), it is fortunate when evaluated for equilibrium that (53) simplifies. Let us denote the equilibrium point as t^o, where $t_o < t^o < T$, and t_o is the initial time and T is the terminal time. By using the definition of equilibrium, $\dot{x}(t^o) = 0$, and setting $t = t^o$ in expression (53) we can write:

$$x(t^o) = [1 - e^{(la-b)(T-t^o)}] \cdot [(l^2ca + l^2c)/ (2(la - b)^2)] - [h/(la - b)] + [(lc) / (2(la - b)]. \tag{54}$$

Expression (54) is the steady state equilibrium for our model. It is a steady state equilibrium because our analysis incorporates the expression for $u^*(t)$ into expression (53) from which we obtain the equilibrium (54). A steady state equilibrium tells us the equilibrium value of $x(t)$ given the optimal control input, $u^*(t)$. For our particular analysis, equation (54) presents the anticipated equilibrium for nation X from the view of nation U. In other words, given the strategy played by nation U (strategy $u^*(t)$) and the model of nation X's reaction (equation (34)), equation (54) tells us the value that $x(t)$ must be for $\dot{x}(t)$, the rate of change in the adversary's armaments, to be zero.

Expression (54) can be numerically evaluated given the results in Table 2.[48] By numerically evaluating expression (54) we are asking, what is the arms expenditure which one's adversary must achieve if equilibrium is to be obtained. The results of the numerical evaluation show that for only three of the six cases, is there an equilibrium present in the data.[49] Table 3 summarizes the results.

As noted in Table 3, only NATO, WTO, and Israel perceive some point in time such that the rate of change in their adversary's armaments is zero. For the other cases no such meaningful equilibrium point exists. The perceptions of the Soviet Union by the United States, of the United States by the Soviet Union, and of the Israelis by the Arabs, rule out the anticipation that the armaments race will grind to some steady state in which the opposing nations will not increase their defense expenditures.

The knowledge that a given empirical arms race has or has not an equilibrium does not tell us whether or not the race is stable. An arms race that has an equilibrium may not be stable; the equilibrium point may be passed in time on the course of ever increasing armaments. At the opposite extreme, an arms race that does not have an equilibrium may be stable; the nations involved may move on a stable trajectory toward greater and greater amounts of armaments. In stability analysis we assume that some force applied to the trajectories displaces a nation from its optimal course. The question asked is, Will the nation return to its trajectory

Table 3. Equilibrium Analysis[49]

Nation U (Perceiving Nation)	Nation X (Perceived Nation)	Equilibrium Value Achieved?
USA	USSR	No
USSR	USA	No
NATO	WTO	Yes
WTO	NATO	Yes
Israel	Arabs	Yes[a]
Arabs	Israel	No

[a] The equilibrium in this case is subject to minor fluctuations.

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[47] For this analysis, estimates of the constant h needed to be derived. Consistent with our analysis is the use of the mean difference in expenditures. For our analysis h is equal to the following values for the six cases.

Nation	h	Measuring Unit
USA	1.67	billions of dollars
USSR	.68	billions of dollars
NATO	2.39	billions of dollars
WTO	.97	billions of dollars
Israel	62.16	millions of dollars
Arabs	57.99	millions of dollars

[48] This numerical evaluation uses the values of h in footnote 47 and sets $(T - t^o)$ equal to numbers ranging from 50 to 1.

[49] The actual values for the equilibrium are meaningless in that we have set the initial conditions for all nations equal to a small positive number. The numbers are interpretable in terms of whether or not the values yielded using series of $(T - t^o)$ numbers are about the same.

or will a new course of armaments behavior be pursued? In stability analysis we are assessing the degree to which the armaments behavior of nations through time is "fixed" and not responsive to outside forces.

Using the Liapunov method one can immediately see from equation (52) that there does exist some condition under which the system is stable.[50] A system is said to be stable at $t = t^o$ (where t^o is some critical point) provided that for some arbitrary positive displacement $\epsilon > 0$, there is a $\delta = \delta(\epsilon, t^o)$ such that whenever $\| x^o \| < \delta$, the inequality $\| x(x^o; t^o; t) \| < \epsilon$ is satisfied for all $t \geqslant t^o$, where $\| x \|$ is the Euclidean norm equal to $(x_1{}^2 + \ldots + x_n{}^2)^{1/2}$. Furthermore a system is said to be asymptotically stable if it is stable and in addition there exists a $\delta_o(t^o) > 0$ with the property that if $\| x^o \| < \delta_o$, then the limit as t approaches infinity of $x(x^o, t^o, t) = 0$.

To examine the stability properties of our model we must first solve equation (52) to obtain $x(t)$. This solution is given in equation (55).

$$\begin{aligned} x(t) = {} & [e^{(la-b)t}] \cdot [x(t_o) + (1/(la-b)) \cdot \\ & (h - (lc)/2) - ((l^2/4)(la-b)^2)\,(c + ac)] + \\ & [e^{-(la-b)t}] \cdot [((l^2/4)(la-b)^2)(c + ac)] - \\ & [1/(la-b)] \cdot [h - (lc)/2]. \end{aligned} \tag{55}$$

Equation (55) is of the form:[51]

$$x(t) = A + B - C. \tag{56}$$

Only the A part in equation (56) contains x terms. As a result the B and C parts are forcing functions, i.e., factors not related to $x(t)$ as a function of $x(t_o)$. Since the x term in the A part is multiplied by an exponential term, if the exponent moves to zero, then $x(t_o)$ multiplied by the exponential term will move to zero. The condition which moves the exponential term to zero is $(la - b) < 0$. Consequently this is the stability condition. To achieve absolute zero, i.e., absolute stability, $(la - b)$ must be equal to minus infinity.[52] If the Liapunov stability condition is satisfied, then the trajectories do have a "homing instinct," i.e., when displaced the trajectories return to their original course. If absolute stability is achieved, then the trajectory cannot under any conditions be displaced.

It is important to note that the stability condition is for a steady state stability incorporating feedback. In other words we are asking if the course of an adversary's armaments behavior is stable given the policy pursued by the perceiving nation.

Now that we have obtained the stability condition for our model, the results in Table 2 can be used to numerically evaluate the steady state stability condition for the six empirical armaments races. Table 4 presents these results.

As Table 4 indicates, only three of the six trajectories are stable. The trajectory showing the greatest instability is the Soviet Union's defense policy as perceived by the United States. The American defense policy as perceived by the Soviet Union is also unstable, but less unstable than is the Soviet Union's defense policy as perceived by the United States. The trajectory for Israel as perceived by the Arab nations is almost unstable, but its instability is less than those exhibited by the Soviet Union and the United States.

The least stable of the stable cases is the Israeli perception of the Arab nations. Although this case is stable, the low value of the stability coefficient $(la - b)$ indicates that as perceived by Israel, considerable time is necessary before the Arab nations return to the anticipated trajectory after some displacement force is applied. Hence, displacement forces, which can be viewed as momentary crises, have

Table 4. Stability Analysis

Nation *U* (Perceiving Nation)	Nation *X* (Perceived Nation)	Stability Coefficient	Stable?
USA	USSR	+1.07	No
USSR	USA	+ .25	No
NATO	WTO	– .62	Yes
WTO	NATO	– .33	Yes
Israel	Arabs	– .25	Yes
Arabs	Israel	+ .13	No

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[50] Aleksandr M. Liapunov, *Stability of Motion* (New York: Academic Press, 1966); Horst Liepholz, *Stability Theory* (New York: Academic Press, 1970).

[51] For equation (56) from (55):

$$A = [e^{(la-b)t}] \cdot [x(t_o) + 1/(la-b)) \cdot (h - (lc)/2) - ((l^2/4)(a-b)^2)(c + ac)]$$

$$B = [e^{-(la-b)t}] \cdot [((l^2/4)(la-b)^2)(c + ac)]$$

$$C = [1/(la-b)] \cdot [h - (lc)/2].$$

[52] An examination of equation (55) will show that when $(la - b) = -\infty$, then $x(t)$ is equal only to the forcing function.

a strong effect on Israeli strategy toward the Arabs, and absorb a considerable amount of time before the anticipated behavior can be restored. Both the NATO and WTO cases are stable, and although WTO is perceived by NATO to be more stable than NATO as perceived by WTO, given the value of the stability coefficient, both original trajectories are resumed after some displacement is applied.

The stability analysis allows us to determine how nations react to sudden changes in their environments. Displacement forces are the results of exogenous momentary impulses applied to the model. When displacement occurs, nations need to adjust to these momentary impulses. The stability coefficients tell us how nations are perceived to adjust by their adversaries. The United States perceives the Soviet Union to adjust by moving to a new course of action. The size of the stability coefficient in this case tells us that shocks to the Soviet's anticipated trajectory lead the Soviets to adopt a radically different trajectory of behavior. Although not as radically different from its anticipated trajectory, the armaments behavior of the United States as perceived by the Soviet Union also when shocked, takes on a new course. The Arab nations also perceive the Israelis to take on a new trajectory of armaments policy when its original trajectory is displaced by exogenous forces.

Conclusions

Formulating an optimal control model developed from the balance of power literature, we have derived the optimal armaments policy. We have assumed that one's adversary behaves as a Richardson-type actor. Examining three arms races, we have arrived at several conclusions. Table 5 summarizes our results.

Examining each armaments race we can conclude that the USA-USSR confrontation is one that lacks both stability and equilibrium and is leading toward continued increments in arms. Both nations perceive each other to arm primarily for economic reasons. Both arm out of a lack of concern for arms proliferation. When both the United States and the Soviet Union, however, are taken as parts of their respective major alliance partners, the armaments race is both stable and equilibrium-seeking. Not only do the perceptions of the adversary change, but also the nature of the armaments race becomes one of control and moderation. Our analysis suggests that direct confrontation between the Soviet Union and the United States is a very high risk phenomenon, whereas lower risks are involved in confrontation between the two superpowers in the context of NATO and WTO.

The Arab-Israeli arms race shows equilibrium and stability in terms of Israeli strategy, but a lack of such equilibrium and stability in terms of Arab strategy. Whereas the Israeli policy leads to an equilibrium and stability, the Arab policy leads away from equilibrium and stability. Such an armaments race, although less risky than direct Soviet-American confrontation, still is highly risky because of the lack of a strategy on the part of the Arab nations that would produce equilibrium and stability in Israeli response.

If nations cannot formulate optimal policies that satisfy their objectives and likewise produce an equilibrium and stability in their adversary's reactions, then the armaments races themselves are subject to radical shifts and changes. Such shifts and changes encourage national decision makers to attempt to play "new games." The human costs of new games of armaments in this advanced nuclear age are far too costly to incur.

Table 5. Summary of Conclusions

Nation/ Alliance	Optimal Trajectory	Opponent's Optimal Trajectory	Equilibrium Achieved?	Stable?	Degree of Fit
USA	Linear incremental	Marginally increasing	No	No	Weak
USSR	Linear incremental	Marginally increasing	No	No	Weak
NATO	Marginally decreasing	Marginally decreasing	Yes	Yes	Adequate
WTO	Marginally increasing	Marginally decreasing	Yes	Yes	Adequate
Israel	Marginally increasing	Marginally increasing	Yes	Yes	Good
Arabs	Linear incremental	Marginally increasing	No	No	Good

A Technique of Computer Content Analysis of Transliterated Russian Language Textual Materials: A Research Note*

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Doing research on the Soviet political system is a difficult task for the foreign scholar. He or she faces a host of well-known problems constraining the selection of topics for and methods of investigation. These problems include, among others, limited access to the country and virtually no opportunities for field research. One widely employed solution to these problems has been content analysis of documents. The persistence of problems constraining the study of Soviet politics makes imperative some way of conducting even more objective and systematic content analyses of documentary data. I have developed such a technique—the computer content analysis of transliterated Russian language textual materials. The purpose of this note is to describe this new technique and to illustrate its use with two examples.

Advantages of the Technique

"Content analysis is any technique for making inferences by objectively and systematically identifying characteristics of messages."[1] Two techniques of content analysis have been used so far in investigations of the Soviet political system: manual content analysis of Russian language textual materials and computer content analysis of English translations of textual materials originally published in Russian.[2] I have developed a new, third technique—computer content analysis of transliterated Russian language textual materials.

This technique has several advantages over the ones used previously. First, this technique opens up to the researcher the widest possible selection of documents. He or she does not have to limit the choice to those documents which are available in translation and thus faces fewer problems of sampling and consequently of statistical generalization. Second, this technique eliminates any problems associated with the translation of Russian into another language. The researcher avoids problems of the accuracy of translations or of variations in their quality. Third, this technique saves the time of the researcher, who does not have to wait for translations or to undertake them personally. If the researcher has possession of the documents, he or she is ready to begin preparing them for content analysis. Fourth, this technique permits the researcher to describe the characteristics of content in the reliable and consistent manner which makes computer analyses superior to manual ones.

Development and Basic Features of the Technique

Development of this new technique depended upon the availability of computer programs for content analysis that are not specific to the English language and of an algorithm for inflectional segmentation of Russian words. The availability of general purpose computer programs meant that Russian language textual materials could in principle be content analyzed. The availability of an algorithm for separating the inflectional ending from the stem of a word meant that it was feasible to construct dictionaries for the computer content analysis programs requiring them.

KADP and WORDS are two general purpose computer programs for content analysis.[3] Both of these programs were originally written for the content analysis of English language textual materials on American computing machinery; their use for the content analysis of Russian language textual materials on the same machinery requires some sort of transliteration of the letters in the Cyrillic alphabet. That is, Cyrillic letters must be assigned on some basis to the characters of another alphabet. The fact that the Cyrillic alphabet has 33 letters and the Latin alphabet 26 letters makes it impossible to

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*I received a grant from the Graduate College of the University of Iowa for computer programming in connection with the research reported in this note. Staff members of the Laboratory for Political Research and of the Statistical Consulting Service, both at the University of Iowa, provided welcome assistance and advice. Gerhard Loewenberg offered helpful comments on a draft of this note.

[1] Ole R. Holsti, *Content Analysis for the Social Sciences and Humanities* (Reading, Massachusetts: Addison-Wesley, 1969), p. 14.

[2] Examples of investigations employing the former and latter techniques are respectively the following: Milton Lodge, *Soviet Elite Attitudes Since Stalin* (Columbus, Ohio: Charles Merrill, 1969); and P. Terrence Hopmann, "International Conflict and Cohesion in the Communist System," *International Studies Quarterly*, 11 (September, 1967), 212–236.

[3] A description of KADP is available from John Winters, Department of Political Science, Tennessee Technological University, Cookeville, Tennessee 37203. WORDS is described in Howard P. Iker and Robert H. Klein, "WORDS: A Computer System for Analysis of Content," *Behavior Research Methods and Instrumentation*, 6 (July, 1974), 430–438.

assign one Cyrillic letter to one Latin letter.[4] An assignment on a one-for-one basis, however, is preferable because such a transliteration avoids certain problems associated with the inflectional segmentation of Russian words. Hence, it is necessary to create a new computational alphabet consisting of 33 characters—26 Latin letters and 7 Arabic numbers. Figure 1 presents a scheme for transliteration of Cyrillic letters into the characters of this new computational alphabet.

Russian is a highly inflected language. Each Russian word has many inflectional variants. For example, there are 24 possible different inflectional variants for each adjective. Content analysis is based upon the lexical meanings of words; and these meanings are expressed in the stems rather than in the inflectional endings of words. Without some means for separating the inflectional ending of a Russian word from its stem, however, most of the entries in a dictionary used in a computer program for content analysis would have to be inflectional variants of a very few words. Since both KADP and WORDS severely restrict the number of dictionary entries, it is desirable to include in a dictionary the stems of as many different words as possible.[5] Manual inflectional segmentation of Russian words is a tedious and time-consuming task. Fortunately, there is a sophisticated algorithm which can be written as a computer program to handle this task automatically.

Anthony Oettinger devised this algorithm for inflectional segmentation of Russian words in the course of his research on computer translation of Russian into English.[6] The algorithm consists of 57 separate statements written in propositional calculus. Each statement is a set of instructions for identifying the inflectional ending of any Russian word by reading its letters backwards, i.e., from right to left. Satisfaction of the conditions specified by the statements in the algorithm results in the identification of an inflectional ending. The algorithm does not always produce a unique stem for each word. The resulting number of stems for a word is, however, substantially less than the number of its inflectional variants. This algorithm constitutes the core of a computer program called DWLRUSS.

resulting number of stems for a word is, however, substantially less than the number of its inflectional variants. This algorithm constitutes the core of a computer program called DWLRUSS.

There are several features of DWLRUSS which make it well suited for the preparation of transliterated Russian language textual materials for computer content analysis.

(1) DWLRUSS does three things: (a) it reads the textual material which has been encoded according to a given transliteration scheme such as the one in Figure 1; (b) where possible, it separates the stem and inflectional ending of a word; and (c) it prints out on cards the textual material in a form in which each inflectional ending is bracketed and is separated by one space from the companion stem and by one space from any following punctuation mark. Figure 2 presents an example of the operation of DWLRUSS. Handling the inflectional endings in this manner is essential in the preparation of the textual materials for eventual computer content analysis. Computer programs for content analysis usually allow the user to designate textual material to be excluded from the actual content analysis. KADP automatically excludes any textual material bracketed by "less than" and "greater than" signs. WORDS requires a list of all words to be excluded; each one of the 56 different inflectional endings bracketed by "less than" and "greater than" signs would be on this list.

(2) DWLRUSS is a combination of several flexible subprograms. One of these subprograms permits the user to specify words up to 16 characters in length which will not be subject to

Alphabet	Characters
Cyrillic	АБВГДЕЁЖЗИЙКЛМНОПРСТУФХЦЧШЩЪЫЬЭЮЯ
Computational	ABVGDE2JZI3KLMNOPRSTUFX4CW59Y67QH

Figure 1. Transliteration Scheme

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[4] Conventional schemes for transliteration involve assignments of two Latin letters for a single Cyrillic letter and/or use of diacritical marks. See J. Thomas Shaw, *The Transliteration of Modern Russian for English-Language Publications* (Madison, Wisconsin: University of Wisconsin Press, 1967).

[5] In KADP the dictionary entries are specified by the user; in WORDS they are automatically chosen on the basis of the frequency of usage.

[6] Anthony G. Oettinger, *Automatic Language Translation: Lexical and Technical Aspects, With Particular Reference to Russian* (Cambridge, Massachusetts: Harvard University Press, 1960), pp. 143–151.

Original Textual Material:

Расскажите о Советах рабочих депутатов и их деятельности.
Приведите факты, подтверждающие превращение Советов в
органы власти.
Приведите доказательства контрреволюционности русской
буржуазии. Охарактеризуйте цели буржуазных партий.
Какие новые средства применяло самодержавие для борьбы с
революцией?

Input to DWLRUSS:

```
RASSKAJITE O SOVETAX RABOCIX DEPUTATOV I IX DEHTEL6NOSTI.
PRIVEDITE FAKTY, PODTVERJDAQ5IE PREVRA5ENIE SOVETOV V
ORGANY VLASTI.
PRIVEDITE DOKAZATEL6STVA KONTRREVOLQ4IONNOSTI RUSSKO3
BURJUAZII. OXARAKTERIZU3TE 4ELI BURJUAZNYX PARTI3.
KAKIE NOVYE SREDSTVA PRIMENHLO SAMODERJAVIE DLH BOR6BY S
REVOLQ4IE3?
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Output from DWLRUSS: [a]

```
RASSKAJ <ITE>  O SOVET <AX>  RABOC <IX>  DEPUTAT <OV>  I IX                    100
DEHTEL6NOST <I> .    PRIVED <ITE>  FAKT <Y> ,                                  200
PODTVERJDAQ5 <IE>  PREVRA5ENI <E>  SOVET <OV>  V                               300
ORGAN <Y>  VLAST <I> .                                                         400
        PRIVED <ITE>  DOKAZATEL6STV <A>                                        500
KONTRREVOLQ4IONNOST <I>  RUSSK <O3>         BURJUAZI <I> .                     600
OXARAKTERIZU3T <E>  4EL <I>  BURJUAZN <YX>  PARTI <3> .                        700
     KAK <IE>  NOV <YE>  SREDSTV <A>  PRIMENHL <O>                             800
SAMODERJAVI <E>  DLH BOR6B <Y>  S    REVOLQ4I <E3> ?                           900
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Figure 2. Inflectional Segmentation of Russian Words

[a] Prepositions, conjunctions, and words less than three characters in length were not subjected to inflectional segmentation.

inflectional segmentation. For example, the user could exclude all prepositions and conjunctions from inflectional segmentation. Parts of these subprograms can be easily altered by the user so that the output of DWLRUSS is an acceptable input to one or another computer program for content analysis. For example, the user could change the transliteration scheme; that change would result in a different computational alphabet.

(3) There are only three rules concerning the encoding of textual material that will be processed by DWLRUSS. First, a single transliteration scheme must be consistently employed. Second, any Latin letters in the transliteration scheme must be encoded as capitals. Third, textual material must be contained within the first sixty columns of each card or within the first sixty bytes of each tape or disk record. If the user encodes the textual material according to these three rules and to any noncontradictory rules imposed by the selected computer program for content analysis, then the card output from DWLRUSS will be an acceptable input to this program with minimal extra work. For example, this card output can be the input to KADP after the complete removal of the sequence numbers on each card and into WORDS after the replacement of the sequence numbers with special identification codes.

The serial operation of DWLRUSS and of a computer program for content analysis constitutes the new technique of computer content analysis of transliterated Russian language textual materials which I have developed.[7] Preparation of textual material for initial processing in the former program and, if necessary, construction of a dictionary for incorporation into the latter one are facilitated by the use of two auxiliary computer routines, both of which can operate with a 33-character computational alphabet. FREQLIST provides an alphabetical listing of the different words in a block of encoded textual material.[8] Such a listing is helpful for checking the accuracy of the spelling of individual words. Since many words would be likely to appear more than once in a block of textual material, any misspellings would show up out of place in an alphabetical listing of the different words in that block. In addition, an alphabetical listing of stems simplifies the construction of a dictionary. If a word selected for inclusion in a dictionary has multiple stems, this listing will contain each one of them. The "Locate" command in the IBM administrative terminal system offers a way of finding the locations of words in a block of textual material. Knowledge of these locations speeds up the correction of misspellings and permits checks on the usage of words with possible ambiguous referents.

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[7] The use of DWLRUSS requires little computer time; processing approximately 5000 cards with this program takes less than five minutes. DWLRUSS and its component subprograms are written in either PL/I or assembly language. These programs and a technical description of their operation are available from the Laboratory for Political Research, Department of Political Science, University of Iowa, Iowa City, Iowa 52242.

Illustrative Examples

I have used the technique of computer content analysis of transliterated Russian language textual materials in research on the political socialization of Soviet schoolchildren.[9] The following two examples from this research illustrate the use of the technique and can possibly clarify its mechanics. It should be pointed out that the purpose of presenting these examples is not to answer the questions posed. Such answers would require, in addition to the findings from the content analysis, contextual information and detailed arguments. A full elaboration of the two examples can be found elsewhere.[10]

Both examples report the findings from a content analysis of Soviet school textbooks with KADP. These textbooks, published circa 1970, were in required use in general-education schools throughout the Soviet Union.[11] The sample of textual material from these textbooks includes all of the exercises, i.e., questions and assignments, which conclude the units within the textbooks. Textbooks are the principal tool of instruction in Soviet classrooms, and teachers grade schoolchildren's performance in completing the exercises. Thus, these exercises summarize what Soviet schoolchildren are expected to learn from instruction.

The findings in both examples are patterns of emphasis upon selected political themes in the textbooks. There are two basic types of emphasis—relative and developmental. Relative emphasis involves a comparison within a grade; it refers to the amount of emphasis either upon various themes in a given subject or upon a particular theme in two or more subjects. Developmental emphasis involves a comparison over a span of grades; it refers to change in the emphasis upon a particular theme in a given subject. The patterns of relative and developmental emphasis upon the selected themes in the textbooks suggest possible configurations of the content and sequencing of the political socialization of Soviet schoolchildren.[12]

Levels of a Political System. The first example concerns the emphasis upon three themes—political community, political regime, and political authorities—in Soviet school history textbooks. The 11 textbooks used for history instruction in grades 4-10 contain 3281 exercises. It was necessary to construct a dictionary consisting of a subdictionary for each of the three themes. Construction of these subdictionaries involved four steps: (a) review of David Easton's conceptualization of the three levels of a political system;[13] (b) preliminary selection of words cognitively referring to each level from an alphabetical listing of different stems appearing in the entire set of exercises; (c) elimination of those words ambiguously used in the exercises; and (d) a determination of whether each word refers to the Russian or Soviet political system, to another political system in particular, or to a political system in general. The subdictionaries for political community, political regime, and political authorities contain, respectively, 133, 58, and 49 word-stems. Neither the affective dimension of

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[8] A description of FREQLIST is available from Keith Swigger, Department of Linguistics, University of Iowa, Iowa City, Iowa 52242.

[9] Use of content analysis in research on political socialization has been encouraged by Jack Dennis, "Future Work on Political Socialization," in *Socialization to Politics: A Reader*, ed. Jack Dennis (New York: John Wiley, 1973), p. 498.

[10] Charles D. Cary, "Patterns of Emphasis upon Community, Regime, and Authorities: A Computer Content Analysis of Soviet School History Textbooks" (paper, University of Iowa, 1974); and Charles D. Cary, "Patterns of Emphasis upon Marxist-Leninist Ideology: A Computer Content Analysis of Soviet School History, Geography, and Social Science Textbooks," *Comparative Education Review*, 20 (February, 1976), 11–30.

[11] Standardization of textbooks is possible through translations of the Russian language versions into all of the languages in which instruction is offered.

[12] The findings from a different kind of research are necessary to answer the question about the extent to which instruction affects the political socialization of Soviet young people.

[13] See David Easton, *A Systems Analysis of Political Life* (New York: John Wiley, 1965).

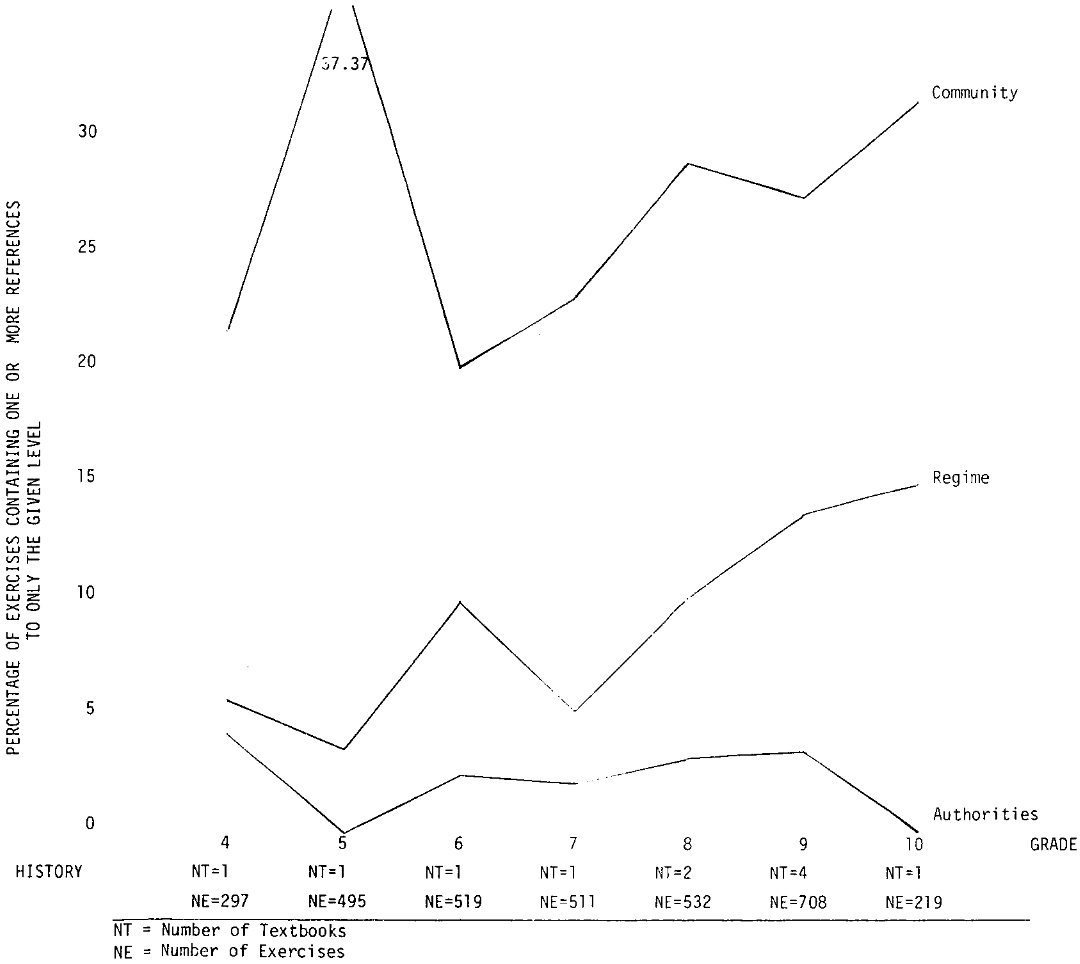

Figure 3. Emphasis upon Community, Regime, and Authorities: Overall

a word nor the frequency of its usage in the exercises was taken into consideration in the construction of the subdictionaries. Nor was any attempt made to equalize the number of words in the subdictionaries.

Figure 3 displays one set of findings about the overall emphasis in the textbooks upon the three levels of a political system. Over the span of grades in which history is taught as a separate subject, political community receives more emphasis than political regime, which in turn tends to receive more emphasis than political authorities.[14] There is the same pattern of greatest relative emphasis upon political community if the target is specifically the Russian or Soviet political system rather than all political systems. These two patterns suggest an attempt to tie Soviet schoolchildren to political community—the most fundamental level of a political system.

While the emphasis upon political community and political authorities does not tend to change in one direction, the emphasis upon political regime tends to increase.[15] If the target is specifically the Russian or Soviet political system rather than all political systems, the pattern of developmental emphasis is the same. The increasing exposure to political regime takes place within the context of constantly high emphasis upon political community and constantly low emphasis upon political authorities. Thus, as Soviet schoolchildren progress through school, they are expected to develop an increasingly sophisticated orientation and predisposition toward behavior with respect to the structure, organization, and operation of government.

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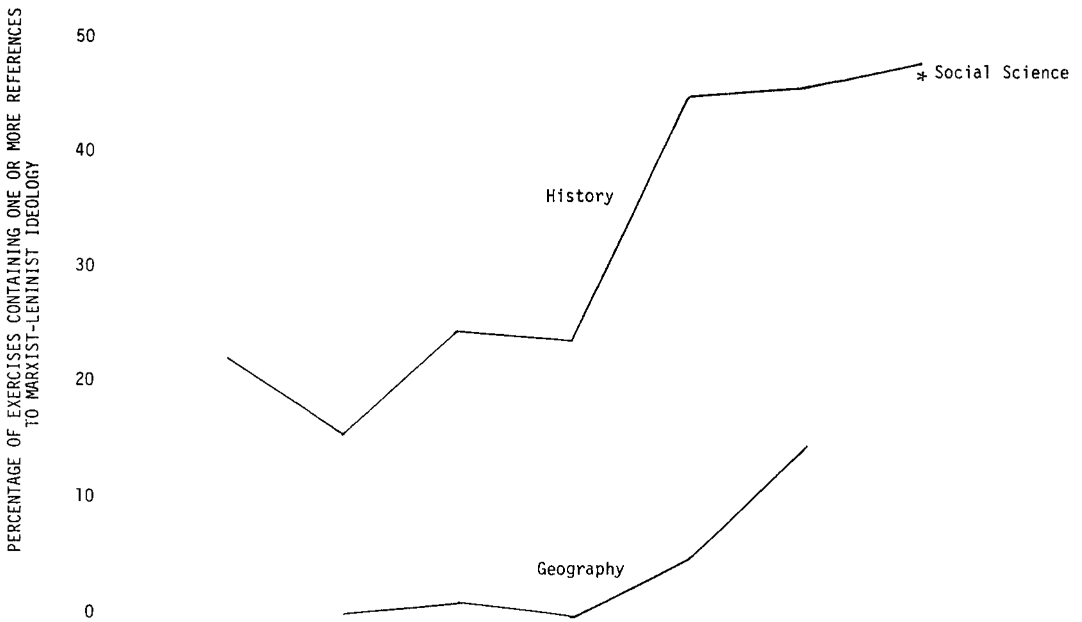
[14] This inference is supported by an analysis of the confidence intervals for the percentages of exercises containing one or more references to the separate levels of a political system.

[15] This inference is supported by the values of chi-square and chi-square-on-the-slope for the percentages of exercises containing one or more references to the separate levels of a political system.

Marxist-Leninist Ideology. The second example employing the technique of computer content analysis of transliterated Russian language textual materials deals with the emphasis upon Marxist-Leninist ideology in Soviet school history, geography, and social science textbooks. There are 5030 exercises in the seventeen textbooks required for instruction in these three subjects. Steps in the construction of a dictionary of words and short phrases referring to Marxism-Leninism were similar to the ones mentioned in the first example: preparation of a preliminary list of words and short phrases from an alphabetical listing of different stems appearing in the exercises; verification of the ideological meaning of these words and short phrases in recent editions of *Kratkii politicheskii slovar' (Short Political Dictionary)* and/or *Filosofskii slovar' (Philosophical Dictionary);* and elimination of those words and short phrases which did not have an ideological meaning or which were used ambiguously in the exercises. The final dictionary included 83 words and short phrases.

Figure 4 shows one set of findings about how Marxist-Leninist ideology is emphasized in the three subjects. In the fifth through ninth grades, i.e., over the span of grades in which both history and geography are taught, Marxism-Leninism receives more emphasis in the textbooks for history than in those for geography. In the tenth grade, i.e., in the single grade in which both history and social science are taught, there is approximately the same emphasis upon Marxism-Leninism in the textbooks for both subjects.[16] These patterns of relative emphasis suggest the varying roles of different subjects in inculcating a Marxist-Leninist world outlook. The role of geography is less than that of history and social science. The fact that the textbooks in history, a standard subject, emphasize Marxism-Leninism as much as those in social science, an overtly indoctrinating subject,



	4	5	6	7	8	9	10	GRADE
HISTORY	NT=1 NE=297	NT=1 NE=495	NT=1 NE=519	NT=1 NE=511	NT=2 NE=532	NT=4 NE=708	NT=1 NE=219	
GEOGRAPHY		NT=1 NE=471	NT=1 NE=327	NT=1 NE=267	NT=1 NE= 91	NT=1 NE=530		
SOCIAL SCIENCE							NT=1 NE= 63	

NT = Number of Textbooks
NE = Number of Exercises

Figure 4. Emphasis upon Marxist-Leninist Ideology in History, Geography, and Social Science Textbooks

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[16] These inferences are supported by an analysis of the confidence intervals of the percentages of exercises containing one or more references to Marxist-Leninist ideology.

underscores the importance of the former's role.

The emphasis upon Marxist-Leninist ideology in history and geography textbooks increases with grade; and the average emphasis upon Marxism-Leninism in the textbooks for each subject is greater in the higher grades (8-9/10) than in the middle grades (4/5-7).[17] These patterns of developmental emphasis suggest that Soviet school children are expected to acquire an increasingly sophisticated Marxist-Leninist world outlook and that this acquisition may be proceeding in a series of more or less well-defined stages.[18]

Conclusion

This note has described a technique of computer content analysis of transliterated Russian language textual materials and illustrated the use of this technique with two examples. The technique is promising for researchers who must study Soviet politics and political life at a distance and who consider analysis of documents an appropriate methodology for their studies. For the researcher who is already accustomed to careful analysis of documents, the technique enhances his or her ability to conduct objective and systematic analyses of them. It is now practical to use the technique described in this article; developments in other areas (e.g., perfection of a system for encoding through optical scanning) will make the technique even easier to use in the future. In addition, the eventual classification of the affective meanings of Russian words will permit the construction of even more sophisticated dictionaries for use with the technique.

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[17] These inferences are supported by regression analysis and analysis of variance of the percentages of exercises containing one or more references to Marxist-Leninist ideology.

[18] The passage from the seventh to eighth grade corresponds with the transition between two periods of the psychological development of the Soviet child. For a discussion of these periods, see L. I. Bozhovich, *Lichnost' i ee formirovanie v detskom vozraste (Personality and Its Formation in Childhood)* (Moscow: Izdatel'stvo 'Prosveshchenie', 1968).

Lobbyists and the Legislative Process: The Impact of Environmental Constraints*

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In the recent past we have seen a number of disparate studies describing the legislative process. The studies differ in the relative significance attached to committee and floor activity in Congress. Berman, for example, devotes considerable attention in his study of the 1960 Civil Rights Act to action and maneuvering on the floor.[1] Floor action in this case was a crucial aspect of the process and a serious potential obstacle to enactment. Since the time of Wilson, however, the congressional process has been described as being dominated by committees.[2] Leroy Rieselbach, for example, argues that "the central fact is, of course, that the autonomous committees make the major choices and that these are merely ratified at a later point in the proceedings by the entire chamber."[3]

Clearly, neither picture comprehensively portrays congressional action. Recent data from a study by Richard Fenno shows comparative committee success in getting favorable floor action. Fenno found that the Ways and Means Committee has managed to get more than 90 per cent of its major legislation passed by the House, while Education and Labor has had a success rate of only about 60 per cent in the recent past.[4] While both committees were more likely to be successful than not, one did considerably better than the other.

Studies of the relationships between interest groups and legislators differ about the nature of interaction between them. Milbrath argues, for example, that lobbyists generally use direct methods of contacting legislators (such as through personal presentation of viewpoints and research findings or through testimony at hearings). Yet he and others have also acknowledged that lobbyists at times use indirect approaches to congressmen (i.e., through constituents).[5]

To explain the variations in patterns of interaction discussed above, a number of political scientists have provided theoretical overviews of legislative policy making and the role of interest groups in it. Theodore Lowi, for example, argues that legislation can be classified according to its expected impact on society. The "arenas" he posits are said to show distinctive "political structure, political process, elites and group relations."[6] Lowi argues that one may distinguish three kinds of policies. Those of narrow impact are made without respect to resources and are called *distributive* legislation. Policies of somewhat broader impact, involving choices about who will be deprived or indulged, are called *regulative.* Finally, those of still broader impact, affecting social classes or races, for example, are called *redistributive.* It is doubtful, however, whether Lowi's categories can be operationalized.

One difficulty comes in attempting to distinguish between arenas. Determining how broad an impact an issue has can be difficult. Some actors involved in policy making see the impact of issues differently than others. Take, for example, the question of freight rate setting. Is the impact of that matter to be judged as involving competing claims of operators of different modes of transportation, or is it to be judged as primarily a controversy between users and transporters? If the issue involves users, which ones are most important? Differing perceptions of issues are inescapable. Bauer,

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*The author is indebted to Thomas Patterson and Robert deVoursney for their criticisms of the author's doctoral dissertation, from which this article has been adapted. The author is also indebted to Eugene Lewis, David Rosenbloom, Robert Kweit and Jeffrey Ross and two anonymous editorial readers for their criticisms and advice. Financial support was provided by the Syracuse University Department of Political Science and the Huber Foundation through Kirkland College. Computing was done using the facilities of Hamilton and Kirkland Colleges and Cornell University.

[1] Daniel Berman, *A Bill Becomes A Law* (New York: Macmillan, 1962). See also Eugene Eidenburg and Roy D. Morey, *An Act of Congress* (New York: Norton, 1969). In describing the enactment of the Elementary and Secondary Education Act of 1965, the authors describe in detail floor action on crucial amendments raised by opponents.

[2] Woodrow Wilson, *Congressional Government* (New York: World, 1956), pp. 57–98.

[3] Leroy Rieselbach, *Congressional Politics* (New York: McGraw-Hill, 1973), p. 237.

[4] Richard Fenno, *Congressmen in Committees* (Boston: Little-Brown, 1973), p. 237.

[5] Lester Milbrath, *The Washington Lobbyists* (Chicago: Rand-McNally, 1963), pp. 209–252, 328–354.

[6] See for example, Theodore Lowi, "American Business, Public Policy, Case Studies, and Political Theory," *World Politics,* 16 (July, 1964), 677–715. More recently, Lowi has proposed a refined version of this argument. See Lowi, "Four Systems of Policy, Politics and Choice," *Public Administration Review,* 32 (July/August 1972), 298–310. Also see Robert Salisbury, "The Analysis of Public Policy: A Search for Theories and Roles," in *Political Science and Public Policy,* Austin Ranney, ed. (Chicago: Markham, 1968).

Pool, and Dexter, for example, point out that some actors involved in the free trade controversy primarily perceived it in terms of the impact of low tariffs on workers in their particular constituencies, while others saw it in terms of its impact on the position of the United States in its trade relations with other nations.[7] If policy makers cannot agree on the impact of issues, it seems unlikely that a classification based on impact can be operationalized.

Although "impact" does not appear to be useful as an explanation of interactions among policy makers, it suggests a fruitful direction for further inquiry. Clearly, the environment of legislative decision making varies in different policy areas. Fenno, for example, argues in *Congressmen in Committees* that

> if one is searching for the antecedents [of the influence outsiders have on committee members] one finds that the subject of the policy and its associated characteristics must be given a central place. We have compared the relative prominence of four categories of interested outsiders. But we have found again and again, that similarities and, more often, differences in their interest and prominence are related to the policy area itself.[8]

To understand why different kinds of legislation are treated differently, we should look at variations in political processes outside the legislature that affect the way demands are placed on legislative policy makers.

One way in which the environment of legislative policy making varies is the process by which issues come to Congress for action. Some issues, for example, are used by presidential candidates in public speeches and statements in attempting to put together a winning coalition. If these are reported by the media, the public (or some part of it) is likely to become aware of the fact that an issue is being discussed among political decision makers. Whether these issues are raised originally by interest groups, by political candidates, by the media, or by other means is not relevant. What is important to the political decision maker is that they have been brought to the public by candidates through the media and that the public is presumably aware of and concerned about them. The salience of these issues to the reelection of candidates and to the political party in the legislature is likely to affect legislative behavior. Further, once elected, the victorious candidate develops a program for congressional action which he works to get passed on becoming head of the administration. Issues used by presidential candidates have an impact on the whole decision-making system. Issues used in individual congressional campaigns usually have little electoral impact on other members of Congress. Thus, issues raised by presidential candidates will be called "campaign issues."

Other issues are brought directly to legislators and bureaucrats without going through the electoral process. These issues are settled with little public participation. By far the greatest number of governmental decisions fall within this category. These issues are primarily raised and defined by interest groups and the executive branch. They will be called "group issues."

A second relevant characteristic of the environment that affects the way that demands are placed on legislative policy makers is the controversiality of the issues involved. Some issues, such as pork-barrel programs and veterans benefits, have traditionally involved no opposition from other groups in the legislative environment. These "noncontroversial" issues do not place decision makers in the position of choosing among the demands of different groups in society. Although candidates for the presidency may attempt to exploit noncontroversial issues in their attempts to win office, these issues are less likely to be the subject of media coverage than others because stories about agreement are not as dramatic or "newsworthy" as areas of conflict. Groups seeking governmental action on such issues are not likely to use the electoral process because it is slow, expensive, and not likely to be necessary for the enactment of programs that have no organized opposition. Therefore, noncontroversial issues are likely to be group-defined rather than campaign-defined.

Other issues force decision makers to choose between alternative group claims. In dealing with these "controversial" issues, some groups may seek to work through the electoral process in attempting to get them satisfactorily resolved. McConnell, however, points out that groups will ordinarily seek to avoid raising issues in the electoral process because of the time, expense and uncertainty involved in it. Financial and organizational costs are likely to be heavy and the outcomes of such involvements are likely to be reached slowly. Nevertheless, under some circumstances some groups are likely to find that a group-defined settlement is more costly than electoral participation.[9] Thus, controversial issues may be group- or campaign-defined.

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[7] Raymond Bauer, Ithiel de Sola Pool and Lewis A. Dexter, *American Business and Public Policy* (New York: Aldine, 1963), pp. 444–458.

[8] Fenno, *Congressmen in Committees*, p. 45.

[9] Grant McConnell, *Private Power and American Democracy* (New York: Alfred A. Knopf, 1966), pp. 298–335.

Not all organizations are likely to find that they are equally able to raise campaign-defined issues. Thus, organizational resources are likely to affect decision makers' interactions. While such resources as money, status and skill are likely to affect the success of all groups seeking policy decisions from the government, group membership has a particular impact on the ability of groups to have issues raised in campaigns. Groups with large memberships can use them to generate an appearance of public concern about issues. Their size makes them important to candidates seeking political office. As a result, several mass membership groups have developed partisan ties. These groups have gained privileged access to party platforms in return for their support and activity in electoral campaigns. Labor unions and farm groups are good examples of groups that have taken this route.[10] We may expect to find, then, that mass membership groups will be involved most frequently in campaign-defined issues. They should also be likely to participate in group-defined situations, because of the advantages of avoiding campaigns. Nonmass groups should be likely to avoid campaign issues, only participating when they are forced to.[11] They would be even more likely to deal with issues on a group-defined basis.

Three variables in the decision-making environment have been identified: (1) the definition of issues (group- or campaign-defined), (2) the controversiality of issues, (3) membership resources. This paper deals with the impact of these variables on the perceptions of legislative interactions held by lobbyists. Hypotheses will be derived that deal with the focus of lobbying activity and the techniques used by lobbyists.

From the model posited above, we may draw the following hypotheses: (a) Lobbyists dealing with group-defined, noncontroversial issues should deal with them mostly while those issues are in committee. (b) Lobbyists dealing with campaign-defined controversial issues should be most oriented to floor activity. (c) Those dealing with group-defined, controversial issues should fall between the other two groups of lobbyists in committee and floor orientation.

These hypotheses should be valid because the main problem facing groups interested in noncontroversial issues should be getting the committee to act. If a bill is reported out, they do not have the problem faced by groups dealing with controversial issues—that the legislation may reflect the objectives of other groups with opposing interests. Therefore, noncontroversial legislation should be reported by the committee in a form which reflects the preferences of interested groups. In contrast, controversial issues should involve more frequent appeals from committee decisions. Campaign-defined issues are most likely to be appealed to the floor. All members of Congress are likely to be aware of and concerned about them because of their electoral impact. In contrast, congressional norms such as specialization and deference to committee decisions are more likely to operate with group-defined issues because of the smaller significance of such issues to most members.

Second, because the focus of legislative interactions should vary with respect to the nature of the legislative environment, we may expect that techniques employed by lobbyists will also vary. Lobbyists dealing with controversies settled in committees are most likely to use techniques most appropriate in the committee setting, while those dealing with legislation which involves floor controversy are likely to use techniques suited to that situation. Consequently, we may hypothesize that lobbyists dealing with noncontroversial issues will rely most on techniques appropriate in committee, while those dealing with campaign-defined issues will rely most on techniques appropriate for floor action. We may expect to find that lobbyists who deal with group-defined controversies fall between the other lobbyists in the use of these techniques.

In addition, the congressional personnel contacted by lobbyists should vary depending upon the kind of conflict involved. Noncontroversial issues are so routine that we may expect that they will be dealt with primarily by committee staff, while lobbyists dealing with more controversial issues, particularly campaign-defined ones, should approach congressmen themselves in order to make sure that their messages get through.

Finally, we may expect that the use of constituent contact will vary depending on group resources. Groups with large memberships are most likely to use techniques that rely on mass contacting of legislators in attempting to keep them aware of public concern about these issues. In contrast, lobbyists representing nonmass organizations are likely to rely more on individually influential members of the constituency in attempting to affect congressional decisions.

The Data Base

To examine these questions, a sample of 118 lobbyists was questioned about their lobbying techniques and perceptions of the legislative process during the period from January through June 1971. Forty-seven were interviewed at random, and 71 responded to a mail questionnaire sent to the remainder. Thus, 72 per cent of the population of 163 eligible for inclusion in the study responded.[12] Lobbyists reporting less than $1000 per quarter in income from lobbying were excluded from the study to eliminate those who had little interaction with other policy makers. Those not reporting a Washington, D.C. address were excluded on the same grounds.[13] Appendix B shows the types of organizations represented by the lobbyists studied.

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[10] McConnell, *Private Power and American Democracy*, pp. 196–245, 298–335; also see J. David Greenstone, *Labor in American Politics* (New York: Random House, 1969), pp. 39–89, and Milbrath, *Washington Lobbyists*.

[11] E. E. Schattschneider makes essentially the same argument in *The Semi-Sovereign People* (New York: Holt, Rinehart and Winston, 1960).

Lobbyists were asked, "What legislation have you, in representing your organization, been primarily interested in during the past year?"[14] From their responses, the lobbyists interests were classified as noncontroversial; group-defined, controversial; or campaign-defined, controversial.[15] Controversial and noncontroversial issues were differentiated by reference to the *Congressional Quarterly Almanac, 1970* to determine whether or not opposition to them was present. Reports of activity at congressional committee hearings on each piece of legislation were examined to discover whether opposing positions on legislative proposals were taken by those testifying before the committee. Campaign-defined issues were found by referring to the 1968 *New York Times Index* for speeches made by the presidential candidates during the general election campaign.[16] Primary activity was excluded from the study, as were platform promises. Statements made during the presidential primaries do not relate so much to conflict between the significant organizations within Congress and in its environment that organize legislative coalitions; the political parties and the administration; as they do to conflict within them. Platform promises are not good indicators of issues where the scope of conflict is so substantial as to include the public because they contain many elements which merely represent promises to loyal constituents and are consequently not thoroughly discussed by the media. In addition, the content of platforms represents compromise among party factions, not the campaign agenda of the individuals who hope to become the next president.

Mass membership groups were differentiated from nonmass groups by classifying those organizations whose members were other organizations (such as trade associations or businesses) as nonmass organizations, while those whose members were individuals (such as trade unions, farmers and veterans groups) were classified as mass organizations. See Table 1 for the list of issues included in each category.

Findings

The Focus of Legislative Activity. The first hypotheses held that lobbyists dealing with campaign-defined controversies would be most floor oriented, while those dealing with noncontroversial issues would be most committee oriented. Three indicators were examined to test the hypotheses. First, the lobbyists were asked, "How much time do you spend on legislation at the committee stage as opposed to when it reaches the floor?" In addition, they were asked: "How often do you have contact with members or staff of the House Rules Committee?" and "Has the House Rules Committee blocked any legislation that you have been interested in?" The second two measures were included because contact with the Rules Committee follows committee action. Therefore, greater contact indicates a shift in orientation away from the substantive committee.[17] Evidence from the first measure, "How much time do you spend on legislation at the committee stage as opposed to when it reaches the floor?", supports the hypotheses (See Table 2).

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[12] The organizational memberships of those who did not respond were similar to those who did.

[13] *Congressional Record, 91st Congress,* Volume 116, pp. H2970-H2991, H7337-H7365, H12432-H12436.

[14] *Congressional Quarterly Almanac,* 1970 (Washington, D.C.: Congressional Quarterly, 1971).

[15] This method of measuring respondents interests could conceivably generate measurement error, since the interests of some lobbyists may fall in more than one category. To investigate the possibility of this sort of error, responses to the question were examined. Eighty-eight per cent of the respondents answering the open ended question answered exclusively with subject areas defined as falling within only one category. Thus, the activities of lobbyists appear to be rather specialized, and the amount of error introduced by this method of measurement should be small.

[16] *New York Times Index,* 1968 (New York: New York Times, 1969). Statements made by a candidate in response to questions posed by the *New York Times* were excluded from the tabulation of campaign issues because they were responses to questions from a specific organization rather than attempts to inform the public of his position initiated by the candidate. Thus, they may not represent areas emphasized in the campaign. In addition, because they were initiated by the *Times,* responses in these areas are particularly unlikely to represent the issues covered by the media nationally.

[17] Although questions were asked in the same form in both the interviews and the questionnaire, those from the interviews allowed open ended responses, while those from the questionnaire were closed. Responses from the interviews were coded twice, independently. See Appendix A.

Lobbyists dealing with noncontroversial issues reported overwhelmingly that their activities are concentrated in committee. On the basis of evidence gathered in interviews it appears that lobbyists representing groups with these interests develop close ties with the committees that they work with. Once the committees have acted, their recommendations are usually accepted with little conflict. This is unsurprising given the absence of outside groups acting in opposition to noncontroversial legislation.

Table 1. Lobbyists' Issue Interests

Noncontroversial Issues

- Veterans Programs
- Industry Subsidies (Merchant Marine, Airport)
- Public Works
- Research Funding (Health)

Group-defined Controversial Issues

- Civil Service Pay and Benefits
- Postal Pay and Benefits
- Transportation Regulation (Trucking taxation and regulation, Railway safety, Highway safety, Waterways transport)
- Foreign Trade Legislation
- Bank and Savings and Loan Regulations
- Consumer Legislation (Truth in Lending, Insurance legislation, Food inspection)
- Business Regulation (Anti-trust)
- Securities Industry Regulation
- Communications Regulation (Cable TV)

Campaign-defined Controversial Issues

- Welfare legislation (Welfare reform, Manpower retraining, War on poverty, Medicaid)
- Health Legislation (Medicare, National Health Insurance)
- Social Security Legislation
- Civil Rights Legislation
- Education Legislation
- Farm Legislation
- Vietnam
- Cities
- Labor-Management Relations
- Housing Legislation
- Taxation
- Environmental Legislation

Another reason the recommendations are accepted routinely is that the legislation usually has a rather narrow impact. Consequently, it is of little concern to policy makers who do not specialize in the particular area involved.

While committee relationships appear to be stable, one factor emphasized by these lobbyists as a key variable in leading to routine floor approval of bills is the attitude of the committee chairman about the legislation being considered. Since the committees act cohesively in dealing with these issues, their chairmen have a great deal of power in dealing with noncontroversial legislation. This can cause difficulties for outside groups at times:

> Over on the House side, Chairman * * * is a real strong chairman. He had some misunderstanding with the * * * agency a few years back when the Nixon administration first came in on matters completely unrelated to our program. . . [As a result,] Mr. * * * just didn't care a hoot about anything to do with the program. For that reason, and I think for that reason alone, we didn't get any action in the last Congress on bills that were important to us, even after they passed the Senate.

In most cases, lobbyists interested in noncontroversial legislation let the committee chairman guide the legislation through the subsequent procedures leading to passage. Thus, the job of lobbyists interested in this kind of legislation does not often involve contacting congressmen in efforts to insure its passage once it reaches the floor. Passage is usually routine. One lobbyist pointed out that

Table 2. Focus of Lobbyists' Activities

	Type of Controversy			
Focus of Activity	Noncontroversial	Group-Defined Controversial	Campaign-Defined Controversial	Total
Almost entirely committee	17.6% (3)	18.8% (9)	7.7% (3)	14.4% (15)
Mostly committee	82.4% (14)	62.5% (30)	43.6% (17)	58.7% (61)
About equal or mostly floor	0% (0)	18.8% (9)	48.7% (19)	26.9% (28)
Total	16.3% (17)	46.2% (48)	37.5% (39)	100% (104)

$\chi^2 = 17.88$
D.f. = 4
Sig. = 0.01

> aside from [writing letters pointing out the value of the program], to come down to the popular notion of a lobbyist running around the Hill, buttonholding Congressmen saying, 'I demand your vote,' we've never had to do that.

Lobbyists dealing with group-defined controversial issues were more likely to report that they spend as much time dealing with legislation once it reaches the floor as they do when it is in committee. Nevertheless, although floor activity in this arena is significantly greater than in the case of noncontroversial legislation, most lobbyists' activities are concentrated in committee.

The committee writes the basic legislation which will be considered by the whole House or Senate. It chooses among a number of possible courses of action in dealing with each aspect of a piece of legislation. For this reason these lobbyists find the committee to be the best place to work for legislation that conforms to their interests. The point was made by this lobbyist:

> We pretty much let legislation go once it gets out of committee, unless there is something in it that's particularly difficult. I'd say today that by the time a bill gets to the floor, the compromises and the amendments have been pretty well agreed upon. Once in a while when you get a real nationally controversial issue like the SST funding, then the floor vote is very important.

In itself, the fact that the committee performs the function of putting proposed legislation into the shape in which it will be considered on the floor would not explain why the actions of these lobbyists are so concentrated in committee. Other factors combine to make attempts to change legislation on the floor difficult and often futile for them. Lobbyists dealing with group-defined controversies perceive that other members of Congress usually defer to committee actions. One lobbyist pointed out that

> once a bill clears committee the battle is usually four fifths done, because they have a habit over there of backing up their committee actions in both houses. The main battle is to get proper appropriate legislation out of the committee. . . . Once that happens, you don't have any problem. Once in ten times there'll be a floor fight. At that point you have to work with the entire Congress.

In addition, lobbyists note that working with the entire Congress can be difficult. The simple structural problem of the number of members who must be contacted on the floor can create serious difficulties for lobbyists who represent groups with limited resources.

Thus, most of these lobbyists deal primarily with congressional committees. The strategic position of committees and the deference which lobbyists perceive that members pay them, along with the difficulty of dealing with the large number of members who must be contacted to change legislation once it leaves committee, lead them to attempt to shape legislation before it reaches the floor, where possible.

Lobbyists dealing with campaign-defined controversies are considerably more floor oriented than those dealing with other kinds of legislation. Even though they are substantially more floor oriented than others, none reported that their activities are primarily concentrated on the floor, and a substantial number reported that their activities take place mostly at the committee stage. The committee, even here, is extremely important because of its role in shaping the content of legislative proposals. On the other hand, many of them reported that they spend as much time dealing with legislation once it reaches the floor as they do when it is in committee. One puts it this way:

> Obviously you spend as much time after you get out of committee. What good does it do you to concentrate on committees? No bill has ever passed in committee. You have to take it from committee to get it passed on the floor and through conference.

Clearly, these lobbyists face many of the same problems seen by those dealing with group-defined concerns. Yet at the same time, floor action is an important access point for them, unlike for other lobbyists:

> We concentrate in both areas, but I would think that in the committee stage the concentration is on dealing with the legislative concept when it is still jelling. The concentration when you get on the floor is with the reality of what the committee reported out. So that you really are dealing with two different concepts of concentration and development. Sometimes as far as success goes, there's greater success at the committee level. Other times it's at the floor stage.

In addition to the comparative measure of committee and floor activity, a second indicator of a shift in the focus of congressional decision making from the committee to the floor in campaign-defined controversies is greater contact with the Rules Committee on the part of lobbyists dealing with those issues. With this increase in contact, lobbyists dealing with campaign-defined issues should also report increased difficulties in getting legislation scheduled for floor action.

The data supported the hypothesis that argued that lobbyists dealing with campaign-defined controversies would report greater difficulties in getting legislation scheduled than those concerned with other issues (see Table

3). Lobbyists dealing with noncontroversial issues most often reported that they have no problems with the Rules Committee. Those dealing with group-defined issues reported difficulties somewhat more frequently while more than 70 per cent of lobbyists dealing with campaign-defined issues encountered problems.

Lobbyists dealing with campaign-defined issues were likely to encounter problems for several reasons. One problem for them is that the Rules Committee works as an agent of the House leadership. A second problem was that at the time of this study, under Chairman Colmer the Rules Committee was an important access point to groups interested in preventing the enactment of legislation in the area of civil rights. One lobbyist pointed out that

> on some of the civil rights legislation it has been a problem. We lost several pieces last year at the end, just because we couldn't get a discharge petition. I don't sense that other legislation had quite the difficulty. It's just that Colmer is the Chairman and puts his stamp on it.

Other lobbyists stated that this problem is less severe than it was prior to the enlargement of the Committee.

Finally, these lobbyists noted that the Committee serves to protect members who feel that they cannot publicly oppose legislation themselves because of constituent pressure, but want to see it blocked anyway. It can protect the party leadership in the House in a similar fashion. As one lobbyist pointed out,

> The Rules Committee serves a lot more than its public image might lead one to expect. It serves the private interests and the private wishes of a good many members of Congress that aren't always exposed to the public for its immediate consideration. An individual may have terrific pressure from his constituency to get a piece of legislation out. The legislation may go through the committee and get to the Rules Committee but the member can call upon the Rules Committee Chairman to in some manner delay or see that it doesn't come up or keep it from being scheduled. Personal commitments are made so that he will not be in any way criticized for holding this up. So these sort of things are not matters to which the public has an insight, but yet they are matters which affect legislation.

The hypothesis dealing with frequency of

Table 3. Legislation Blocked by the Rules Committee

	Type of Controversy			
	Noncontroversial	Group-Defined Controversial	Campaign-Defined Controversial	Total
Yes	31.3% (5)	45.7% (21)	73.5% (25)	53.1% (51)
No	68.8% (11)	54.3% (25)	26.5% (9)	46.9% (45)
Total	16.7% (16)	47.9% (46)	35.4% (34)	100% (96)

$\chi^2 = 9.79$
D.f. = 2
Sig. = .01

Table 4. Lobbyists' Contact With the Rules Committee

	Type of Controversy			
Frequency of Contact	Noncontroversial	Group-Defined Controversial	Campaign-Defined Controversial	Total
Frequent	5.9% (1)	28.3% (13)	29.4% (10)	24.7% (24)
Occasional	29.4% (5)	21.7% (10)	44.1% (15)	30.9% (30)
Rare or Never	64.7% (11)	50.0% (23)	26.5% (9)	44.3% (43)
Total	17.5% (17)	47.4% (46)	35.1% (34)	100% (97)

$\chi^2 = 10.53$
D.f. = 4
Sig. = 0.03

contact with the committee was also supported by the data. Lobbyists dealing with noncontroversial issues were more than twice as likely to report that they had little or no contact with the committee as were those dealing with campaign-defined controversies (Table 4). Nevertheless, evidence from the interviews indicated that lobbyists dealing with all kinds of issues were not likely to have much direct access to the Rules Committee because it operates primarily as an agent of the House leadership, and because the Committee has isolated its proceedings from outside influence.

> It has never been my experience that this organization or any other outside organization has been able to influence very directly the agenda of items which the Rules Committee will or will not take up. That has usually remained in the complete control of the Chairman of the Rules Committee and to a lesser extent of the membership of the Rules Committee. These are men chosen not only for their qualities of party loyalty, but also for their experience in how legislation should be handled. So they are not by any means a Committee easily to be influenced in a casual manner from the outside.

Differences in Lobbyists' Techniques: Committee Specialization. The next hypothesis argued that the techniques employed by lobbyists would differ depending upon the nature of the controversy. I hypothesized that in dealing with noncontroversial issues, lobbyists would use techniques appropriate to influencing the committees. In handling campaign-defined matters, they should use techniques appropriate to influencing all of the Congress. In approaching group-defined controversies, lobbyists should be more likely than those dealing with nonconflictual issues to use techniques appropriate to influence all members of Congress, but less likely to do so than those involved in campaign-defined controversies.

Activities that lobbyists reported undertaking at the request of congressmen were examined. Such activities as submitting statements to congressional committees and testifying before them were classified as "committee specialized". Contacting congressmen or other interest groups to argue in support of a legislative position was classified as "nonspecialized."[18] While these techniques may be used in committee, they are not used exclusively there, as the others are.[19]

The data support this hypothesis. There were no significant differences between groups in the use of committee specialized techniques. Most groups reported both testifying and submitting statements to committees. In contrast, lobbyists dealing with campaign-defined controversies perform nonspecialized services more often than those dealing with noncontroversial issues and group-defined controversies (Table 5). Statements of lobbyists dealing with group-defined controversial issues showed that requests for support come most often when legislation is in committee. For example, one lobbyist pointed out that

> we will get one of their letters in. It will say, 'We are interested in this bill. Is there any way you can support it?' They will ask us to testify or to write a letter to the chairman of the committee saying that the * * * supports this legislation introduced by so and so.

Table 5. Use of Nonspecialized Activities by Lobbyists

Number of Activities Used	Type of Controversy: Noncontroversial	Group-Defined Controversial	Campaign-Defined Controversial	Total
None	0% (0)	8.0% (2)	4.3% (1)	5.1% (3)
One	54.5% (6)	32.0% (8)	4.3% (1)	25.4% (15)
Two	45.5% (5)	60.0% (15)	91.3% (21)	69.5% (41)
Total	18.6% (11)	42.4% (25)	39.0% (23)	100% (59)

$\chi^2 = 11.93$
D.f. = 4
Sig. = .02

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[18] Milbrath, *Washington Lobbyists*, calls these techniques "direct" and "indirect."

[19] See Appendix A for the exact wording of the questions. Responses taken exclusively from the mail questionnaire. Lobbyists were also questioned as to differences between the houses of Congress during the interviews. No significant differences were found in focus of activity or techniques used.

In responding to this question, lobbyists dealing with campaign-defined issues indicated that requests for support come at all points in the legislative process. For example, one lobbyist stated that

> if the request is in the initial stage, it's asking for our public endorsement. Later it may be with regard to helping to sell, and getting party sponsorship. Finally, we may be asked to provide help when it has reached the floor.

Whom Do the Lobbyists Contact? It was hypothesized that lobbyists dealing with campaign-defined issues would be most likely to attempt to deal with congressmen directly, while those dealing with noncontroversial issues would be most likely to deal with staff. The hypothesis was confirmed (see Table 6). The modal category for lobbyists dealing with noncontroversial interests was "mostly staff" while other lobbyists were more likely to reply "about equal." Only about 20 per cent of the lobbyists dealing with campaign-defined issues dealt mostly with staff members.

Getting a piece of noncontroversial legislation through Congress does not usually necessitate dealing with members.

> On something which has a narrow purview, like the bill to facilitate disposal of government-owned property, there are only a few members of either body who are interested in the legislation in the early stages and even after it goes through committee and onto the floor. It's rather routine if there is not opposition to it. You deal mainly with the staff on that sort of legislation.

Lobbyists with group-initiated interests reported that they dealt more with members of Congress and less with staff than did those representing interests with noncontroversial objectives. The greater controversiality of this legislation leads lobbyists to deal more with those having the ultimate decision-making powers than with those who deal with routine matters. One lobbyist made the point this way:

> I think probably on a time basis [we deal] more with the staff. But the times you are talking to a member, by then he has learned what you have given the staff. So on a time basis, you don't spend as much time with him, but you get as much done. For instance, he may have one or two questions that it has boiled down to that maybe you can supply the information about. When you start out with the staff man, it may be forty questions.

Lobbyists concerned with campaign-defined issues deal less with staff than do lobbyists in the other two areas. The greater contact that these lobbyists have with members occurs because of the greater controversiality and broader impact of campaign-defined issues. They involve larger segments of the public than do issues in other areas, and more conflict is associated with them. One lobbyist described the functions of staff and members in this way:

> We do talk to members of the staff a great deal to find out where things are in the process. They are not by and large partisan. So we look to them for information about what is going on. In terms of advocacy we approach the members directly or if we can't reach them that way we go through the personal staff.

Group Size, Issue Area and Contacting Techniques. The differing resources available to mass and nonmass groups would lead one to expect that nonmass groups would avoid the use of presidential campaigns in seeking their political objectives, while mass groups should use them more frequently,[20] as Table 7 demonstrates.

Table 6. Congressional Decision Makers Contacted by Lobbyists

	Type of Controversy			
Target of Contact	Noncontroversial	Group-Defined Controversial	Campaign-Defined Controversial	Total
Mostly Members	5.9% (1)	20.4% (10)	21.1% (8)	18.3% (19)
Members and Staff Equally	35.3% (6)	38.8% (19)	57.9% (22)	45.2% (47)
Mostly Staff	58.8% (10)	40.8% (24)	21.1% (8)	36.5% (38)
Total	16.3% (17)	47.1% (49)	36.5% (38)	100% (104)

$\chi^2 = 8.93$
D.f. = 4
Sig. = .06

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[20] Nonmass groups were distinguished from mass groups on the basis of whether the memberships of the groups studied were individuals or organizations.

Table 7. Organizational Characteristics

Nature of Controversy	Type of Organization		
	Nonmass	Mass	Total
Noncontroversial	15.0% (6)	18.2% (12)	17.0% (18)
Group-defined controversial	70.0% (28)	31.8% (21)	46.2% (49)
Campaign-defined controversial	15.0% (6)	50.0% (33)	36.8% (39)
Total	37.7% (40)	62.3% (66)	100% (106)

$\chi^2 = 16.29$
D.f. = 2
Sig. = .01

Only 15 per cent of the lobbyists employed by nonmass groups reported being predominantly involved in campaign-defined controversies, while 50 per cent of the mass-based groups were involved in such controversies.

Many observers of policy making have emphasized the attention congressmen pay to constituent contact which appears to be spontaneous. Lobbyists emphasize its use. Nevertheless, the differing problems faced by lobbyists with different resources leads to different uses of constituents. Lobbyists who represent mass organizations such as labor unions and veterans' groups can use the memberships of their organizations to write letters and send telegrams in an effort to make congressmen feel that they are facing issues of concern to the constituency.

In contrast, while the nonmass organizations (primarily businesses and trade associations) do not have access to this resource, their ranks consist of prestigious members of the community, an economic elite which has the status and financial resources to get access to the congressmen. In such groups we would expect a heavier reliance on the use of telephone calls from constituents to congressmen and visits by constituents in the congressional office.

Tables 8 and 9 support the hypothesis.[21] Lobbyists representing mass organizations used personal and telephone contact less and letters and telegrams more than those representing nonmass organizations. Thus, the resources available to nonmass groups appear to lead them to become involved in group-defined controversies and to use office and telephone contact predominantly, while mass groups use their membership resources more often to influence the outcome of campaign-defined controversies.

Table 8. Use of Office and Telephone Contact by Lobbyists

Number of Different Types of Contact Employed	Type of Organization		
	Nonmass	Mass	Total
None	11.5% (3)	37.0% (17)	27.8% (20)
One	50.0% (13)	28.3% (13)	36.1% (26)
Two	38.5% (10)	34.8% (16)	36.1% (26)
Total	36.1% (26)	63.9% (46)	100% (72)

$\chi^2 = 6.09$
D.f. = 2
Sig. = .05

Table 9. Use of Letters and Telegrams by Lobbyists

Number of Kinds of Contact Employed	Type of Organization		
	Nonmass	Mass	Total
None	42.3% (11)	6.5% (3)	19.4% (14)
One	34.6% (9)	30.4% (14)	31.9% (23)
Two	23.1% (6)	63.0% (29)	48.6% (35)
Total	36.1% (26)	63.9% (46)	100% (72)

$\chi^2 = 16.49$
D.f. = 2
Sig. = .01

All kinds of lobbyists, however, emphasized the importance of using constituents to influence congressional decisions. For example, one reported:

> Most of our lobbying is done by letters, and through contacts with the Congress. Mostly through the committees. . . It's by letters, by correspondence. We send back to our members and ask them to contact their congressmen by letter or telegram because they're voting for them. If we walk in there from this office they ask where we are from and they aren't too interested, but if they are people from the lower echelon who call up, then it means something.

This is not the only reason lobbyists use constituent contact, however. Constituent contact is used by some organizations to give the membership a sense of participation.

> We do it for dual purposes. One is that we feel that keeping the constituent in touch with his member of Congress is an effective way of registering opinion. Second, we like to help keep in touch with the issue at hand so that he doesn't lose out and wonder at what point he missed the boat. With Congress being made up

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[21] Responses taken exclusively from the mail questionnaire.

> the way it is it gives a member of * * * or the membership of any organization a sense of real pride in helping out maybe some other section of the country or some other individual he has no real contact with.

Conclusions

In the research presented above, I have shown some ways that the legislative environment affects the behavior of lobbyists and their perceptions of the legislative process. Group characteristics and group relationships around specific issues affect legislative interactions. Legislative policy makers do not behave uniformly, regardless of issue.

Specifically, it was demonstrated that the legislative activity of lobbyists is most committee oriented in dealing with noncontroversial issues, while it is most floor oriented in dealing with campaign-defined, controversial issues. Lobbyists techniques reflect this difference in focus of activity. Nonspecialized techniques are used least by lobbyists dealing with noncontroversial issues and most by those dealing with campaign-defined controversies. Constituent contact techniques were shown to be related to group resources. Mass groups were found to rely more on letter and telegram campaigns, while nonmass groups rely on telephone and direct contact by a few influential constituents.

Although this study was limited to an examination of the impact of environmental characteristics on legislator-lobbyist interactions, a number of other observed discrepancies in the behavior or legislative policy makers would appear to be related to environmental characteristics. Committee integration, for example, has been shown in a number of studies to vary markedly. It seems likely that integration is at least in part a response to the ways that demands are placed on congressional decision makers.[22] Similarly, findings showing differing degrees of constituency influence in different subject areas are probably affected by the same variables.[23] We may also expect to find that interactions between policy makers in the Congress and those in the executive branch vary for the same reasons.

The findings of this study point up the significance of the presidential campaign in the legislative process. Interest groups dealing with campaign-defined conflicts no longer may monopolize the issues presented to decision makers. The techniques employed by them demonstrate the perceived necessity of engaging a large constituency in favor of their positions. At the point when conflict leaves the group-committee-bureaucracy setting, one may no longer argue that the condition of subsystem semi-autonomy is present. Clearly, some degree of accountability to the larger constituency enters the policy process at this point.

Equally significant, however, is the fact that relatively few of the issues studied here were campaign defined. Only about 35 per cent of the lobbyists in this study dealt with issues that fell in this category. While one could not argue that this sample is a representative cross-section of all the issues dealt with by Congress, it is clear that one condition for interest-group effectiveness—maintenance of conflict within a small subset of decision makers—is more often present than not. In non-campaign defined issues, the focus on committee activity reported by lobbyists can be seen as an indicator of the significance of committee subsystems in the decision-making process. Thus, the recruitment of committee decision makers and the decision-making process in committees should be important topics of study for those who wish to understand policy outcomes.

Appendix A. Questions Used in the Study

1. How much time do you spend on legislation at the Committee stage as opposed to when it reaches the floor? (check one) __Almost entirely in Committee __Mostly in Committee __About equal __Mostly on floor __Almost entirely on floor.
2. How often do you have contact with members or staff of the House Rules Committee? (circle one) *Frequently Occasionally Rarely Never*
3. Has the House Rules Committee blocked any legislation that you have been interested in? (circle one) *Yes No*
4. How often do members of Congress request your support on legislation that they have introduced? (circle one) *Frequently Occasionally Rarely Never*
5. If you have been requested to provide support for legislation, please indicate which of the activities you have or have not pursued? (circle *Y* (Yes) or *N* (No) in answering)

Y N Testifying before Committee
Y N Filing statement with Committee
Y N Contacting members of Congress
Y N Contacting other interest groups
Y N Informing your members of your support
Y N Other (specify)__________

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[22] Of course committee recruitment as well as leadership patterns affect integration, as well. See Richard Fenno, "The House Appropriations Committee as a Political System," *American Political Science Review*, 56 (June, 1962), 310–324 and *idem*, "The House of Representatives and Federal Aid to Education," in *New Perspectives on the House of Representatives*, ed. Robert L. Peabody and Nelson Polsby, 2nd ed. (Chicago: Rand-McNally, 1969), pp. 283–323.

[23] Warren Miller and Donald Stokes, "Constituency Influence in Congress," *The American Political Science Review*, 57 (March, 1963), 45–56.

6. Many lobbyists feel that getting constituents to contact their congressmen is essential to a successful legislative program. Which of the following do you encourage most? (circle *Y* (Yes) or *N* (No) in answering)

Y N Letters
Y N Personal contact in D.C.
Y N Personal contact in District
Y N Do not encourage constituent contact
Y N Telephone calls
Y N Telegrams
Y N Other (specify) ____________________

Appendix B.
Organizational Affiliations of Respondents*

	Number	**%**
Labor Unions (Government Employees)	11	10.5
Labor Unions (Other	25	23.8
Trade Associations	37	35.2
Ideological Organizations	5	4.8
Veterans Organizations	7	6.7
Farmers Organizations	10	9.5
Professional Associations	8	7.6
Individual Businesses	2	1.9
	105	100.0

*Affiliations of 13 respondents were not ascertainable.

APSA PRESIDENTIAL ADDRESS, 1977

Wellsprings of Political Leadership

JAMES MACGREGOR BURNS

Introduction

HAROLD D. LASSWELL
Yale University Law School

The distinguished colleague who delivers the presidential address this afternoon stands between most of us, his colleagues in political science, and the American electorate. He stands between us in a very different sense from an Iron Curtain that shuts off contact, or a campaign speaker who cuts off understanding. Jim Burns is an interpreter. He tells our fellow citizens what they want to know about American government and politics. So he picks up whatever he or any of us has been able to find out and puts it together in clear and interesting prose. He separates the wheat from the chaff and refrains from publishing the chaff.

From time to time people want to know about the constitution and the court and the legal system. Jim Burns and Jack Peltason put it together in a book that is well-enough received to keep deserving intellectuals well above the poverty line. At times people want to know about the ins and outs of American government and the party system. Jim has obliged. They want to see how it works and is worked by recent leaders. Jim has obliged.

The Burns technique has been to add field experience and observation to the ordinary diligence of scholarship. He explored at first hand the lives of individuals and institutions at war in World War II. He may or may not have met a payroll; most certainly Jim has faced an electorate. Besides earning the respect of professional political scientists he has won the confidence of professional pols and top-flight leaders. A frequent traveler, he has moved from country to country and confirmed the rumor that if Williamstown is not the peak of contemporary civilization it at least knows where the peaks are.

No matter how schematic may be the formal designs on which we draw in quest of new knowledge, no matter how sophisticated the data-gathering and -processing routines, a continuing test is contribution to what our fellow citizens want to know about American values, institutions, and leaders. President Burns has for years been an indispensable interpreter of professional curiosity and effort to the curiosity and needs of the responsible citizen.

Mr. President, we admire what you have done, and anticipate what you will do.

Wellsprings of Political Leadership*

JAMES MACGREGOR BURNS
Williams College

Tradition stretching back into the mystic origins of the American Political Science Association commands that the retiring president of the Association offer an address, which is later presented in the pages of the *American Political Science Review.* A few months ago I tried to challenge this tradition, as I have a few others in the Association, but in this case with less success. I proposed to the authorities a course of action—or inaction—that I thought might be most acceptable, in our over-verbalizing society; I proposed that I *not* give a speech and hence not do a paper. My manifest reason was to help solve the *APSR* backlog. A glow of noble self-sacrifice spread over me as I thought of some struggling young scholar who might appear in the *Review* because I did not. Another reason was less self-sacrificial. I have been working on general problems of political leadership for most of my scholarly life, and intensively during the last few years. I have not *quite* solved all the mysteries and complexities of the subject but expect to by May 1977, which happens to be the deadline for a manuscript on the subject. Perhaps a speech on the subject now would be a bit premature?

I went to the keepers of tradition in the Association with these irrefutable arguments, only to be told that not only custom but the remorseless and inexorable publishing schedule of *The American Political Science Review* would brook no alteration in timing or tradition. Besides, I was told, my colleagues would wonder why I did not speak. Vainly I argued that it was better that they ask why I did not speak than why I did. Then some choice bait was dangled in front of me. If I gave the address, I was reminded, some eminent political scientist could be invited to introduce me, and he might say unduly kind things about me. Someone of the eminence of Harold Lasswell, perhaps. To this seductive thought I instantly succumbed.

My modesty as an analyst of political leadership is by no means a false one, but I have my excuses. The state of the art is primitive, in my view. Political leadership is one of the most widely noted and reported and least understood phenomena in modern politics. If you doubt this, glance at the indexes of any of several hundred works that deal with political *leaders* but have little concept of, or reference to, the role of political leader*ship*. Moreover, any theory of political leadership must, in my view, be part of a more general theory of social or historical causation. Hence the student of political leadership must deal with the most refractory questions of political power and social change. Brilliant work has been done on countless aspects of political leadership. But we lack a general theory.

Because we lack a general theory of political leadership, we may be tempted to resort to the opposite strategy—the hypothesizing of a limitless assortment of psychological, social, and political variables in the shaping of political leadership and the making of history. This strategy would be the safest and the least rewarding. It would move analysis back from the endless manifestations of overt behavior—voting, for example, or rioting—to the multitudinous forces lying behind those behaviors and hence would pose the same questions of causation all over again, though in different guises. And it would raise the question of just where in the long causal sequence we should concentrate our analytical resources—for example, in the study of the formation of attitudes directly affecting behavior, or in the study of the far less visible but more profound interplay between leader and led that occurs in the earlier stages of the sequence, in the study of obscure but powerful biological, social, and psychological forces.

A related problem is also serious. Though we use the concept of sequence, and though we will see the causal forces operating largely in a certain direction over time (that is, toward time-identified elections, assassinations, revolutions), in fact the sequence consists of interacting dispositions, anticipations, and behaviors over time, and there is a good deal of causal feedback and circularity. Much of this circularity is neglected if we emphasize one or two stages in the sequence to the neglect of others.

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*This paper, presented as the Presidential Address at the American Political Science Association Meeting in Chicago, in September, 1976, is an exploration of sources of leadership that lie in areas that are to some degree outside the traditional boundaries of political science. It is excerpted and summarized from a larger work in progress. I solicit comments and criticism, addressed to me at Williams College, Williamstown, Massachusetts 01267.

We must be more audacious, if we are to locate the process of political leadership in a general theory of social causation. Audacious because, if we wish to understand the genesis of political behavior, we must probe psychological and social forces operant at several stages prior to the interlinked stages of derivative political behavior, governmental policy, and real social change. Audacious also because, amid the plethora of historical, psychological, environmental, and situational forces, for a general theory of leadership we must identify original and decisive sources of leadership—sources that I am calling here wellsprings of political leadership. To find these sources we must enter a darkling plain dotted by phenomena called habit, motive, trait, instinct, need, drive, emotion, feeling, wish, purpose, desire, expectation, aspiration, claim, demand. We find to our dismay that the rulers of this terrain—mainly psychologists—have been unable to establish clear boundaries among these entities or even to reach agreement on their definition.[1] In searching for our original and central variable, we are depressed by the flatness of the conceptual terrain, the lack of a commanding continental feature, the absence of that same central variable that might help transform a welter of social influences into a hierarchy of causal forces, in which the process of political leadership would have a significant and identifiable function.

Could we in our audacity supply that central variable? Such a variable conceptually would have to cut across a wide range of cultures and classes and polities, for it is a *general* theory of leadership in social causation that we are pursuing. It would have to embrace biological and social energies so powerful as to give a general shape and direction and tempo to the social and political forces subsequent to it in the causal flow. Where do we start? Where can we find original springs of human thought and action, the analysis of which will allow us to hypothesize and to generalize through space and time?

Wants, Needs and Expectations

The primal sources of political leadership lie in the vast pools of human energy known as wants, needs, aspirations and expectations. Of these overlapping terms, "want" and "need" in particular are usually treated as equivalents.[2] We begin with the term *want* rather than *need* because "want" over the centuries has meant the condition of persons directly experiencing the lack of certain things, such as food or drink, that stem from immediate tissue demands. The feeling of want is highly conscious, subjective, internal, autonomous, as with a month-old infant wanting nourishment; the want stems from a drive, a tissue demand, born into the child. *Need* in long-time English usage implies a more socialized, collective, objective phenomenon, in the sense of someone being "needy," in the views of others as well as herself requiring certain things. Needs are more widely sanctioned wants.

It is in the transformation of wants into needs that leadership first occurs. The wanting child is responding to a generalized drive born in him. He will want drink but will consume nutrition-lacking liquid as well as milk, will want food but eat poisoned candy as well as rice, will want to explore but touch a scorching andiron as readily as a rubber ball. Parents who insist on milk and rice and balls are substituting their own conception of the infants' needs for those of the infants, and they do so in the pursuit of aims and values that the parents rather than the children establish. This is the initial act of leadership, and a vital one.

Wants and needs and the elements into which they may be transformed may all be considered examples of motivation, a blanket term that has come to mean all variables from innate biological drives to the most refined and developed attitudes. It is these motivations, whether in their more primitive or refined form, whether in their more individual or collective manifestations, that the leaders tap on the basis of their own motivations as well. Wants serve as energizers of action rather than as guides to action. Motives are "pushed" by generalized drives and other body-bound forces and "pulled" by more specific wants, needs, aspirations, goals, and values. As they come to be activated less at the biological and more at the cultural stages, less in body-bound situations and more in socially influenced environments, motives are more likely to be dominated by "higher" needs, aspirations, and values, as noted below. Thus motives may come to be shaped increasingly, but never wholly, by the consequence—the satisfaction of need, the realization of purpose—to which behavior affected by the motive leads.

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[1] "Rather than finding *a* psychological science on which to draw for insight," the political scientist "finds a congeries of more or less competing models and frames of reference, with imperfect agreement on the nature of man's inner dispositions, the appropriate terms for characterizing them, and the methodologies for observation. . . . ," in the view of Fred I. Greenstein, "Personality and Politics: Problems of Evidence, Inference, and Conceptualization," *American Behavioral Scientist,* 11 (Nov.–Dec. 1967), 38–53, at p. 40.

[2] James C. Davies literally equates the terms want and need in his *Human Nature in Politics* (New York: John Wiley and Sons, 1963), pp. 396–7, 403.

We must discriminate among processes covered by the umbrella concept of motivation to glimpse the role of leadership at every stage in the sequence of transitions from the "pushes" of "lower" wants and needs to the "pulls" of higher aims and values. At the earliest and most primordial stage the phenomenon of want has typically been seen as a stimulus to the system by a tissue requirement external to it, leading to activity terminated by satisfaction of the requirement. The want is direct, internal, physiological, and to a degree undiscriminating; the tissue requirements of liquid is satisfied by drink in almost any form. Even at the stage of want a person will begin differentiating—the infant will doubtless prefer milk to whisky—but the differentiation stems almost wholly from inherited dispositions rather than from cultural influences on wants and needs. The physical structure of the person comes into play—motor abilities, for example, and sensory perceptual tendencies, and these capabilities increasingly make contact with environmental cues, responses, and resistances. This transition is marked by a shift from "I want" to "I want to. . . ." The source of action, though, is still the subject, responding to internal requirements.[3]

It is the deliberate process of selective encouragement and discouragement by others of a person's felt wants that brings into play the leadership process. The leader—parent, doctor, schoolmate—chooses to satisfy or encourage the expression of certain wants and discourage or deny others. Drives and wants remain the basic energizers, but the targets against which the wants are directed become more focused as wants give way to needs. The child's want for food becomes, under his mother's guidance, a need for nutrition; his want for freedom from pain becomes, under his doctor's prescription, a need for medicine. The leaders are those who direct the transformation and satisfaction of the wants arising from the original energizers.

If wants are subjectively felt drives, needs are socially influenced wants. Needs are shaped to a significant degree by impersonal environmental forces and by persons who themselves are caught in the grip of circumstance. Arendt refers to those "who desired liberation from their masters or from necessity, the great master of their masters. . . ."[4] Franklin D. Roosevelt reiterated that "necessitous men are not free men."[5] Within the workings of iron necessity, however, there is some play at the joints, and this provides the margin for leadership. Various leaders—parents, teachers, etc.—encourage and suppress feelings of need based on their perception of what the "needy" truly need and their ability and desire to affect the needy's behavior. Such actions serve to exacerbate or diminish needs; once the leader has curbed or reinforced or otherwise altered the followers' definition of their needs, behavior will tend to change accordingly, but this behavior is closely affected by further leadership.

Both the frustration of needs and their gratification may have a direct influence on the political environment. During the French Revolution, at a time when workers spent the bulk of their income on food alone and sometimes on bread alone, the housewives of Paris exerted an intense pressure in face-to-face confrontation with the Jacobin leadership in the capital; they would not long be put off by excuses and promises. Throughout history food-deprivation has led to fierce competition for limited resources, to massive migrations, wars of conquest, revolutions, civil strife—as well as to egalitarian ideas and movements.

Another powerful "lower" need is that for safety or security—for order and predictability, for protection against invasion, war, catastrophe, disease, against threats to one's territory or home, against crime in the streets. Karen Horney conceived the phenomenon largely in individual terms—as feelings of insecurity, especially on the part of infants and children, in the face of a hostile and dangerous environment, resulting in turn in neurotic needs of adults for affection, approval, and prestige.[6] This kind of individual need for security may strongly influence the personality and attitudes of specific political leaders, but the safety needs rising from a sense of collective insecurity can influence whole populations. "People generally do not turn to politics," Davies says, "to satisfy hunger and to gain love, self-esteem and self-actualization; they go to the food market, pursue members of the opposite sex, show friends what they have done, and lose themselves in handicrafts, fishing, or contemplation—with rarely a thought about politics. If achievement of these goals is *threatened* by other individuals or groups too powerful to be dealt with privately, people then turn to politics to secure these ends."[7] Moreover, the need for safety may be combined with other basic needs, such as that for food, and produce a multiplier effect on political leadership. That kind of combination helps explain such episodes as the massacres and other terrors of the French Revolution and the ferocity of combat in the Russian and other civil wars.

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[3] C. N. Cofer and M. H. Appley, *Motivation: Theory and Research* (New York: John Wiley & Sons, 1964), p. 80.

[4] Hannah Arendt, *On Revolution* (New York: The Viking Press, 1963), p. 129.

[5] *The Public Papers and Addresses of Franklin D. Roosevelt,* 1944–45 Volume (New York: Harper & Brothers, 1950), p. 41.

[6] Karen Horney, *The Neurotic Personality of our Time* (New York: W.W. Norton and Co., 1937.)

If the deprivation of food, drink, and perhaps of sex and security is politically consequential, paradoxically the *gratification* of such needs may be even more significant for the political process. According to need-hierarchy theory, *the satisfactions of basic needs releases persons from basic physical and psychic restraints* and *frees them to respond to higher needs.*

The first of these processes is the more apparent. No one is more enslaved—at the level of bodily need—than the person wild with thirst, deep in pain, desperate with hunger, driven by fear. No matter that it is his own body that imprisons him; necessity controls. Those economic, social or political leaders who can supply these needs become the masters of necessity. It is not a far step from this concept to Marx and the economic iron laws that are supposed to determine rewards and deprivations for whole classes and nations. When lower needs have been gratified, the second and more complex step in the liberation of humans from "lower" needs is the consequent capacity to satisfy the "higher" needs. These are the needs for belonging, for love, for esteem from oneself and from others, for self-actualization; the need to know; aesthetic needs beyond self-actualization.[8] While psychologists are broadly agreed on the major distinctions between lower and higher needs, they differ on the question of whether these higher needs are themselves hierarchically organized, from the lowest higher need, that of belonging, to the highest, self-actualization. This dispute need not divert us. The essential question is the extent to which the higher needs are common to all humankind and hence constitute a variable in theories of social causation and political leadership in all cultures. Do all humans share the need for love, for the esteem of themselves and of others, for self-actualization? The evidence for this is far less persuasive than that for the commonality of basic or lower needs. As a person satisfies his lower needs and becomes freer to respond to higher ones, he is liberated from biological urgencies and exposed to the needs developed in his socialization and learning processes. These manifestations of biologically determined needs will be also environmentally shaped.

Another crucial process in the alteration of needs is the change in the extent of leadership influence, which varies widely according to the qualities and purposes of the leader and follower, and the responses and restraints of the environment. To a degree the follower's needs become less egoistic and short-run and more consciously and deliberately defined as he moves out of the gross restraints rising from hunger, perceived threats to survival, and the like and as he both cultivates and becomes engaged with a broader, more diverse, more culture- and leader-influenced set of needs. This is not to say that leaders may not shape the manifestations of more basic needs, as well. In the endless interplay between leaders and followers each will be motivated by sets of needs. Followers may seek to enlist leaders' concern for their own needs, or turn to other leaders who promise to show more concern. At the political level, they may follow a Winston Churchill with enthusiasm during a time of need for security, and later reject him when social and economic needs seem likely to be paramount. But the process of selecting and rejecting begins much earlier, on the part of both leaders and followers. People usually will turn to leaders for specific remedies for specific problems, but people may also feel a need for a certain style of leadership, as Davies has emphasized. People select "characteristics within a broader framework which determines both the limits of leadership characteristics and of the awareness of the fact that certain characteristics are appreciated or depreciated," Davies says.[9]

And then another great transition. As the expression of needs becomes more purposeful—that is, as that expression becomes less subjective, egoistic, body-bound, direct and immediate, as it becomes more related to socially sanctioned aims and collective goals and values—needs are transformed into positive hopes and aspirations. Hopes emerge from needs but are closely influenced by leaders who arouse or dampen them. Hope—looking forward with desire and with belief in possibility—can readily be escalated into aspiration—the eager and ambitious desire of a higher goal—by leadership and other socializing influences. Studies of aspiration levels, such as subjective factors involved in the game of throwing darts, suggest that such levels are affected by leaders—for example, by the way in which experimenters influence aspiration levels by the phrasing of questions—as well as by more subjective factors such as anticipation of success. Leaders also, of course, can play on fears of failure.

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[7] Davies, *Human Nature in Politics,* p. 10.

[8] See the work of A. H. Maslow, esp. " 'Higher' and 'Lower' Needs," *Journal of Psychology,* 25 (September 1948), pp. 433–436; and *Motivation and Personality* (New York: Harper and Brothers, 1954).

[9] Davies, *op. cit.*, p. 291.

Leaders can, in turn, help convert hopes and aspiration into expectation. Expectations carry more psychological and political force than hopes and aspirations. They are more purposeful, focused, and affect-laden; the expectation is directed toward more specific and explicit goals, ones that are valued by the builder of expectations. They carry a greater air of legitimacy; people expect what is rightfully theirs and are provoked or outraged when they do not receive it. And they widen the margin of the perceived probable. All these factors, and especially the last, have two further effects. They cultivate a situation in which heightened expectations, confronted with lowered or zero realization, combine to produce an extreme sense of deprivation in people. And dashed hopes, blighted aspirations, and thwarted expectations are the materials for political exploitation. Thus theories of revolution have been based in large part on the broadening gap between what people expect and what they get. Violence becomes increasingly probable, Davies argues, when any basic need that has come to be routinely satisfied suddenly becomes deprived.[10] Revolution occurs, Gurr contends, when major segments of society perceive a discrepancy between their "value expectations" and the "value capabilities" of the environment.[11] Expectations are closely influenced by what leaders hold out as necessary, desirable, deserved, and possible.

Heightened expectations, crushed expectations—these are materials for political action and leadership. Expectations of government are easily converted into demands on government. And in these processes again leaders have a central part in shaping, articulating, and targeting popular demands. But typically their ability to shape and redirect is limited, for they are dealing—in these instances—not with transient popular attitudes but with demands emerging directly from hopes and needs and wants. They can redefine purpose, but their power of manipulating mutual goals is subject to the circumstance that followers' wants, as they are transformed into needs and aspirations, are more and more closely linked with followers' purposes and their own purposes or both.

The Structure of Values

To probe into the day-to-day thoughts, impulses, beliefs, biases, and preferences of humankind is to marvel at the grip that want and need hold on attitudes and behavior. Biology, culture, situational factors, and circumstance seem all but controlling. Yet we know too that some persons will transcend every conventional want and defy all ordinary needs to fulfill some overwhelming ideological motive. Men and women will slowly and deliberately starve themselves to death—they will override the most powerful and primitive instincts of survival—to protest a wrong or champion an ideological, or even a highly parochial, cause. They will endure years of suffering in gas-filled trenches or on icy battle plains, suppressing every want and need in their bodies, and then march to their deaths in the cause of God or Fatherland or Proletariat. Editors will risk and suffer mob attack, religious heretics will go to the flame, rebels will slaughter their children, in the name of Liberty. The old adage is correct: men possess thoughts, but ideas possess men.

Clearly, the leader who commands causes so compelling has an extraordinary potential influence over committed followers. But followers so armed by idealism, so mobilized, so purposeful, become zealots and leaders in their own right, and hence the leader-follower relationship over values raises in sharpened form the paradox of leadership: Who are the true leaders? Who are the true followers?

Values have special potency because they embrace several significant and related phenomena: values as definitions of desirable or preferred end-states or collective goals or explicit purposes; values as standards in terms of which specific criteria may be established and choices made among alternatives. We will use the term mainly in these senses. Values are also defined as modes of conduct, such as prudence, honor, courage, civility, honesty, fairness—these are less goals in themselves than specifications of the manner in which politics and other enterprises should be conducted—and these we will call modal values. Finally, some values are both ends in themselves and means of achieving further end-states, as in the case of a young man who buys a car to achieve privacy but values the car in itself; the colonial people who embrace nationalism to achieve independence but end up more nationalistic than independent; the student who values a college education in itself but also as a means of getting a better job. These are instrumental values. Goals, criteria of end-states, principles and standards of behavior, and values that represent both means and ends—all these are a formidable arsenal for any leader who can command them. Thus if one believes in the goal of equality, measures questions of public policy by egalitarian standards, monitors his own and his adversaries' efforts by the canons of civility and honor, and favors a value like fraternity because it also leads to the end-state of equality, he can summon wide support from followers with many different values.[12]

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[10] James C. Davies, "Aggression, Violence, Revolution, and War," in *Handbook of Political Psychology,* ed. Jeanne N. Knutson (San Francisco: Jossey-Bass Publishers, 1973), p. 247.

[11] Ted Robert Gurr, cited in Knutson, *op. cit.,* p. 248.

Are values mere motives, or do they have a different and stronger arousing and directing power? Students of the subject disagree; sometimes values are treated as merely another, perhaps "higher", motive. If so, the difference in degree amounts to a difference in kind. "Values have a strong motivational component as well as cognitive, affective, and behavioral components," Rokeach says.[13] As compared to beliefs, which may be opportunistic reflections of ephemeral attitudes, values are internalized so deeply that they define personality as well as the structure of attitudes. They become an expression of both conscience and consciousness. Hence holders of values will follow the dictates of those values in the absence of incentives, sanctions, or even witnesses—like the girl who returns a lost wallet when she knows no one saw her pick it up, or the politician who defends free speech when it is guaranteed to lose him votes. A test of adherence to values is persons' willingness to apply principles or standards to themselves as well as others. Inevitably values are seated deep in cognitive, affective, and behavioral processes.

Phenomena so powerful hardly develop in a fit of absentmindedness, nor do they spring full-clothed from the brow of Jove. Yet the source of these potent and palpable forces has long been in dispute. As a "practical" matter, parents, teachers, ministers and other keepers of parochial and general liturgies, codes, and moralities have, over the millennia, sought to instill the right values, including especially modal values, in the young and seemingly pliable. These deliberate efforts have had mixed results; some recent evidence suggests that the more deliberate, obvious, and formal the efforts at teaching values, the more meager the results.[14] Perhaps the official norm-definers and standard-setters influence the "lower-level" value retailers—parents, teachers, etc.—who have some influence on followers, in the familiar two-step process. But the practical, grass-roots teachers of morals are often the first to admit the limits on their influence, as they see daily evidence of this.

How, then, account for the strong grip of certain values on large numbers of persons? In part because value setters are established throughout society, and they are all the more influential because they are informal and do not seem bent on reforming their fellow humans. Both Piaget and Kohlberg have thrown some light on this process in certain societies. Why do children move on from "lower" to "higher" stages in moral outlook? Why, if they are born into a lower stage, do they not remain locked into it, captive to cultural forces, prisoners of their "lower" wants and needs? Some of course so remain, but many do not. For the latter, the moving force is composed mainly of peers who set norms for their fellows, or of persons who are one step ahead of the others, whether in school, church, social status, or peer group. Studies have indicated that children tend to prefer levels of moral thinking just above their own stage of development, but not necessarily higher stages of morality.[15] Doubtless there is considerable slippage in the communication of values. It is unlikely that leaders who serve as "value communicators" convey the precise values adumbrated from on high, except perhaps in wars or other crises. Usually they exact a price in serving as transmitters, modifying values as they express or impart them. But this role may make the assimilation of the values more expeditious and effective, and even insidious. Different types of leaders exercise influence at different stages—parents during the earlier years, teachers later, peers and politicians at others. Fathers may instill values as authority figures more than as mealtime or bedtime preachers, mothers as suppliers and withholders of affection rather than as in-house purveyors of official doctrine. Kohlberg hypothesizes that "there are natural tendencies for the child to perceive the father as the dominant authority in almost all societies. . . ."[16]

Precisely which peers or cultural heroes—playmates, schoolmates, older siblings, parents, surrogates, teachers, scoutmasters, village editors, folk heroes—exercise how much influence over value-formation cannot be ascertained with much precision; in any event, those forms of influence would vary from culture to culture. For our purposes, several factors must be kept in mind. The total, cumulative influence of these leaders on value formation is considerable but not total. The movement of children through stages of moral development and their receptivity to influence at any stage, still turns significantly on their lived, personal experience and on their skills. Widening activities and cognitions are in themselves forces for change. But cognitive development is in turn dependent on the direct, the two-step, and the multi-step flow of influence from peers and others. Through participatory experience the evolving persons "hook in" cognitively and morally with influentials around them, but on their terms as well as the leaders'.

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[12] On definitions of values, cf. Milton Rokeach, *The Nature of Human Values* (New York: The Free Press, 1973).

[13] *Ibid.*, p. 14.

[14] Lawrence Kohlberg, "Moral Development and Identification," chap. 7 in Harold W. Stevenson, ed., *Child Psychology* (Chicago: University of Chicago Press, 1963), p. 281ff.

[15] James Rest, Elliot Turiel, and Lawrence Kohlberg, "Level of Moral Development as a Determinant of Preference and Comprehension of Moral Judgments Made by Others," 37 *Journal of Personality* (1969), pp. 225–252.

[16] Kohlberg in Stevenson, *Child Psychology*, p. 309.

The psychological processes are multiple and complex. The child must develop some sense of guilt, or at least of empathy, as he makes value judgments about "fairness" and "honesty" at higher and higher levels above primitive levels of morality. He learns how to imitate parents and others, but his imitation is selective. A sense of reciprocity grows, taking different forms at different stages. In Kohlberg's first stage of moral development, the "most primitive form of reciprocity is that based on power and punishment, i.e., the reciprocity of obedience and freedom from punishment." In the next higher stages come literal exchange and then a recognition that positive social relationships are systems of reciprocity based on gratitude, empathy, and mutual expectations. At the highest stage the child is appealing not to the palpable rules of the social order but to the abstract conception of justice that lies beyond those rules.

As children participate more widely in home, school, and play groups, confront diverse personalities, exchange confidences, take part in group decisions, they are drawn more and more into differentiated roles. Ultimately young people move into wider educational, occupational, legal, and political milieus. But not all do, or they do so on differing terms. "One index of differential opportunities for participation in the social structures of government and of work or economy," according to Kohlberg, "is that of socioeconomic status. It is abundantly clear that the lower class cannot and does not feel as much sense of power in, and responsibility for, the institutions of government and economy as does the middle class. This, in turn, tends to foster less of a disposition to view these institutions from a generalized, flexible and organized perspective based on various roles as vantage points. . . ."[17]

Qualities of leadership emerge out of these imitative, selective, and role-taking processes. As persons gain in experience, cognition, imitation, intentionality, and capacity for higher moral judgment, and as they grow more skilful in accommodation and role-taking, they gain in the capacity for leadership that both turns on an understanding of others' needs, roles, and values and expresses fundamental principles and purposes. Distinctions emerge between leader and follower, for the leader must comprehend many roles and the follower fewer, must accommodate followers' wants and needs without sacrificing basic principle (otherwise she would not be a leader), must mediate group conflict without becoming a mere referee or conciliator without purpose of her own, must be "with" her followers but also "above" them. But the leader's main strength is that she can operate close enough to the follower to draw him into the leader's level of moral development, but far enough from him not to be pulled back down into the preconventional or conventional levels of moral development.[18]

Leaders do not neatly array themselves in stages, any more than followers do. In a study of high school teachers in California, Simpson, in a preliminary exploration, found a group of men and women who tended to be moderate-to-left in political beliefs, above average in interest and political participation, generally optimistic about the resolvability of social problems, and favorable to the values of freedom, responsibility, law-and-order, and fairness. But they were prone to conventional as well as postconventional thinking, in Kohlberg's definition. They viewed the functions of law as more the prohibition of undesirable acts than "the prescription of facilitating behavior as standards and guidance, or a *beneficial* and *rational* approach to the assistance of human beings based on a principled belief in what is right or just."[19] Most felt that the law could be broken under certain conditions, to be sure, but when asked the functions of law in the abstract, they responded on the basis of prohibition and control to a much larger degree than might have been expected from their ages and social class. There were other inconsistencies in their politico-legal ideology. On balance, though, these teachers, in their collective shaping of the curriculum, their control of readings and discussions, their role as authority figures—but not, we can presume, in their formal preachings and moralizings—were helping to draw some students up through the levels of moral development.

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[17] Kohlberg in *Handbook of Socialization Theory and Research*, ed. David A. Goslin (Chicago: Rand McNally, 1969), p. 401.

[18] In order to clarify references to "leader" and "follower"—and also to counter slightly the sexual partiality of traditional usage—I refer occasionally herein to the "leader" as "she" or "her" and to the follower as "he" or "him." Those experiencing difficulty with this formulation might find it helpful to conjure up images of Joan of Arc, Elizabeth I, Catherine the Great, Eleanor Roosevelt, Golda Meier, Indira Gandhi, or Bella Abzug.

[19] Elizabeth Léonie Simpson, "Teachers of Justice: A Preliminary Report of Politico-Legal Socialization," paper presented at the meeting of the American Psychological Association, Montreal, 1973, quoted at pp. 19–20.

The Empowering of Leadership

Power over human beings is exercised when potential power wielders, motivated to achieve certain goals of their own, marshall capabilities in their power base, such as economic, institutional and skill resources, that enable them to influence the behavior of respondents by activating their motives—wants and needs, expectations and values—relevant to those capabilities. This is done in order to realize the goals of the power wielders, whether or not these are also the goals of the respondents. Power wielders also exercise influence over respondents by mobilizing their own power base in such a way as to establish direct physical control over the respondents' behavior, as in a war of conquest, or through direct psychological control, as in hypnosis, but these are relatively restricted exercises of power, dependent on certain times, cultures, and personalities. Conflict between, and among, power wielders and power respondents helps to bring to fuller consciousness underlying motives, and hence conflict both molds and enhances the existing power relationships.

Leadership over human beings is exercised when persons with certain motives and purposes mobilize, in competition or conflict with others, their own institutional, political, psychological and other resources in such a manner as to arouse, engage, and satisfy the motives of followers. This is done in order to satisfy similar needs and values held by both leaders and followers. In brief, leaders with motive and power bases tap followers' motive bases in order to realize the purposes of both leaders and followers. Leadership is exercised especially in a condition of conflict or competition in which other leaders compete in appealing to the motive-bases of potential followers.

Thus, leaders constitute a particular kind of power wielder. Like power, leadership is relational, purposeful, and collective. Leadership shares with power the central function of achieving purpose. But the reach and domain of leadership is, in the short range at least, more limited than that of power. Leaders do not obliterate followers' motives even if they wish to do so. They lead only other creatures, not things (and lead animals only to the degree that they recognize animal motives—i.e., leading cattle to shelter rather than to slaughter). To control *things*—tools, mineral resources, money, energy—is an act of power, not leadership, for things have no motives; power wielders, but not leaders, may treat people as things. And unlike the power holder, who may operate in a closed system, leaders act in a context of conflict and competition. All leaders are actual or potential power wielders, but not all power wielders are leaders. Conceptually, leadership is a subset of power.

These definitions of power and of leadership differ from a number of others. Lasswell and Kaplan hold that power must be relevant to people's valued things; I hold that it must be relevant to the power wielder's valued things and may or may not be relevant to the *respondent's* needs or values.[20] Janda defines power as "the ability to cause other persons to adjust their behavior in conformance with communicated behavior patterns"; I agree, assuming those behavior patterns effectuate the *purpose* of the power wielder.[21] According to McFarland, "If the leader causes changes that he intended, he has exercised power; if the leader causes changes that he did not intend or want, he has exercised influence, but not power. . . ."[22] I dispense with the concept of influence as unnecessary and unparsimonious. For me, the leader is a very special, a very circumscribed but potentially the most effective type of power-holder, judged by the degree of intended "real change" finally achieved. Bell et al. contend that power is a relationship rather than an entity—an entity being something that "could be smelled and touched, or stored in a keg"[23]—while I agree that power is a relationship but contend that the relationship is one in which an entity—part of the "power base"—plays an indispensable part, whether that keg is a keg of beer, of dynamite, or of ink. The crucial variable is *purpose*. Some define leadership as leaders making followers do what otherwise followers would not do, or as leaders making followers do what the leaders want them to do; I define leadership as leaders inducing followers to act for certain goals that represent the values and the motivations—the wants and needs, the aspirations and expectations—*of both leaders and followers.* And the genius of leadership lies in the manner in which leaders see and act on their own, and their followers', values and motivations.

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[20] Harold D. Lasswell and Abraham Kaplan, *Power and Society* (New Haven: Yale University Press, 1950), p. 77.

[21] Kenneth F. Janda, "Towards the Explication of the Concept of Leadership in Terms of the Concept of Power," in *Political Leadership*, ed. Glenn D. Paige (New York: The Free Press, 1972), pp. 45–68.

[22] Andrew S. McFarland, *Power and Leadership in Pluralist Systems* (Stanford: Stanford University Press, 1969), p. 174.

[23] Roderick Bell, David V. Edwards, and R. Harrison Wagner, *Political Power* (New York: The Free Press, 1969), p. 4.

Thus leadership is inseparable from followership. The essence of the leader-follower relation is the interaction of persons with different levels of motivations and of power potential, including skill. Various types of relationships between leader and follower are conceivable. Two persons having similar basic wants—hunger, or some other physiological want—could meet without either of them emerging as leader unless one had the motive and the capability of enlisting the other in a common purpose, exploiting the power bases of each, and sharing in the goals reached and the benefits received, presumably food. Or two persons with different motive bases might interact; one of them might have moved up through a hierarchy of wants and needs and other motives to the point where her main need is self-esteem (Maslow) or achievement (McClelland) or the value of justice (Kohlberg), while the other is operating at a "lower" need level, such as safety. The measure of leadership would be the extent to which, in the interaction between the two, both persons met their needs, the former by winning office or merely by feeling efficacious, and the latter by gaining a sense of national security or of personal safety. Or two persons with high achievement needs might encounter each other in pursuit of the common goal of esteem by others; they would variously share leadership and followership roles.

If leaders and followers are joined so closely in these processes, what are the differences in their functions? These may be inseparable, but they are by no means indistinguishable. First, the leader takes the initiative in making the connection between the leader and the led; it is the leader who creates the linkage that allows communication and exchange to take place. An office-seeker does this in accosting a voter on the street, but if the voter espies the politician and accosts him, the voter is assuming a leadership function, at least for that brief moment. Second, the leader is more skillful in evaluating follower's motives, anticipating his response to an initiative, and estimating her power base, than the reverse. Third, the leader continues to take the dominant part in maintaining and effectuating the relationship with followers and will have the major role in ultimately carrying out the combined purpose of leader and follower. Finally, and most importantly by far, the leader addresses herself to the followers' wants and needs and other motivations, as well as to her own, and thus serves as an independent force in changing the make-up of the followers' motive-base through gratifying their motives.

The last function warrants special attention. The potential leader and follower might come together on the basis of a common motive, such as personal safety, in which case it would be simply the former's initiative, persistence, and skill that would mark her as the leader. In some cases, the potential leader may operate at a lower motivational base, perhaps in warning a follower that his prestige needs must give way to security needs. But more typically, the leader may operate at a motivational level higher than the follower's, in part because persons acting as leader usually have satisfied their own lower need levels and are seeking to satisfy "higher" ones, such as the need for self-realization or efficacy. But whatever the relative motive levels, the test of leadership is the capacity to help gratify follower's need and, by thus extinguishing it, help the follower move to a higher need level, following the principle of unidirectionality and irreversibility. This process changes the environment in which both leader and led operate and hence produces a new configuration in the relations of leaders and followers.

The question remains: just what are the leaders doing when they seek to satisfy and extinguish the followers' "lower" needs? Are they dealing with the followers' needs as leaders define them or as followers define them? Different leaders will have different notions of the follower's true needs, values, and goals. They will seek to influence followers toward different motive levels. They will define true consciousness in sharply different ways. What, then, is the follower's true need? Only the self-conscious follower can answer this question, and he can answer it effectively only when he has been exposed to the competing claims, approaches, values, and goals of the would-be leaders. Thus conflict and competition are essential to the legitimacy of leadership, as well as its empowering. True consciousness does not result from the mobilization of motives by one group or party with overwhelming, if not exclusive, access to the minds of the followers. That can only lead to false consciousness. True consciousness on the part of both leader and follower arises only out of the free play of competing doctrines and philosophies and appeals, out of full opportunity for followers to perceive, comprehend, test, and perhaps try out the principles and promises that are offered to them. A power wielder can exert power in a situation without conflict. A *leader* can exert leadership only in conditions offering a significant degree of effective competition.

The common perception of leadership that it should inspire and elevate the public is quite correct. Revolutionary leaders may seek to appeal to the egalitarian ideals of followers at the same time that they satisfy their needs. Ministers urge their parishioners to rise above Mammon and materialism and to pursue more principled missions. Moralizing politicians seek to bring out "the best in us" in following some cause. What is the "best in us"? That remains the secret of the followers, until they wish to answer in the choices they make. Ultimately the legitimacy of political leadership does not turn mainly on elections and constitutions and representative processes, though these may be significant in measuring public attitudes and legitimating decisions. It stems from the capacity of the leader to establish an authentic relationship with the genuine needs of followers, as the leader can influence them in a free society. The empowering of leadership collapses unless it is, at least in the long run, validated by this kind of ethical certification. Hence, despite all the abuses and perversions and hypocrisy, the call for moral leadership, as it was issued by a founding father of this Association, Woodrow Wilson, and as renewed at this annual meeting by George McGovern, is still the noblest cry that can be sounded on this planet.

COMMUNICATIONS

No Clear and Present Danger

To the Editor:

I was glad to see that my book, *No Clear and Present Danger,* which was published in January 1972, was reviewed in the March 1976 issue of the *Review,* and by such a distinguished foreign policy analyst as Morton Tenzer.

But in his haste to review the book, Mr. Tenzer apparently failed really to read it. Virtually every point he makes is an error, a quotation out of context, or other distortion.

1) He says that Hitler's declaration of war removed any realistic basis for a policy of United States aloofness from the war. But that declaration almost surely would not have occurred without the Japanese attack on Pearl Harbor, and I argue, in the less controversial portion of the book (which he totally ignores), that the Pearl Harbor attack itself could have been avoided by a more appropriate policy in the Far East.

2) He attributes to me the belief, quoting me partially, that "American foreign policy after World War II was 'to oppose injustice everywhere by military means.' " While the latter phrase is mine, he quotes it out of a context (p. 107) which nowhere applies it in the manner he implies by his preceding phrase.

3) He characterizes as one of the book's "banalities" my prescription, "there must be an initial presumption against the use of force in international politics," but omits the immediately preceding phrase, "to a greater degree than has been true in American policy during recent decades." (p. 88) With that part of the sentence restored, is it still such a banality?

4) More offensive is his statement that I ignore the danger to "the survival of basic Western values threatened by transnational totalitarian mass movements." In fact, this danger is discussed on the very page (43) which he cited in the preceding sentence of his review.

5) A final example is his allegation that I was "attempting to revive the older revisionist stance of Tansill, the Beards, and other interwar critics of United States foreign policy." While I do cite Charles Beard, with mixed approval, I nowhere even mention Tansill, whose contentions differ greatly from mine. And the only evidence I can imagine for his assertion that "Russett's hero seems to be the late Senator Robert Taft" is the fact that I use a quotation from Taft as one of the book's epigrams—but the other epigram is from Norman Thomas!

In short, the review is, at best, a careless distortion of the book, a distortion perhaps reminiscent of an earlier era. The book was meant "to stimulate a reasoned questioning, leading all of us to rethink our premises and search out new evidence" (p. 107) on a controversial topic, despite "the moral outrage against Nazism that we all share" (p. 21). "It is precisely moral considerations that demand a re-examination of our World War II myths" (p. 21). Possibly some readers who manage actually to read all 108 pages of the text may find this spirit—to question, not to pontificate with assured answers—useful in their own attempts to grope toward some image of an appropriate policy for the United States in the contemporary world arena.

Perhaps even Mr. Tenzer would want to question one of his assumptions. He quotes me as saying, "In the early years of the cold war all American liberals were required to demonstrate their loyalty and freedom from any taint of pro-Communism." Is that really, as he calls it, only one of the "loose generalizations of leftist historians"? Who needs to recall a bit of history?

Bruce M. Russett

Yale University

To the Editor:

In a cultural era when we have been asked to think more kindly of poor old De Sade, have had Genêt nearly sanctified, and have seen Charles Manson become the center of a worshipful cult, perhaps it was inevitable that a political scientist would find it necessary to make an attempt at "leading all of us to rethink our premises" about America's entry into World War II. Some of us are closed minded in certain areas—rape, murder, Hitler—we lack the stimulus of Professor Russett's curiosity, which prods him to rethink and reexplore terrain that no sensible person needs to go over again once its moral and political significance is evident.

In a brief review of a short book peppered with direct quotations, it may seem to the author that the full flavor of his argument was not adequately presented, but I deny that I distorted the major thrust of the book either in my summary of its theme or in the quotations I made from it, and I note that in his letter Professor Russett does not claim that I misstated what I called the repellent central proposition of his book, that the United States should have stayed out of World War II, indeed that it was an unnecessary war, because there was "no clear and present danger" to us.

It would be pointless to trade interpretations of Professor Russett's various phrases with him. He admitted in his book that many of its readers would be offended by its theme (p. 20), and the editors of this *Review* found one whose reaction was what was expected. So be it.

I must, however, allude to Professor Russett's final point about the early years of the cold war. If he reads his sentence carefully, he will note that he says, "*all* American liberals [my emphasis] were *required* [my emphasis] to demonstrate their loyalty," etc. This is, of course, a sloppy left revisionist generalization. Professor Russett surely must recall that over a million Americans voted for Henry Wallace in 1948, and it is hard to believe that American liberals like Dwight MacDonald, Mary McCarthy, Murray Kempton, Leslie Fiedler, the editors of *Partisan Review*, et al. who vigorously opposed Wallace were *required* to do so by anything other than the logic of their beliefs and their understanding of the world political situation of the time. Or does Professor Russett mean to imply that someone *required* his liberal teachers, predecessors, and colleagues at Williams and Yale like Frederick Schuman, James MacGregor Burns, Arnold Wolfers, Karl Deutsch, Gabriel Almond, Robert Dahl, Robert Lane, et al. to come to their conclusions about American foreign policy issues as a condition of gaining or maintaining their appointments? Nonsense, of course. I still have in my files old issues of *Dissent, The Progressive, The Nation,* and *The New Republic* from the 1950's, and I would commend a review of them to Professor Russett to remind him that rather lively disputes occurred then among liberals and between them and others in our society who had different views. The current myth-making of the Left would have us believe that we had monolithic attitudes in this country toward foreign policy during the early years of the cold war, when, in fact, we did not. Recall a bit of history, Professor Russett?

MORTON J. TENZER

University of Connecticut

Public Policy, Party Platforms, and Critical Elections: A Reexamination

TO THE EDITOR:

Mr. Ginsberg's article[1] is to be commended for its serious attempt to address empirically a central issue in political theory. The issue is whether public sentiment does, in fact, become translated into concrete public policy through institutional democratic processes. This issue is central for "pluralist" theory but is rarely tested.[2] Thus, we welcome Mr. Ginsberg's attempt to address so critical an issue in an innovative, empirical manner. A series of significant problems must be noted, however, before this bit of knowledge can be added to the storehouse of political scientific fact.

Our basic criticism is that the article claims to do much more than it actually does. It *claims* to relate "popular choices to public policy."[3] In fact, it only attempts to relate party platforms and statutes to critical elections. Furthermore, the attempt to relate platforms and statutes to critical election periods is a tenuous one.

Ginsberg's indicators do not clearly represent the conceptual variables. Party platforms represent alternatives offered by party leaders and should not be construed as a surrogate for "citizen policy choices" when citizen awareness of platforms is quite low.[4] Citizen policy choices or issues are ignored in platforms while wholly new ones are offered.

Unweighted statutes are an unsatisfactory indicator of federal policy. As Ginsberg notes, statutes vary enormously in their importance and impact; however, we cannot assume one summary measure represents policy without some weighting of statutes for their importance. For example, to the extent to which a set of statutes consists of bills that are more symbolic than substantive, more narrowly aimed than broadly aimed, more ill-funded than well-funded, the relation between platform statements and statutes will be inflated. The relation is inflated because party leaders will find it easier to "fulfill" platform promises with symbolic, narrow, or ill-funded statutes which lack policy impact.

[1] Benjamin Ginsberg, "Elections and Public Policy," *American Political Science Review*, 70 (March 1976), 41-49.

[2] Robert Alford and Roger Friedland, "Public Policy and Political Participation," *Annual Review of Sociology*, 1 (1975), 429-479.

[3] Ginsberg, p. 1.

[4] Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: John Wiley, 1970).

Both platform and statute indicators are trichotomously coded (+1 support, 0 neutrality, −1 opposition) on seven issues of unclear origins. The statements which define the seven issues are highly abstract and general. Given the lack of a rationale for issue selection and the insensitivity of coding all major political statements into only three categories, the validities of the measures must be regarded skeptically, whatever their intercoder reliabilities.

Total party difference, the platform measure which is given greatest emphasis by Mr. Ginsberg, is unsatisfactory for several reasons, all of which are quite independent of the quality of the trichotomously coded data that they employ.

Party difference is defined as the product of party polarity, salience and salience difference.[5] Party polarity is defined as

$$\frac{AD-BC}{(A+B)(C+D)}$$

where A = Proportion of Positive Democratic Statements, B = Proportion of Negative Democratic Statements, C = Proportion of Positive Republican Statements, D = Proportion of Negative Republican Statements. It may range between maximal and minimal values of +2 and −2, respectively. The maximal value occurs when 100 per cent of Democratic statements support an issue and 100 per cent of Republican statements oppose it. Minimal party polarity occurs where there is 100 per cent Republican support and 100 per cent Democratic opposition on an issue. In itself this measure seems suitable. Salience is defined as (A+B+C+D). Taken by itself, this measure, which may range between 0 and 2, also seems suitable. So does salience difference, which is defined as (A+B) − (C+D) and which may range between +1 and −1.

Only when the polarity, salience, and salience difference measures are examined in unison and as multiplicative components of the party difference measure do problems appear. There are two principal difficulties. First, whatever the merits of Mr. Ginsberg's party difference measure as *some* indicator of party platforms, the measure can hardly be regarded as an adequate index of "party difference". Second, because of the algebraic interdependence of party polarity and salience difference, the meaningfulness of Mr. Ginsberg's multiplicative definition of party difference is called into question.

The first problem is simple. Party difference will equal zero if any of its components do. For example, it will equal zero if party polarity equals zero, even though salience difference equals its maximal value and salience takes on an intermediate value of 1. Alternately, it will equal zero if salience difference equals zero, even though party polarity and salience take on their maximal values. That is, party difference will equal zero "party difference" in the presence of even maximal differences on some of the selected party attributes. This problem may be interpreted as semantic, but it nevertheless remains a real problem. A measure should measure what it says it measures. Perhaps renaming "party difference" as something like "cumulative party conflict" would resolve this first definitional problem with Mr. Ginsberg's focal platform measure.

Second, Mr. Ginsberg's "party difference" measure undercuts the meaningfulness of its multiplicative formulation. One expects a multiplicative measure to increase as its components do and to have maximal (or minimal) values of its components that reinforce each other, particularly where they appear jointly. Because of the algebraic interdependence of party polarity and salience, these expectations are violated by "party difference." Maximal and minimal values of Mr. Ginsberg's party polarity, unfortunately, imply zero values of party polarity. These implications of Mr. Ginsberg's measures hardly seem to have any theoretical justification. Certainly, Mr. Ginsberg provides none for them. They imply, in turn, that whenever either party polarity or salience difference take on either maximal or minimal values, "party difference" must equal zero. It seems to us that a close examination of Mr. Ginsberg's "party difference" may serve as its *reductio ad absurdum.* Near-to-extreme values on two of the three components of "party difference" push it toward its zero midpoint, extreme values on two of its three components push it *to* zero.

A lesser problem with the party difference measure should be noted: if *either* party fails to make any non-neutral stands on an issue, party polarity and "party difference" in turn must go to zero.[6]

When historical variations in the total party difference score are presented (Table 1), we are not given an indication of their statistical or substantive meaning. When we note a decline in party difference scores since the Civil War, or a difference between a score of 0.028 in 1916 and 0.069 in 1932, what does this mean? We are told that there are "considerable" differences, but given no basis on which to judge this claim. When a *t*-test for differences between means between critical election periods and other periods is calculated, the test yields significant differences.[7] Tests such as this allow a systematic evaluation of the results presented beyond an "eyeballing" of long, detailed tables.

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[5] Benjamin Ginsberg, "Critical Elections and the Substance of Party Conflict: 1844 to 1968," *Midwest Journal of Political Science,* 16 (November 1972), 603-625, discusses each formula component.

[6] The vast number of years which show no party differences for the issues of redistribution, labor, ruralism, and universalism presented in Table 2 may be the result of one party's failing to take a stand and not due to a real lack of platform difference between the parties.

The second major set of measures presented in the paper indicates "policy" changes over time. While the scores presented in Table 3 do present an interesting picture of American political history, a review of the signs of the scores yields counterintuitive findings which are not discussed. Thus, we find the strongest pro-capitalism and anti-labor policies occurring during the Great Depression. One explanation of this finding is that it arises from weighing all statutes equally, although the results are too anomolous for this problem. Another explanation is that the trend score was computed in an idiosyncratic manner. Instead of subtracting the earlier year (t_1) from the latter year (t_2) as in the conventional equation: $(t_2 - t_1)/\text{var}(t_2 + t_1)$, the opposite was done. If computed correctly, a positive sign would mean that changes favored an issue position.

Moreover, the whole purpose of adjusting the "policy" change variable for trend is questionable. Detrending is used to remove the effects of accumulative increase or decrease, and for linear relations. Accumulative increase is not an initially predetermined attribute of annual statutes and should be so noted if it is. Likewise, the trend may be cyclical rather than linear. If so, detrending could yield unusual results. Periodization effects which may be salient are not taken into account.[8]

The number of statements or statutes on which the paper's measures are computed are not presented. This may misrepresent the scores and make across-time comparison misleading. Thus, a year with two pro-labor statutes will indicate greater pro-labor "policy" than a year with ten pro-labor statutes and one anti-labor statute.[9]

The paper's most serious flaw is its basic research design. The central hypothesis, that platform content is translated into policy content, is *never directly tested.* Platform and statute measures are only related to critical elections. They are not related to each other. While platform and statute measures may be more strongly associated during critical election periods, it is fallacious to assume they are interrelated merely because each correlates with critical elections.

The zero-order correlation between the overall "party difference" scores of Table 1 and the overall change in statutes scores of Table 4 is −0.001. The zero-order correlation between the party platform scores of Table 2 and the party difference scores of Table 3 on the one issue for which we ran correlations was −0.074. Furthermore, although Table 3 reveals considerable variation in all statutes over time, Table 2 reveals a nearly constant level of zero "party differences" on four of the seven issues, namely, redistribution, universalism, labor, and ruralism.

Interestingly, when party platforms are related to statutes for critical election periods only (as defined by Mr. Ginsberg on the opening page of his article) some interpretable relations appear. During critical election periods the correlation between overall "party difference" and overall statute change is −0.259 and the correlation between "party difference" and statute change on the capitalism issue is −0.107. (Corresponding correlations for non-critical election years are −0.022 and −0.025, respectively). If we assume, as we had earlier suspected, that the change in statute score was computed "backward," these findings can be interpreted as meaning the *cumulative* platform issue salience and party polarity and salience difference do foster statute change during critical election periods and that the Democratic party has tended to be an agent of policy change during these periods.[10] Of course, the character of the measures is likely to have attenuated the "true" bivariate relations of platforms and policies; and it obscures the exact meaning of the obtained relations.

Future work on this important issue might employ more careful data analysis and take into account other factors which may distort the relation, including the magnitude of party electoral victories, party cohesion, and party legislative strength. Also, it might be advisable to control for such factors as business cycles for "capitalism" statutes, strike activity for "labor" statutes, and war for "international cooperation" statutes.

Ginsberg is *unjustified in claiming* that ". . . the choices made by the electorate during these critical periods are translated into major changes in national policy."[11] In order to support this claim, the linkages from electorate opinions to party platforms, from party platforms to statutes, and from statutes to public policy implementation must be demonstrated. Even the limited linkage between party platform content and statute content was not adequately tested in the article.

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[7] It was found that $t = 2.01$, significant at the 0.05 level, with df = 56.

[8] We found some minor differences by dividing the entire period into "before 1900" and "after 1900." Ginsberg's earlier article (Ginsberg, "Critical Elections") even argues for some periodization effects.

[9] We owe this insight to an anonymous reviewer.

[10] The correlation between critical election periods (coded 1) with party platform changes is 0.332 and with total policy changes is 0.387.

W. LAWRENCE NEUMAN AND
ALEXANDER HICKS
University of Wisconsin, Madison

TO THE EDITOR

What I set out to do in "Elections and Public Policy" (*APSR* [March 1976]) was to examine, over time, some aspects of the relationship between the choices made by the national electorate and the content of national public policy. Specifically, I wished to see whether change in national policy was related to electoral decisions. My general hunch was that it was during "critical" periods in American political history that the alternative of changing policy was available to voters, and that insurgent victory during critical periods was the probable link between voting behavior and policy change.

The paper employed two sorts of data. First, I analyzed the content of all Democratic and Republican platforms from 1844 to 1968. I used platforms, as I stated quite explicitly, to indicate the alternatives available to voters during each electoral contest. Now, obviously as I noted, voters seldom read platforms. Yet, as I also noted in the paper, if insurgent candidates advocate policy alternatives substantially different from those defended by incumbents, electoral decisions can result in changes in policy. The consequences of electoral decisions do not depend, necessarily, on voters' awareness of the implications of their actions.

My second source of data was the *U.S. Statutes*, themselves. My coding procedure weighed each statute equally. To do this obviously ignored, distorted, and generally did violence to the qualitative differences among statutes. But I could not then and still cannot conceive of any objective way to take systematic account of the "importance" of individual statutes. Neuman and Hicks suggest that a distinction be made between "symbolic" and "substantive" laws. Even if such judgments could always be made, which I doubt, I would hardly wish to discount laws because they were "merely" symbolic. Surely, it is not necessary to point out that political symbols can have substantive importance. The pitfalls of weighing statutes on the basis of the breadth of their aim or the extent of their funding are, I think, equally obvious. While I am always happy to join the search for a better indicator, I must for the time being, at least, continue to refer readers to the argument made in the paper in defense of the coding procedure used.

The research design of "Elections and Public Policy" was, I thought, relatively straightforward. First, I sought to measure the difference between the issue positions expressed by the major parties' platforms during each campaign. I argued that the degree of difference between the parties on any issue involved at least two elements: (1) the difference in polarity between the parties' positions, and (2) the relative weight or importance assigned the issue by the two parties. I noted that the issue alternatives presented by opposing parties are conventionally said to be radically different when differences of polarity on issues are exacerbated by differences on the salience or priority of the issue.

Neuman and Hicks object to the name and formulation of my measure of party difference. I call the measure, "difference of issue position." They suggest "cumulative party conflict." Since it would be presumptuous to reject all criticism, I will concede this point. From now on, I will refer to the measure as "cumulative party conflict." More important, of course, is the question of the indicator's formulation. Neuman and Hicks point out correctly that, as defined, maximal (and thus minimal) values of salience difference imply zero polarity; and that maximal (and thus minimal) values of polarity imply zero salience difference. My first response is that the theoretical extremes are never approached by empirically realized values. Very large values on all the components of the indicator, however, can and do co-occur. But more important, the algebraic interdependence of the indicator's components is consistent with their logical interdependence. Maximum salience difference *should* imply zero polarity, i.e., if social welfare policy dominates one party's agenda but does not appear on the other's agenda at all, the two cannot clearly be said to be opposed to one another on the issue. Similarly, maximum polarity *should* imply zero salience difference, i.e., if the two parties each devote 100 per cent of their time to discussion of diametrically opposite views on foreign affairs, even though polarity is extreme, the parties' agendas can still be said to be identical. What is the theoretical justification for conceiving cumulative party conflict in the way that I have? Unfortunately, there is no body of theory to tell us how to measure the phenomenon in question. The indicator was based on my intuition about party conflict. It appeared to be useful when applied to the data. The indicator obtained values consistent with what intuition and the historical record would suggest. Cumulative party conflict reaches its highest values during critical periods and its next highest values in 1912 and 1964. In the absence, and sometimes even in the presence, of theory, the proof of the pudding remains empirical.

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[11] Ginsberg, "Elections and Public Policy," p. 48.

With regard to the measure of policy change, the data were detrended simply because of the enormous increase over time in the number of statutes enacted each year. And, indeed, as I thought I made clear, t_2 was subtracted from t_1. Simply reversing the signs, however, would indeed have averted any possible confusion.

Once cumulative party conflict and change in policy were both measured over time, the problem was, of course, to compare the two. It makes little sense to correlate platform differences and change in policy with one another directly. Any linkage between the two is through elections. At the very least, who wins might matter. One would expect policy change to be a more likely result of insurgent than of incumbent victory, for example. Correlating platform differences with policy change while controlling for the outcomes of elections would be a plausible procedure if the number of data points were sufficient to permit meaningful interpretation of the coefficients produced. With more data points one could, for example, examine the association between cumulative party conflict and change in policy for all elections won by insurgents. The two do appear to vary together. As it stands, however, I am limited to the observation that during critical periods the scope of the alternatives offered voters is greater than during any other periods and, moreover, that during those critical eras in which insurgents were victorious, the resulting policy changes were larger than usual and consistent with the alternatives offered by insurgents. A more precise way of putting this is that the decisions, in effect, made by the electorate during critical periods appear to have been translated into major changes in national policy.

BENJAMIN GINSBERG

Cornell University

More on Macropolitics

TO THE EDITOR:

Professor Waltz's appraisal (*APSR*, 70 [March, 1976]) of my *Macropolitics* confirms once again the old rule of life that unorthodox works will not please the orthodox. This is not necessarily a misfortune. It becomes one, however, when difference of outlook leads to erroneous assertions and, still worse, to unfair and false conclusions.

I regret to say that Professor Waltz's review is such a misfortune. In response, I should like to consider some specifics of Professor Waltz's criticism as well as some major theoretical issues dividing us.

First, I am rebuked for "claiming without much evidence that domestic violence is associated with poverty." I reaffirm that scarcely novel claim and repeat in capsule form some of the evidence already set out in *Macropolitics:* in the period 1958–1966, as one example of evidence used, the Defense Department tabulated 149 violent domestic upheavals around the world of which approximately two-thirds occurred in the 80 poorest countries and one-third in the 37 middle income countries (a category which in that period was based on a per capita annual income of $250 to $749). Among the 27 rich countries in the same period there was only a single instance of violent domestic upheaval.

This evidence should suffice to permit the relatively modest inference of a "marked correlation between poverty and violence" and the observation that for this "period at least, . . . the poorer the society, the more likely it is to experience violence" (*Macropolitics* pp. 84, 85). If more evidence is required, one can find it by observing the geographical distribution of both domestic and international violence around the world in the ten years since 1966—or, for that matter, in the entire postwar period. One can also find it in the considerable literature concerning the relationship between crime and socioeconomic deprivation in civil societies.

In the circumstances, I had better make explicit reference to my understanding that there is no direct or otherwise simple causal link between poverty and violence (*Macropolitics* p. 85 and *passim*) and that poverty is not the only phenomenon "associated" with violence. *Macropolitics* discusses at considerable length other phenomena related to violent behavior including competition, insecurity, greed, will to dominate, and structural weaknesses in the international system.

Having misrepresented my discussion of the violence-poverty relationship, Professor Waltz then opens up a major issue: "by assertion Sterling has abolished any 'qualitative difference between international and domestic politics (p. 330).' " I don't think I abolished anything, but I do stand by the substantive proposition that international and domestic politics have always been essentially identical.

And in the same paragraph from which Professor Waltz quotes I went beyond assertion. I sought to support the credibility of the offending proposition by pointing to the essential identity of the most important and enduring issues facing both civil and international society: wealth and poverty, liberty and oppression, war and peace (p. 330).

These are the kinds of themes that invite debate, and properly so. But no debate is offered in the Waltz review—only the accusation that I arbitrarily and without foundation asserted something out of existence. I did not assert the proposition; I argued it—at length and in many different contexts. Indeed, there is not a single chapter in *Macropolitics* that does not present evidence and analysis relevant to the identities, the similarities—and the differences—between domestic and international politics.

The abundance of such material is hardly surprising since the merging of domestic and international political theory and practice is a central concern of the book. What is surprising is that my critic did not even acknowledge the presence of evidence and argument in support of the identity proposition, much less evaluate them.

The reviewer's next major complaint is that the policies advocated in *Macropolitics* "are ordinarily to be carried out by no one in particular": "one has trouble figuring out who is supposed to follow the imperatives in order to fulfill the [policy] requirements." (Less courteously, but perhaps revealingly, he claims in this connection to detect "the whirring about of disembodied forces.")

Vertigo or not, Waltz is in trouble here. First alleging that *Macropolitics* offers no satisfactory answers to the question of what agencies should implement the recommended policies, he then reverses himself—all in the same paragraph—and declares that "the answer nevertheless slips out." That is, the text reveals the chief agencies of policy change to be (variously) nation-states, rich nations, "ultimately, . . . the superpowers." Waltz approves: "Sterling realizes that great tasks can only be accomplished by agents of great capability."

I am pleased by the compliment but puzzled by the difficulties Professor Waltz had in working through this issue. The "answer" didn't "slip out." It was up there in lights: "the globe depends for its survival on the diplomatic capabilities of its constituent states . . . above all, on the goals and capabilities of the two superpowers." Or, ". . . only superpower diplomacy is capable of providing the leadership and a sufficient share of the resources necessary to reverse the trend toward ever greater inequality among the globe's inhabitants" (p. 339).

Similar observations about the continuing paramountcy of the state are to be found in *Macropolitics* from beginning to end. Why was it so difficult for Professor Waltz to spot them and assess their significance? Possibly because *Macropolitics* pays a good deal of attention to agencies other than nation-states that have also shown some capabilities for performing international tasks and for responding rationally to the exigencies of the global system. They include global and regional organizations, international systems of trade, finance and employment, multinational corporations, transnational ideologies, and transnational development undertakings. In sum, I sought to analyze a considerable number of non-national institutions currently functioning in the global society in order to assess their present and potential contributions to the achievment of macropolitical goals. I hope Professor Waltz will not continue to label those institutions "disembodied forces" nor to contend that I do not understand the importance of agency.

Professor Waltz's review is littered with half a dozen other unwarranted accusations, but I shall let them lie. They are in any case and without exception manifestations of basic differences between author and critic in respect to normative and empirical theory. Professor Waltz would have been more accurate and more modest had he acknowledged these differences instead of charging the opposition with "lacking a theory."

I believe the real difference between us is this: Professor Waltz thinks that macropolitical goals (notably the reduction of global inequalities in power and wealth) really are not attainable. This view apparently rests on the assumption that states must be the chief implementing agencies in any global policy-making endeavors (here we agree), and that states act only in terms of microperspectives (here we disagree). I think, on the other hand, that the achievement of macropolitical goals is neither certain nor impossible. It is instead a matter of probabilities which *Macropolitics* seeks to identify and measure against the survival requirements of the global system.

Throughout history and at every level of politics and society system-wide challenges have periodically confronted actors more accustomed to thinking in terms of self than system. But great political achievement is always measured by the degree to which individuals or societies are able to reverse the customary priorities, to recognize the systemic nature of a challenge, and to mobilize system-wide resources to deal with it. *Macropolitics* argues that such political capacities continue to be in evidence in both national and transnational systems and that they are the creative presence in what for Professor Waltz is an apparently empty realm beyond the national interest.

RICHARD W. STERLING

Dartmouth College

TO THE EDITOR:

I am not led by Professor Sterling's letter to amend my critical remarks, and so I shall resist the temptation to repeat them. The aim of his book is to develop a macroapproach to international relations. How this might be done is a question of major importance. The reader who is interested in the unorthodox can quickly see what is at issue by comparing the Introduction to Professor Sterling's book with the second half of my review of it.

KENNETH N. WALTZ

University of California, Berkeley

Television News

TO THE EDITOR:

In the June, 1976 issue of the *Review*, Michael J. Robinson addresses the issue of the impact of television news on individual political attitudes. Robinson argues that despite the conventional wisdom in the social sciences, which holds that television news has no significant effects on the mass public, his analysis of new experimental data and secondary analysis of survey data support the conclusion that television does, in fact, have an effect. Specifically television helped create the current political malaise which shows up in surveys of the mass public. Professor Robinson should be commended for studying such an important subject. Moreover, it is inevitable that problems will arise in the research on such a difficult and complex topic. He has handled many of these difficulties admirably. However, because the political and public policy implications of any findings on the relationship between public affairs television and the mass public are extraordinarily significant, it is particularly important that research in this area be analyzed and reported in a fashion that does not encourage others to put more reliance on the findings than they deserve. Television news may have the effects which he suggests, but, for several reasons, I do not feel that he has yet demonstrated that they exist.

First, Robinson's analysis and speculations about the survey data overemphasize the television effect regardless of its origins. He notes that when one controls for education in Table 8 the television effect is still present, but he neglects to mention that the effects of education are much greater than those of television dependency when one reads *down* the table rather than *across*. A comparative analysis of the education and television variables in the remainder of the tables is inconclusive primarily because there are insufficient cases to make valid comparisons useful. Given that even he admits at one point that the evidence for the television effect is not "earth-shaking," Robinson's theoretical and policy-related speculations in the latter part of the article are overdone. For example, at one point (p. 431) Robinson states that ". . . one might use these data to explain the erosion of the Kennedy-Johnson coalition, a coalition which vanquished its Republican opponents in 1964 but which barely survived the campaign of 1968, let alone 1972." Although initially suggesting that he is only "supplementing" an interpretation which stresses the events of the last decade, he ends up arguing that ". . . television has been, for more than a decade, at the core of our growing political malaise" (p. 425).

Second, and more importantly, the only alternative interpretation of television dependency other than education effects that Robinson considers is based on a personality variable: political malaise may cause television dependency rather than the reverse. There is a third possibility. A cognitive psychological variable may be operating here rather than a personality variable. Persons who are low in cognitive political sophistication may be more likely to depend on television news and to view the world in simplistic terms that are easily influenced by television. The relationship between cognitive political sophistication and television dependency is not measured in this article. But something very much like cognitive political sophistication forms the basis of Robinson's theory of "inadvertent audience" which has yet to be tested. In his version of the theory Robinson associates cognitive political sophistication with low levels of education and income in arguing that ". . . inadvertent viewers tend to lack political sophistication; they cannot cope well with the type of news and information that the networks provide. But because the networks are too credible to be dismissed in their messages these viewers respond to the content by growing more cynical, more frustrated, more despairing. . ." (p. 426). A number of problems arise from Robinson's theory of the inadvertent viewer.

(1) Regardless of whether or not cognitive sophistication interacts with other demographic variables, there is no reason to assume if the cognitive theory is found to be accurate that one has truly found a television effect. The

cognitively unsophisticated may react differently to news even when it is received through interpersonal channels rather than media channels. The SRC items on which Robinson bases his conclusions only asked people which media source they relied on. They did not deal with those who rely primarily on interpersonal communication for their political news. Future research will have to study nonmedia users as well as cognitive variables before we can say whether the television effect is real or imagined.

(2) Even if one discovered that television dependency has an interactive effect with a cognitive variable, this effect would not necessarily be unique to television. In the first quarter of this century the yellow journalism of William Randolph Hearst and others functioned in much the same way that Robinson suggests the television networks now function, stressing muckraking, conflict and negative reporting. Further research can test some of these speculations by focusing on those persons receiving their political information primarily from tabloid style newspapers. Although Robinson acknowledges (p. 430) that "... in some respects, all media are qualitatively similar," he argues that "... television reaches millions who would ordinarily go without the information." He provides no evidence for this statement. An historical analysis of the development of the American newspaper industry and social structure might reveal that in the past news was disseminated rapidly and widely.

Robinson further argues (p. 430) without any documentation that the inadvertent audience historically was "a repository of passivity" because it received its political information from a "local precinct worker or friend or relative" while now it is a source of dysphoria and instability because it receives its information directly from television. Again, a careful reading of American history will probably demonstrate that there have been numerous periods when "destabilizing" attitudes in large segments of the mass public existed, often on issues analogous to what is now called the Social Issue.

(3) If the television effect is independent of education and economic status, why does Robinson's speculative explanation of the television effect rely in part on these latter two variables? One cannot have it both ways. Robinson suggests a two-step model (as yet untested) to account for the spread of malaise to those who are not part of this inadvertent audience, but this does not help in explaining the associations that are suggested in this statement: "Because the inadvertent viewers within the general audience are of lower socioeconomic status and educational attainment and because they come to the information without the benefit of mediation through opinion leaders, they are more vulnerable to almost any type of television effect" (p. 426). Moreover, as previously noted, such a group is more vulnerable to the effects of any number of influences. The primacy of television has yet to be demonstrated.

In the final section of his article Robinson cites studies of news outputs in arguing that the television news programs tend to be negativistic, conflict-oriented, and anti-institutional. He makes it clear that he is not impressed with the argument that these characteristics are primarily the result of period effects stemming from the traumatic events of the last dozen years. Regardless of what causes news outputs, Robinson presents no evidence for his assertion that "... the theory which places events, per se, at the center of the changing American ethos seems to be an insufficient explanation of the 1960s and even the 1970s" (p. 425).

These events included the assassination of political leaders from all parts of the political spectrum, including a president, the most destructive urban riots in a century, the Vietnam War, Watergate, the worst recession since World War II occurring at the same time as the worst inflation since the late 1940s, the integration of different class and racial groups in the schools of most American communities, the politicization of many new occupational and social groups, a tremendous growth in the numbers of those receiving welfare benefits in our largest urban settings and great proliferation in the number of federal agencies administering bureaucratic rules governing domestic programs. These are just a few of the events that took place primarily during the period from 1963 to 1975. After examining this list what is remarkable is that *more* political malaise has not occurred. Events may not be everything but they are certainly "central" to any explanation of changing attitudes towards political institutions. Moreover, there have been other such periods of disillusionment with political institutions in our history prior to the development of television news: the post–World War I reactions against foreign involvements are one example.

It should also be kept in mind that television networks are economic institutions. Television news producers are sensitive to trends in the interests of the mass public. On occasion television news may help create "opinion markets" in the mass public. But at other times, television news may merely be responding to "opinion markets" already existing just as other corporations respond to markets for their products. Students of television news should give more attention to the possibility that news organizations are influenced more by the public than the public is influenced by the news establishments.

PAUL HALPERN

University of California, Los Angeles

TO THE EDITOR:

At times, certainly not always, criticism serves to sharpen the thinking of those criticized. In this case, Professor Paul Halpern has, indeed, made me think through, once again, several aspects of my recent *Review* article about television and political malaise. Nevertheless, after considering his comments, I'll stand pat with my theory.

Let me begin by noting that Halpern continually alludes to the findings from the secondary analysis I did with the SRC election studies, 1952–1968. He then goes on to criticize the inconclusiveness of those findings which show some relationship—albeit a weak one—between television dependency and political estrangement. But the bulk of the energy expended in my research was given over to experimentation and to my experimental findings. Halpern makes only one mention, in his introductory paragraph, of the "new experimental data." I—and I imagine my dissertation chairman as well—must regard this as an unfortunate oversight on Halpern's part. In fact, a good portion of the argument used against my theory by Halpern could be countered by looking at the laboratory research which demonstrates directly that political messages from television can diminish the viewer's own perception of his or her political competence or self-esteem. The experimental findings are the best justification for arguing that the survey findings are not spurious. To ignore the laboratory work is to cut away much of the muscle from my collection of data. It seems a little unfair to ignore half my data and then criticize the remainder as incomplete.

In a direct communication between Professor Halpern and me, Halpern acknowledged that he had not paid great attention to my experimental findings, in part, because he doubted the generalizability of the findings from the laboratory to the outside world, where viewing conditions would be quite different. This is a topic which needs some consideration, surely. But this is not the appropriate time for raising those issues involving the utility of experimental political science. Still one must not forget that there were experimental data in this research that tended to reinforce the findings of the survey research.

Halpern also criticizes my article by pointing out that two other variables—educational attainment or "cognitive political sophistication"—may be a better explanation for the apparent relationship between television and inefficacy, television and mistrust, etc. I have done what I could do to consider the effects of education—an effect which is, as he notes, considerable—and the relationships *did* survive the tests. As for cognitive variables, the case remains open.

I have also accepted the notion that there may well be an interaction among the content of television journalism, the style of television journalism, and the audience for television journalism that causes malaise. But, unlike Halpern, I do regard this as a TV effect because were there no television news, as it exists now, there would be less malaise than exists now. In my opinion television news is, at the very least, a necessary condition for the rapid growth of political malaise.

Halpern also doubts the existence of an "inadvertent audience"—an audience which "falls into" network or local television news and which, I contend, would remain "newsless" without television. It is virtually essential to my argument that an inadvertent audience exists, and I am convinced that it does. The most recent nationwide research into television news audiences, conducted by a political scientist, Richard Hofstetter, demonstrates that the audience for television news is, unlike all other news audiences for other media, an undifferentiated one. That television news viewers are, for the most part, a random collection of people, reflecting in most every respect the population in general. Hofstetter also finds that those watching the news, in overwhelming proportions, watch for nonpolitical reasons, suggesting clearly that inadvertent is the proper term for this audience.

Professor Halpern also implies that I did not consider in my analysis those people who rely on interpersonal communication for monitoring politics. I did. They were coded as not TV-dependent.

Despite my problems with Halpern's problems to this point, I do share his concern in four areas. First, I accept his point that I have not demonstrated beyond all reasonable doubt a TV effect, per se; I say so in the article. Given the events which transpired between 1963 and 1974, it is virtually impossible to say that events are of no significance. But it seems equally implausible to argue that the medium made no difference either. It is the other side, Halpern's side, which argues, without data, that events are *the* factor. Although a good chunk of the profession may be willing to accept the events theory, without proof, I am unwilling still to do so.

Second, Halpern raises the point that my data are relevant only to the unique period of the 'sixties. I tried to deal with that thesis in the last pages of my article and I am eager to learn if the medium was the message only until Watergate made the message so strong as to overwhelm the effects of medium.

Third, Halpern caught me in an apparent small breach of logic. If education cannot explain away the relationship between TV and malaise, education cannot be used "to correct" the problem. It is also true, however, that the stronger relationships between television and political malaise appear to be found among those of moderate social and economic standing. If there is some interaction between television and social status, then perhaps there may be a brighter day ahead, as those who presently fall within that middle level category move beyond. However, if Halpern prefers to hold me to one side of the dilemma, as he poses it, I choose the first argument as I stated it. That is, education does not explain away the relationship between television and malaise and, therefore, it cannot be invoked as a cure. The gap in my logic derived, I suppose, from my general optimism about the political future. Perhaps I need to watch more television news myself.

Fourth, and finally, Halpern says something in his opening paragraph which has forced me to reconsider much of my work beyond that which appears in the article at question. He writes that "because the political and public policy implications of any findings on the relationship between public affairs television and the mass public are extraordinarily significant, it is particularly important that research in this area be analyzed and reported in a fashion that does not encourage others to put more reliance on the findings than they deserve." I could not agree more with this admonition.

I must also say that my article has been an attempted test of an important theory—albeit a weak test for a strong theory. My article is not a call for action. I do not offer these findings to advocate changing public policy toward the networks or local news organizations. I admit that in part I am attempting to communicate with television news people because television news people do not consider that their personal politics and political impact seem to run at odds with one another—although I am not altogether certain that TV people are unaware of what they do. My data on effects, as well as my data on television newspersons, are grey, not black and white.

Of one thing I am certain; nobody has been asked to redo telecommunications policy on the basis of my research. I only ask that my article force political scientists to reconsider the time-worn theories about the impotence of television in affecting American politics. Halpern's comment implies that my article has been doing what I had hoped it would.

MICHAEL J. ROBINSON

The Catholic University of America

EDITORIAL COMMENT

The Editorial Interns: An Interim Report. The *APSR* Editorial Interns in Berkeley have now completed their work. A similar group has been formed in Pittsburgh to carry on the tasks of checking footnotes and quotations, and improving the scholarly accuracy and, we hope, the usefulness of the *Review* for the benefit of its readers. The names of the current roster of Interns appear on our masthead. Alumni of the Interns have scattered to the four winds. Many of them have written to say that their experience in the program was a good one for them. All of them have contributed to the success of the *Review*. Those of us who have worked with them through the last six years believe that political scientists everywhere are in the debt of these devoted young colleagues who, while pursuing their own graduate studies, also gave their time and energy, uncompensated either monetarily or with academic credit to help us put out the *APSR*. We propose, therefore, to mention the current affiliation or location of former interns so that those of you who are nearby one or more of them, and so inclined, can add a word of thanks to our own.

In the San Francisco Bay Area: William Cavala (University of California, Berkeley), Beverly Kearns (Oakland), Harry Kreisler (University of California, Berkeley), Bonnie McKellar (Oakland), Brinton Rowdybush (Berkeley), John Ruggie (University of California, Berkeley), Christine M. Sierra (Palo Alto), Robert Stumpf (San Francisco), Arthur Trueger (Berkeley). In Washington, D.C.: Richard G. Hutcheson III (White House), Elaine Kamarck, James Lengle (Georgetown), Matthew Pinkus (House Majority Whip Office), Tom Reese (Taxation With Representation), Byron Shafer (American Enterprise Institute), Stephen Turrett (U.S. Defense Department). In Toronto: Colin Campbell (York University), Richard G. C. Johnston (University of Toronto). At the University of California, San Diego: Peter Cowhey, David Laitin; at the University of California, Davis: William M. Lunch; at the University of California, Los Angeles: M. Stephen Weatherford. At the University of Minnesota: Sam Kernell; at Washington University, St. Louis: Jesse McCorry; at Southern Methodist University: Jay Starling; at Denison University: Dorothy Clayton and Gerald Clayton; at Indiana University: Dan Metlay; at Tennessee State University: Harry Williams; at the University of Michigan: Serge Taylor; at Ohio State University: Richard Gunther; at Pennsylvania State University: Robert Lyle Butterworth. In Amherst, Massachusetts: Joseph Martin; at MIT: Jeffrey Pressman; at Dartmouth College: Ian Lustick and Robert Nakamura; at Princeton University: Jeffrey Hart. At Hebrew University of Jerusalem: Alex Radian.

Articles Accepted for Future Publication

Joel D. Aberbach, University of Michigan, "Power Consciousness: A Comparative Analysis"

Paul Allen Beck, University of Pittsburgh, "Partisan Dealignment in the Post-War South"

Robert A. Bernstein, Texas A & M University, "Divisive Primaries Do Hurt: U.S. Senate Races, 1956–1972"

Paul R. Brass, University of Washington, "Party Systems and Government Stability in the Indian States"

Steven R. Brown, Kent State University, "Toward an Experimental Analysis of Political Literature"

Paul Burstein, Yale University, "Social Cleavages and Party Choice in Israel: 'A Log-linear Analysis' "

Melissa A. Butler, The Johns Hopkins University, "Early Liberal Roots of Feminism: John Locke and the Attack on Patriarchy"

George W. Carey, Georgetown University, "Separation of Powers and the Madisonian Model: A Reply to the Critics"

Clarke E. Cochran, Texas Tech University, "Authority and Community: The Contributions of Carl Friedrich, Yves Simon, and Michael Polanyi"

Neal E. Cutler, University of Southern California, "Demographic, Social Psychological, and Political Factors in the Politics of Age: A Call for Research in Political Gerontology"

Barbara Deckard, University of California, Riverside, "Party Realignment and the Transformation of the Political Agenda: The House of Representatives, 1925–1938"

Shaheen F. Dil, Princeton University, "The Cabal in Kabul: Great Power Interaction in Afghanistan"

Lloyd S. Etheredge, Massachusetts Institute of Technology, "Personality Effects on American Foreign Policy, 1898–1968: A Test of Interpersonal Generalization Theory"

Robert Eyestone, University of Minnesota, "Confusion, Diffusion and Innovation"

Richard F. Fenno, Jr., University of Rochester, "Congressmen in Their Constituencies: An Exploration"

John A. Ferejohn and Roger G. Noll, California Institute of Technology, "Uncertainty and the Formal Theory of Political Campaigns"

Morris P. Fiorina and Charles R. Plott, California Institute of Technology, "Committee Decisions Under Majority Rule: An Experimental Study"

Norman Frohlich, Joe A. Oppenheimer, Jeffrey Smith and Oran R. Young, University of Texas at Austin, "A Test of Downsian Voter Rationality: 1964 Presidential Voting"

Dante Germino, University of Virginia, "Eric Voegelin's Framework for Political Evaluation in His Recently Published Work"

Robert T. Golembiewski, University of Georgia, "A Critique of 'Democratic Administration' and Its Supporting Ideation"

George D. Greenberg, Jeffrey A. Miller, Lawrence B. Mohr and Bruce C. Vladeck, University of Michigan, "Developing Public Policy Theory: Perspectives from Empirical Research"

John G. Gunnell, State University of New York at Albany, "The Myth of the Tradition"

Douglas A. Hibbs, Jr., Massachusetts Institute of Technology, "Political Parties and Macroeconomic Policy"

Barbara Kellerman, Tufts University, "Mentoring in Political Life: The Case of Willy Brandt"

Samuel Kernell, University of Minnesota, "Explaining Presidential Popularity"

James H. Kuklinski, Wichita State University, "Representativeness and Elections: A Policy Analysis"

Arthur J. Lerman, Yeshiva University, "National Elite and Local Politician in Taiwan"

Michael S. Lewis-Beck, University of Iowa, "The Relative Importance of Socioeconomic and Political Variables for Public Policy"

J. Miller McPherson, Susan Welch, University of Nebraska, and Cal Clark, New Mexico State University, "The Stability and Reliability of Political Efficacy: Using Path Analysis to Test Alternative Models"

Abraham H. Miller, University of Cincinnati, Louis H. Bolce, Fordham University at Lincoln Center, and Mark Halligan, Northwestern University Law School, "The J-Curve Theory and the Black Urban Riots: An Empirical Test of Progressive Relative Deprivation Theory Using Both Objective and Perceptual Indicators"

Edward N. Muller, State University of New York at Stony Brook, "Behavioral Correlates of Political Support"

Edward N. Muller and Thomas O. Jukam, State University of New York at Stony Brook, "On the Meaning of Political Support"

Garrison Nelson, University of Vermont, "Partisan Patterns of House Leadership Change, 1789–1976"

Thomas C. Nowak, Waynesburg College, "Politics and Administration: The Philippines Before Martial Law"

John M. Orbell, University of Oregon, and L. A. Wilson II, University of Nevada, "Institutional Solutions to the N-Prisoners' Dilemma"

A. Kenneth Organski, University of Michigan, and Jacek Kugler, Boston University, "The Costs of Major Wars: The Phoenix Factor"

David E. Price, Duke University, "Policy Making in Congressional Committees: The Impact of 'Environmental' Factors"

Steven J. Rosen, Institute of Advanced Studies (Canberra, Australia), "A Stable System of Mutual Deterrence in the Arab-Israel Conflict"

Howard Rosenthal, Carnegie-Mellon University, and Subatra K. Sen, University of Rochester, "Spatial Voting Models for the French Fifth Republic"

Barry S. Rundquist, University of Illinois, Urbana, Gerald S. Strom, University of Illinois at Chicago Circle, and John G. Peters, University of Nebraska, "Corrupt Politicians and Their Electoral Support: Some Theoretical and Experimental Observations"

Lester M. Salamon, Duke University, and John J. Siegfried, Vanderbilt University, "Economic Power and Political Influence: The Impact of Industry Structure on Public Policy"

Lars Schoultz, Miami University, "The Socio-Economic Determinants of Popular-Authoritarian Electoral Behavior: The Case of Peronism"

Thomas Schwartz, Carnegie-Mellon University, "Collective Choice, Separation of Issues and Vote Trading"

Donald D. Searing, University of North Carolina, "Measuring Politicians' Values: Administration and Assessment of a Ranking Technique in the British House of Commons"

Goldie Shabad, University of Chicago, and Sidney Verba, Harvard University, "Workers' Councils and Political Stratification: The Yugoslav Experience"

Gerald S. Strom, University of Illinois at Chicago Circle, and Barry S. Rundquist, University of Illinois, Urbana, "A Revised Theory of Winning in House-Senate Conferences"

Donley T. Studlar, Centre College of Kentucky, "Policy Voting in Britain: The Colored Immigration Issue in the 1964, 1966, and 1970 General Elections"

Gerald C. Wright, Jr., Florida Atlantic University, "Contextual Models of Electoral Behavior: The Southern Wallace Vote"

Marvin Zetterbaum, University of California, Davis, "Equality and Human Need"

Money, Politics, and Democracy: A Review Essay*

DAVID ADAMANY
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The 1976 presidential campaign is history. Nomination hopefuls began their campaigns under the Federal Election Campaign Act of 1974, signed into law on October 15, 1974, by President Ford.[1] On January 30, 1976, the Supreme Court struck down provisions of that law limiting campaign expenditures, curbing candidates' contributions to their own campaigns, and granting wide powers to an independent Federal Election Commission whose members included congressional as well as presidential appointees.[2] At the same time, limits on contributions by groups and individuals and sweeping requirements for disclosure of campaign finances were sustained.

The justices also stamped their constitutional imprimatur on the sharpest departure from traditional campaign funding practices in the Republic's history—the public financing of presidential campaigns. Presidential nomination contests are now funded by public matching grants for private contributions. General election campaigns are almost wholly supported from direct public grants. And national party conventions receive public subsidies.[3] In brief, the 1976 presidential campaign was mounted on a foundation of public money.

The earth tremors of a coming divide in political finance should have been apparent to political observers at least a decade ago, when the Congress first enacted, then later suspended, the income tax check-off to fund presidential campaigns. But on the whole, political science ignored the early warnings and so was caught unprepared for events which have transformed the face of American politics. Scholars have done little to shape these developments and only slightly more to explain them. Tumultuous national events involving political money have brought forward some scholars to take political finance, an orphaned area of inquiry, back into the academic family. So far these efforts have been isolated, usually anecdotal, and almost universally nontheoretical.

Sources, Uses, and Sums

In 1960, Alexander Heard aptly remarked that "if the demographer had to take his own

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[1] The Federal Election Campaign Act Amendments of 1974, Public Law 93-433, 93d Congress, 2d Sess., 1974.

[2] *Buckley v. Valeo,* –U.S.–, 96 S. Ct. 612 (1976). A more complete description of the decision is found in the appropriate sections of this essay.

[3] Full public subsidies are available to candidates of major parties—those receiving 25 per cent or more of the vote in the last presidential election. Proportional subsidies are available to candidates of minor parties—those receiving between five per cent and 25 per cent of the vote in the last election. And postelection subsidies are available to candidates of parties which qualify as either major or minor parties because of the votes received in the current election.

census and the election analyst had to tally the ballots, they would be in the same predicament as the student of campaign finance."[4] The herculean data-gathering activities of the Citizens' Research Foundation, the FCC's tallies of political expenditures for radio and television advertising,[5] the sweeping disclosure requirements of the 1971 and 1974 Campaign Acts and of comparable state laws, and the Common Cause campaign finance monitoring project have recently provided reliable paving stones for the path to serious analysis.

Herbert E. Alexander's *Financing the 1968 Election* may be the last of the major political finance studies in which the analyst must also serve as census taker. Written without the benefit of recent disclosure laws, Alexander's description of the 1968 presidential campaign is built on painstaking investigation of the hopelessly inadequate reports filed under the old Corrupt Practices Act and similar state laws, but especially on the author's relentless collection of information from his carefully cultivated network of extralegal sources.

By contrast, Alexander's *Financing the 1972 Election* rests primarily on information disclosed under the 1971 and 1974 Campaign Acts. Alexander has supplemented this rich data vein with judicially ordered disclosures in lawsuits brought by Common Cause and Ralph Nader's Public Citizen, with the revelations of a watchful press, with thousands of pages of testimony and exhibits before various congressional committees, and with Common Cause's sweeping report on congressional campaign finance. This latest of Alexander's quadrennial studies is, therefore, also his most complete.[6]

Alexander reports that total nomination and election expenditures for all local, state, and national offices reached $300 million in 1968 and $425 million in 1972—up sharply from $140 million in 1952, $155 million in 1956, $175 million in 1960, and $200 million in 1964. The 1972 Nixon campaign's $63 million treasury greatly exceeded the $45 million spent by Republicans in 1968, including high outlays in Nelson Rockefeller's nomination campaign that year. And George McGovern's $30 million general election expenditure easily outdistanced all previous Democratic presidential campaign budgets. Total Democratic presidential campaign costs, for nomination and election, were $63 million in 1972 ($33 million in the pre-convention race) contrasted to $37 million in 1968 ($25 million before the convention).

Also found in Alexander's reporting are the highest interparty spending disparities in modern history. National level Republican committees outdistanced their Democratic and labor rivals by a margin of 67 per cent to 33 per cent in 1972. Not only was Hubert Humphrey badly outspent in 1968, but his funds came too late in the campaign to be effectively used. Alexander does not say so, but the reader comes away with the impression that 1968 was an election that money did decide. In 1972, however, no amount of additional spending could have saved George McGovern's ill-fated candidacy.

Alexander's studies also point up the perplexing countertrends in current American contribution patterns. We find some major presidential campaigns financed primarily by a tidal wave of small gifts. The 1968 Wallace campaign and, to a lesser extent, the McCarthy canvass drew money from large numbers of small contributors. (Fully 86 per cent of the Wallace dollars were in gifts under $100). In 1972, George McGovern waged the first modern Democratic presidential campaign funded mainly from small contributions—about two-thirds of his funds represented gifts under $100. Barry Goldwater's 1964 campaign holds the distinction of being the only modern Republican canvass financed with small sums.

The Goldwater, Wallace, McCarthy, and McGovern campaigns were all ideological, appealing on highly salient and emotional issues to a minority segment of "true believers" in the nation. Yet despite the success of these ideological fund appeals, there was no appreciable increase in contributing by Americans; 8 per cent gave to a political campaign in 1968 and 10 per cent in 1972, while 11 per cent had given in both 1960 and 1964. And, of course, the ideological candidates who so richly succeeded in winning vast numbers of small contributions failed signally to rally the electorate.

In sharp contrast to the broad-based financing of ideological campaigns was the growing role of big givers in 1968 and 1972. From 1952 through 1964 the number of individual contributors giving more than $10,000 hovered between 95 and 130; they gave between $1.5 million and $2.3 million. But in 1968 there were 424 of these biggest givers, and their contributions totaled more than $12 million. In 1972 about 1750 Americans contributed or lent sums of $10,000 to political campaigns; their total gifts reached $73 million (17 per cent of all political spending). The systematic Nixon money canvass in 1972 represented an historic departure from past financing practices: 153 donors gave $50,000 or more, for a total of $19.8 million (about one-third of the total Nixon treasury).

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[4] Alexander Heard, *The Costs of Democracy* (Chapel Hill: University of North Carolina Press, 1960), p. vii.

[5] These compilations were published by the Federal Communications Commission under the title *Survey of Political Broadcasting* for each election year from 1960 through 1970. The 1972 data were published as a congressional document. U.S. Congress, Senate, Sub-Committee on Communications, Committee on Commerce, *Hearings on S. 372, Appendix A*, 93d Congress, 1st Sess., 1973.

[6] Alexander's earlier presidential studies are *Financing the 1960 Election* (Princeton: Citizens' Research Foundation, 1972); *Financing the 1964 Election* (Princeton: Citizens' Research Foundation, 1966); and *Financing the 1968 Election* (Princeton: Citizens' Research Foundation, 1970).

Interest-group giving reached unprecedented levels in both 1968 and 1972. Members of the Business Council, who had contributed $272,500 in 1956, gave $363,900 (77 per cent to Republicans) in 1968 and $1,180,400 (86 per cent to Republicans) in 1972. Officers and directors of *Fortune*'s 500 top industrial firms and of contractors with the Defense Department, the Atomic Energy Commission, and the Aeronautics and Space Administration increased their political gifts from $1,479,000 in 1968 to $3,065,900 in 1972, with Republicans receiving 85 per cent of the funds in both years.

Alexander's 1968 study makes no judgments about the amounts or sources of money. But his 1972 volume includes three chapters which methodically trace the irregular, immoral, and illegal financing practices of the Nixon campaign. Alexander's narrative powerfully reminds us, without editorial comment, about the low estate to which American politics had fallen in 1972 and about the crucial role that political finance practices played in that fall. At the same time, neither of Alexander's books ventures any theoretical framework for understanding the relationship of campaign money to other political, social, or economic practices in America nor for assessing the role of political money in a republican polity. Despite their theoretical poverty, both the 1968 and the 1972 volumes will stand as the basic reference works on presidential campaign financing in their respective years.

The Secretary of the Senate and the Clerk of the House prepared summaries of contributions and expenditures in 1972 congressional campaigns, but this practice was discontinued by law when the Federal Election Commission was established by the 1974 Campaign Act. This lapse of governmental compilations will not, however, impair our understanding of congressional financing, for the authoritative studies of 1972 and 1974 legislative races are the volumes compiled by the Common Cause monitoring project.[7] For 1972, Common Cause published a three-volume set on interest group and party financing as well as a ten-volume set on Senate and House campaigns. For 1974, similar reports are contained in a single five-volume set. These systematic surveys of organization and candidate finance are a gold mine of information for concerned citizens, political practitioners, and scholars.

Common Cause shows that in 1972, nationally registered interest committees contributed $8.5 million to congressional campaigns and in 1974, $12.5 million. Party committees gave $5 million in 1972; but $1.3 million of party funds came from interest groups, presumably for laundering. In 1974, party committees played a smaller role, contributing only $3 million. Business, agriculture, and health groups contributed $3.5 in 1972 and $4.8 million in 1974. Labor gave $3.6 million in 1972 and $6.3 million in 1974. Interest committees favored incumbents over challengers in both years; in 1972, incumbents received $4.4 million to $2.1 million for challengers, while in 1974 the ratio was $6.7 million to $2.5 million.

The volumes on Senate and House campaigns are similarly detailed. Total congressional campaign spending is estimated at $77 million in 1972 and $88 million in 1974. (The 1972 figure is understated because the Campaign Act of 1971 did not go into effect until April 7, 1972.) The financing of each campaign is carefully documented. We are told, for example, that in 1972 Henry S. Reuss was the Democratic incumbent of 18 years in Wisconsin's fifth congressional district; that he was unopposed in the primary and opposed in the general election, which he won with 77.3 per cent of the vote; that his challenger was Republican Frederick Van Hecke; that Reuss received $14,829 and spent $12,613; that Reuss began the campaign with cash on hand of $8,451 and disclosed additional contributions of $6,378. We also learn not only that Reuss received $700 in individual contributions, but also the identity and state address of each contributor and the amount of his gift. Further, Common Cause reports that Reuss received $4,450 in committee gifts ($1,650 from party committees and $2,800 from other groups), and they detail the identity and amount of each committee gift. In 1974, we are apprised of the individual gifts of $500 or more rather than those exceeding $100, but we are helpfully told the interest affiliation of each such individual giver.

The Common Cause studies confirm long-suspected patterns of congressional campaign financing. As in presidential politics, large contributions are important. Almost $43 million (68 per cent) of the $62.3 million raised by general election candidates after April 7, 1972, was in sums exceeding $100. Individuals accounted for $21.7 million of the over-$100 gifts, with more than half that amount in contributions of $500 or more; and interest groups, party committees, and unrepaid lenders contributed the rest. In 1974, more than 25 per cent of the $88.2 million spent in congressional races was raised from individual contributors in sums of $500 or more.

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[7] *1972 Congressional Campaign Finances; 1972 Federal Campaign Finances: Interest Groups and Political Parties; 1974 Congressional Campaign Finances.*

"Incumbency Means Everything," according to the Common Cause tally. In 1972 Senate and House races, incumbents averaged a two-to-one dollar edge over challengers. In 1974, Democratic incumbents maintained that lead; but Republican incumbents, suffering the aftershocks of Watergate, outspent their Democratic challengers by only three-to-two. As expected, campaign spending spirals as competition heats up. In 1972 House races decided by more than 70 per cent of the vote, winners averaged outlays of $38,700 and losers $7,500; but when races were won with less than 55 per cent, winners spent $107,000 and losers $101,200.

Like other analysts, Common Cause is plagued with data problems, despite the sweeping disclosure provisions of federal law. Receipts and expenditures are reported for the whole campaign, because it is impossible to untangle which pre-primary expenditures and receipts (separately reported to the federal government) had general election impacts. In addition, however, Common Cause creates its own data gaps. The 1974 report summaries are plainly less complete than the 1972 summaries. Missing are such bits of information as total receipts in all congressional races, aggregate party committee contributions, and the incumbent/challenger expenditures in districts won by varying margins. These data are reported district-by-district, but not for the Congress as a whole.

The careful observer wonders whether these exclusions do not reflect a decision to publish less comprehensive information which somehow better serves the lobbying and litigation strategies of the organization. Common Cause has a point of view, of course. There is too much big money and too much special interest money, it believes. And incumbents have such long financial leads that effective competition, and thus official accountability, is thwarted. Spending and contribution limits, disclosure of campaign money, and public financing of campaigns are its preferred policy responses. And, "it should be noted that Common Cause won its dominant [reform] position by first mastering information generation and utilization. Essentially, it used information generation as a resource to create for itself an influential policy-making position, firmly 'astride the communication process.' "[8] Despite the gaps in the 1974 summary reports, which may be filled if Common Cause eventually makes its data base available to other researchers, the campaign finance monitoring project will be justly applauded as the most thorough review of congressional financing ever conducted in the United States.

Two other recent studies can be treated lightly. Former Congressman Max McCarthy's *Elections for Sale* is well written and engaging. But it is wholly anecdotal and without perspective. He mentions familiar examples of high spending, big contributions, special interest financing, highly paid media consultants, covert funding arrangements, and weak laws.

McCarthy also reviews campaign financing practices in foreign nations, but typically ignores how differences in structure (parliamentary versus separation of powers arrangements), in party organizations (mass membership or disciplined parties versus coalition parties), and in election rules (proportional representation versus single-member districts with plurality decisions) affect financing. He describes the low constituency spending limits in Great Britain, but he ignores massive outlays during elections by the central offices and between elections by parties and economic institutions. McCarthy finds encouragement in Florida's election disclosure and spending limit laws and in Lawton Chiles' successful low-budget walking campaign. There is something funny about a book which rails against high-spending media campaigns but finds encouragement in a low-budget, but equally frivolous, gimmick.

George Thayer's posthumously published *Who Shakes the Money Tree?* is a longer and more sophisticated book of the same genre. An opening chapter reports on "Big Money At Work," with special focus on the financial advantages of the Kennedy family (whom Thayer plainly dislikes). Four chapters hastily rewrite the well-known history of campaign funding in "The Early Years" (1789-1876), "The Golden Age of Boodle" (1880-1930), "The Age of Media" (1932-1960), and the period of "Big Money in Control" (1964-1972). Thayer then turns to contemporary contributing, easily identifying three significant patterns—money from the personally wealthy ("Fat Cats and Serious Money"), from the influence brokers who raise money ("Key Men with Access"), and from organized special interests ("Lobby Money"). He probes campaign expenditures and why costs are rising ("The Money Trough").

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[8] Joel Fleishman and Carol Greenwald, "Public Interest Litigation and Political Finance Reform," *The Annals* 425 (May, 1976), 116.

Thayer's most important point, though not well or thoroughly executed, is the significance of key fund raisers. He notes that some have national standing, while others limit their fund-raising to states or regions. Most are not only personally wealthy but also prominent in non-political fund-raising—ranging from investment banking to community charities. Their clout with contributors derives only partly from politics; social standing and business positions may be equally important. To be enduringly successful, fund raisers must maintain current "tickle lists" of those able and willing to give, change solicitation techniques to follow current fashions ("entertainments" rather than dinners), and produce access to politicians or results from government just often enough to persuade others that they may be able to do so again if necessary. Although the central role of money brokers has been discussed elsewhere,[9] it is less well understood than most aspects of political financing and Thayer's treatment is helpful.

Money In Perspective

While reporting the sources, uses, and sums of campaign cash is important, it should be considered only a prelude to broader analysis of money's role in American politics. A growing literature on political finance inquires into the relationship between campaign spending levels and other political, economic, and social variables; the impact of campaign expenditures on registration, voting turnout and electoral outcomes; the concern that campaign spending is "too high"; and the influence of contributions on the policy-making process.

Political, Economic, and Social Variables. John Owens's study of political finance in California is a long step beyond reportage and toward analysis.[10] Owens has collected fascinating data on total campaign spending in California from 1958 to 1970; on outlays by candidates for national, statewide, and state legislative offices; on the division between primary and general election expenditures; and on the balance between Democratic and Republican spending. But Owens has taken the additional step of studying the relationship between campaign spending levels and other political, economic, and social variables, which previously had been investigated in only a "qualitative" manner.[11]

Using the chi-square test on data from California legislative and congressional races, Owens finds spending higher for Republicans than for Democrats, and also higher for incumbents, for winners, in closely competitive districts, and where party organization is strong. Contrary to theses advanced elsewhere, the size of districts and their urban or rural character do not change per-voter costs; nor do the differing cultures of Northern and Southern California. Owens shows by regression analysis, however, that the combined effect of all these factors accounts for only 10 per cent of the variance in spending; and he concludes that the factors typically cited by students of political finance are substantially inadequate.

In a subsequent study of congressional districts, Dawson and Zinser explore relationships between per capita income, the inequality of income distribution, rates of in-migration, racial mix, age stratification, population density, and the presence of a United States Senate race, on the one hand, and voter registration, campaign contributions and expenditures, voter turnout, and electoral outcomes, on the other.[12] They advance a "rational man" theory which implies that citizen political activity turns on investment and consumption motives discounted by the costs of civic action. Most of their results conform, however, to traditional explanations of differential political activity among citizens. Since the evidence is convincing that few citizens have vast stores of information on which to make political calculations (and many believe that the general public need not have such information to make democracy workable), the value of a model assuming high levels of rational calculation by citizens is unclear.

Dawson and Zinser suggest that per-capita income and income inequality in a district are positively correlated to total contributions, while higher percentages of minority races and young people as well as high population density reduce total giving to congressional campaigns. Incumbency tends only slightly to increase total contributions, but it significantly enhances the amount of small in-state contributions and, for Democrats, the amount of large, out-of-state gifts. Where party registration is high and evenly balanced, contributions to both parties increase, and the financial advantages of incumbency are somewhat decreased.

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[9] Heard, *The Costs of Democracy*, ch. 8.

[10] Owens, *Trends in Campaign Spending in California, 1958-1970: Tests of Factors Influencing Costs.*

[11] Owen's list of variables for testing was drawn from Heard, *The Costs of Democracy*, pp.280–287; David Adamany, *Financing Politics* (Madison: University of Wisconsin Press, 1969), pp. 61–107; and David Adamany, *Campaign Finance in America* (North Scituate, Mass.: Duxbury Press, 1972), pp. 51–78.

[12] Paul Dawson and James Zinser, "Political Finance and Participation in Congressional Elections," *The Annals* 425 (May, 1976), 59–73.

Many of these conclusions are methodologically elegant confirmations of previously established findings. The well-off make political gifts, so districts with high per-capita income but a wide disparity in income distribution, make more contributions because they include a larger percentage of wealthy people. Similarly, it has long been known that the young and minority races make few political contributions. Democratic incumbents receive substantial sums from outside their districts because Democratic electoral strength is relatively low among those high-income groups likely to make in-district contributions, and Democratic incumbents must therefore turn to national labor and liberal organizations for campaign cash. And where party strength is evenly balanced and districts are therefore likely to be competitive, each party may go to a relatively large number of activists with the appeal that their contributions are likely to affect electoral outcomes.

Seldom do Dawson and Zinser recognize that their findings have been suggested by earlier, less elegant studies. Indeed, one is compelled to ask whether they have surveyed closely the literature on political finance. Their research therefore does not build on existing studies, and the authors frequently explain their findings with a naivete that could have been avoided by rubbing shoulders with the work of other scholars more experienced in election practices.

Electoral Outcomes. Dawson and Zinser suggest that high expenditures increase voter turnout and that spending (together with incumbency and party strength) affects electoral outcomes. Common Cause and Owens also point out that winners spend more than losers. But is this an artifact of other relationships? Incumbents raise and spend more money than challengers; in most districts the incumbent's party has a long lead in voter allegiances; and incumbents therefore tend to be re-elected because of the voter composition of their districts rather than their edge in expenditures.

A number of scholars take a cautious or skeptical approach to the impact of money on electoral outcomes. Howard R. Penniman opposes limits on campaign spending, arguing that money does not have a large influence on voter decisions. His essay on "Financing Campaigns in the Public Interest" reminds readers that "the cost of providing the information [about election choices] is enormous, but the bulk of the expense is not borne by the parties or candidates."[13] Not campaigns, but events, news media, party affiliations, and the voters' retroactive judgments on regimes decide elections. Penniman does join others in acknowledging, however, that money may be quite important in primaries. Media coverage is less intense and the party label is not available to guide voters' choices; hence name identification, which campaign money can buy, may influence nomination contests.

In a companion essay, Ralph K. Winter endorses Penniman's skepticism that money decides elections.[14] Citing studies emphasizing that activation of voters and reaffirmation of prior opinion, not conversion to new beliefs, are the main effects of campaigning, Winter finds little danger that money decides elections. His analysis overlooks the argument that political funds might be used effectively and decisively in elections for activation (registration and getting out the vote) and reaffirmation (canvassing the party faithful). Winter does recognize that campaigning may have some desirable results: allowing the "outs" to gain visibility, which they can seldom do during the interelection season; permitting systematic challenges to the records of those in office; and allowing new issues or programs to be floated, even if they have no prospect for success.

Herbert Alexander has suggested that we may err to focus so heavily on the immediate electoral impacts of campaign spending.[15] Expenditures may serve other purposes—to quiet candidate anxieties, to impress reporters with the campaign's seriousness, to stimulate party workers and voters, to scare off future challengers or to lay the groundwork for a future campaign for higher office. In addition, Alexander reiterates that spending may not be related to electoral outcomes because politicians do not know what increment of marginal votes is won by each additional dollar spent. In politics, "not enough is known about the bedrock minimum needed to reach the electorate."[16] Consequently, while politicians do strive for a basic budget, they regard it as even more important to match or overmatch the opposition.

A somewhat different view holds that money may not be as important as other political resources and that a candidate with a "well-balanced" array of resources can usually raise adequate campaign funds. In their little-noticed but exceptional study of money in Tennessee Democratic senatorial primaries from 1948 to 1964, William Buchanan and Alice Bird suggest that a serious candidate would need about $150,000 to wage a serious campaign.[17] Money is only one of many political resources; and it must be ranked as relatively unimportant compared with a candidate's personal capacities, his issues and other appeals, his institutional alliances, and his campaign organization. These other resources usually can be converted to campaign funds; but money alone can rarely purchase these other, more critical, campaign resources.

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[13] Penniman, "Financing Campaigns in the Public Interest," *Campaign Finances: Two Views of the Political and Constitutional Implications,* p. 1.

[14] Winter, "Money, Politics and the First Amendment," *Campaign Finances: Two Views of the Political and Constitutional Implications.*

[15] Alexander, *Money In Politics,* p. 22.

[16] Alexander, *Money In Politics,* p. 37.

Some recent writers have argued that money is becoming more important in politics because of changing campaign techniques. In *Financing Presidential Campaigns,* Delmer Dunn stops short of asserting that money wins elections, but he does observe that "candidates' increasing use of high-cost modern technology inevitably will make money more decisive in the future in determining election results."[18] In a society where partisan identification is declining, split-ticket voting increasing, party organization atrophying, and mass media influence widening, Dunn's point is plausible.

Indeed, his argument gains support from several other quarters. An examination of 1970 congressional races concludes that broadcast expenditures do bear significantly on winning vote margins and that this tendency is especially pronounced in Senate contests.[19] Both incumbency advantages and one-partyism are stronger in small House constituencies, while media apparently play a greater role in statewide contests. In campaigns "a minimum [broadcast] expenditure per capita is necessary to break established voting patterns and voting turnout. . ."[20]

A study of the 1972 presidential campaign bears even more dramatic witness to the growing influence of media advertising and thus of campaign expenditures.[21] Patterson and McClure demonstrate that voters with little political interest are less likely to watch television news, to learn from it when they do watch it, or to retain what they learn. This is as much the fault of the network news formats as of the viewers. News focuses on high-excitement video shots, which obscure the audio discussion of issues. Presidential campaign advertising, by contrast, tends to have reinforcing video and audio messages. There was less issue content in national network news than in the campaign ads. And the news programs did not reiterate issue materials, while advertising tends to be repetitive and therefore to impress messages more firmly on the public consciousness.

As a result, frequent and infrequent watchers of television news tend to increase their information about politics at roughly the same rate. On the other hand, frequent viewers of television political advertising increased their information at a greater rate than infrequent viewers. "It seems clear, then, that television is able to boost the level of knowledge low-interest voters have about the candidates and issues. Indeed, television does make a contribution to that elusive democratic goal of widespread electoral participation by a well-informed citizenry. But the television presentation that best achieves that goal is political advertising, not network news."[22] And political advertising costs lots of money—not only for broadcast time itself, but for the pollsters and expert production specialists whose assistance is essential for the development of an effective mass media campaign.

Total Expenditures. Herbert Alexander puts into perspective the question of whether campaign spending is "too high" when he notes that we pay only insignificant shares of personal income or gross national product to sustain a democratic election system. Both he and Owens suggest that American political costs are not as high as those in other democratic nations. Increases in political spending did not far outrun increases in national wealth or inflation until the mid-1960s. Then total campaign spending "took off." Even after this sharp escalation of expenses, total nomination and election spending by all candidates for office was only about $3.00 per eligible voter in 1972.

Nonetheless, as Alexander points out, "costs are too high if, because sufficient money is not available, a candidate becomes beholden to those who will provide funds but with strings attached, or a campaigner is not able to achieve equitable access to the electorate, or a potential candidate is discouraged from running."[23] All those conditions seem to prevail in America, not because spending is too high, but because too little is spent and untainted money is often unavailable.

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[17] William Buchanan and Agnes Bird, *Money as a Campaign Resource: Tennessee Democratic Senatorial Primaries, 1948–1964* (Princeton: Citizens' Research Foundation, 1966).

[18] Dunn, *Financing Presidential Campaigns,* p. 9.

[19] Paul Dawson and James Zinser, "Broadcast Expenditures and Electoral Outcomes in the 1970 Congressional Elections, *Public Opinion Quarterly* 35 (Fall, 1971), 398–402.

[20] Dawson and Zinser, "Broadcast Expenditures and Electoral Outcomes in the 1970 Congressional Elections," p. 400.

[21] Thomas Patterson and Robert McClure, "Television and the Less-Interested Voter: The Costs of an Informed Electorate," *The Annals* 425 (May, 1976), 88–97.

[22] Patterson and McClure, p. 96.

Contributions and Policy. Finally, writers have been concerned with the effect of contributions on politics. Delmer Dunn has suggested that contributors have access to the election arena. The wealthy candidate or the one backed by substantial contributors has access to the voters and can therefore advance his personal ambitions and his program.

Dunn also asks, "Does money affect policy decisions?" A number of factors—public norms against money influence over officials, statutory regulations, candidate strategies to limit their obligations, and the wide mix of contributor motives—diminish the impact of contributions on policy. Nonetheless, Dunn quotes senators and staffers who acknowledge that contributors have access to officeholders. "The very least a contributor expects his contribution to do is to get him access the next time he has a problem he wants to take up with the officeholder."[24] Beyond access are implicit commitments. A "candidate's stance . . . tells potential contributors whether he be friend or foe. Solicitation by either the office seeker or his staff implicitly promises a sympathetic response to matters of interest to the giver. . . ."[25]

Alexander also emphasizes the complex of contributor motives: gratification in party or governmental influence, personal prestige, ego-satisfaction, belief in the two-party system, a sense of civic duty, ambition for appointments, the desire for good government, friendship with candidates or solicitors. But he also acknowledges that contributor goals often involve political, legislative, judicial or administrative action.

Ralph K. Winter takes a more sanguine view of contributor influence. Winter finds it "difficult to see how 'fat cats' can dominate office-holders unless campaign money is the critical determinant of elections."[26] In any case, he points out, campaign funds are not raised from a few monied interests with a common ideology but from an incredibly wide variety of groups and individuals. In *Campaign Financing and Political Freedom,* Winter quotes scholarly studies to show that contributors usually support candidates and parties whose stances coincide with their own; they give support rather than buying policy. For good measure, he notes that contributing is not directed toward maintaining the status quo. Radical movements have typically drawn their seed money from a handful of wealthy backers; and despite the alleged influence of conservative contributors, the last forty years have witnessed the passage of much social and regulatory legislation.

Winter's generalizations are correct. But they ignore the all-too-frequent exceptions, which bring money to bear on national policy. There is, first, a class bias in giving: the well off and well educated give disproportionately to campaigns and they impose a certain class style on American politics.[27] Second, Winter fails to acknowledge the recent tendency of ideological movements to draw disproportionate funding. Small but zealous groups may have an inordinate impact through many modest contributions. This is the apparent lesson of the Goldwater, Wallace, McCarthy, and McGovern campaigns. Third, Winter glosses over the advantages available to organized interests, especially powerful economic institutions, in gaining access both to the electoral arena and to officeholders. The massive contributions of government contractors, oil company officers and directors, labor unions, and others bear ample witness. Finally, of course, Winter wrote before the campaign of 1972 revealed attempts by ITT, the dairy interests, Robert Vesco, and numerous other corporations and individuals to influence government policy, law enforcement, and appointments.[28]

Conservatives, Radicals, etc.

If recent writing is the measure, the analysis of political finance often turns on the author's ideological perspective. The Penniman and Winter essays are, for instance, clear cases of conservative preference. Penniman accepts as fixed and worthy some aspects of American politics that others will doubt. Noting that one-partyism, incumbency, and other factors virtually predetermine the outcome of most congressional races, he concludes that public

[23] Alexander, *Money In Politics,* p. 20.

[24] Dunn, *Financing Presidential Campaigns,* p. 20.

[25] Dunn, p. 21.

[26] Winter, "Money, Politics and the First Amendment," p. 52.

[27] The class bias of campaign giving is described in David Adamany and George Agree, *Political Money* (Baltimore: The Johns Hopkins University Press, 1975), pp. 29–31.

[28] An outstanding treatment of the uses and misuses of money during the 1972 campaign is Alexander, *Financing the 1972 Election,* chaps. 3, 12, 13.

financing of campaigns would be wasteful. Penniman ignores other possibilities—that races are noncompetitive precisely because the opposition cannot muster enough resources to wage a vigorous campaign; or that a well-financed, albeit losing, opposition campaign would force greater accountability on officials in "safe" districts.

He makes similarly conservative assumptions about the party system. Correctly warning that public financing to either party organizations or candidates would shift the balance of influence within the party coalition, he does not explore the desirability of using campaign reform legislation to achieve such a shift. Nor does he consider that public financing for both the party of the activists and the party in government might disturb intraparty balances least and produce a desirable increase in political activity.

Winter's views are even more emphatically conservative. He finds no substantial bias in the distribution of big givers across the political spectrum. They are ordinarily "representatives" or "surrogates" for wider bodies of opinion, and their contributions do not therefore bias access to government or to the voters. Neither the evidence on the sociology of givers nor the studies of interest group contributions confirm his view.

Winter also insists that any restriction on political contributions or spending is incompatible with political freedom. Financing is a form of expression and must remain unfettered, he asserts. It does not disturb Winter that in a society based upon an equal ballot, his formulation gives wealthy individuals or groups massively unequal opportunities to influence the ballots of their fellow citizens.

Both Penniman and Winter are at their best when warning that restrictions on political money may only shift inequalities, not diminish them. Those with nonmonetary resources and those with interelection visibility may simply gain strength if campaign money is restricted. Winter even suggests that current campaign financing practices advantage "change agents," citing the McGovern, Goldwater, McCarthy, and Wallace campaigns as examples. He is not concerned, however, that a financing system which advantages some (but not all) ideologues as well as wealthy individuals and interest groups still does not meet democratic criteria for full presentation of all significant alternatives and for citizen equality in elections.

If Winter concludes too optimistically that givers are representative of the political spectrum, David Nichols is too certain that money is monolithic. His *Financing Elections* imposes the well-honed radical perspective on political finance analysis. Citing Parenti and Domhoff, Nichols argues that American wealth and power are highly concentrated in a corporate elite, that the political and social system maintain elite wealth and power, and that public attitudes and leadership are manipulated to win support for the system. Election financing is one of the most important techniques for the elite to maintain control over politicians and officials. Finally, effective reform requires a total restructuring of American society to make it classless and to give the whole people equal control over the economic system.

This argument finally turns on intellectual sleight of hand. Sometimes the elite is given credit for specific policies which favor corporate wealth. But when leaders in society are divided over such issues, Nichols retreats to the more general formulation that concurrence in the overall political order is the measure of elite domination. What about those who, while generally supporting the political system, advocate specific reforms that spread and equalize political power? In Nichols's eyes, they too are creatures of the corporate elite: reform can never be effective without a total restructuring of society, and incremental reform merely shores up the corporate elite by ameliorating discontent. The measure of elite domination, then, is the absence of substantial leadership or public support for Nichols' personal definition of democracy—a society without economic or social classes.

Nichols's analysis of election financing is often insightful. He recognizes that big givers are especially influential because they contribute the early money essential for the start-up stages of campaigns. He understands the great importance of money in the nomination stage. He correctly argues that contributions mean access: first to the voters in elections, then to officeholders who receive the money. He is not misled by cries that campaign expenditures are too high. "The crucial question is not the amount raised but its source and political character."[29] Although Nichols has contributed no new research, his commentary often sharply highlights the bias of the present campaign financing system.

His shortcoming is that he is unwilling to consider that the bias may be pluralist in nature—that various groups, often competing, but not necessarily representative of the full range of public opinion, are unfairly advantaged in pushing their policy preferences because they contribute significantly to politics. Hence Nichols insists there is only one elite and that its common tie is exclusively economic. He is led to tortured interpretations: the same economic motivations for contributing to Democrats must be imposed on "a small number of Protestant patricians" together with "the Jewish businessmen of New York and the Cowboy oilmen of Texas."[30] It does not occur to Nichols that Jews may give because they care about civil liberties or Israel, that Corliss Lamont may really wish to use his wealth to promote a more equalitarian society, or that Texas oilmen may be motivated by traditional party allegiances as well as by economic self-interest.

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[29] Nichols, *Financing Elections*, p. 60.

The same technique is repeated to discredit those advocating finance reform. Their objective, Nichols insists, is not to diminish the political power of big givers, but to repair badly tattered public confidence in the existing elite political order. To prove his point he selectively cites the reformers' arguments. If they advanced many reasons for reform, and if one of the reasons was the maintaining of public confidence in the political process, Nichols cites that as their only or true reason for seeking change.

In his haste to make his point, Nichols often slips into factual errors of real consequence. He is mistaken in asserting that the corporate elite engages in "split contributions" to keep Democrats as well as Republicans financially dependent on them. Herbert Alexander has discovered relatively few split givers among big contributors. More important, Alexander has demonstrated that in one presidential election after another contributions by those affiliated with major trade associations, by officers and directors of government contractors, by leaders of the largest corporations, by families of established wealth, and by the nation's centimillionaires go more than 80 per cent, and sometimes as high as 95 per cent, to the Republican party.

Nichols is too eager to show that McGovern, like other pro-system candidates, was controlled by big givers; McGovern did not receive half his money from givers of more than $100, as Nichols reports. The figure was about one-third. (Besides, if $100 is the measure of corporate elite giving, a fair number of readers of this essay have suddenly joined the ranks.)

He also badly garbles the text of recent legislation, significantly misstating the 1971 Act's limit on media spending and the Revenue Act's provision for tax incentives. Again, Nichols asserts specifically that neither I nor others who advocate public subsidies would "cut out the input of private big money in national politics." In fact, however, most reformers who testified in favor of the 1971 Act, the liberal senators who drafted the bill, and my book cited by Nichols all suggest limits on individual contributions.

In sharp contrast to both the conservative and radical views is Herbert E. Alexander's *Money In Politics*. Unlike most of Alexander's writing, which describes and details financing practices, this volume puts political money in broader perspective. To be sure, Alexander uses the fruits of his own past labors and those of other scholars. He reviews the role of money in campaigning; the amounts and causes of spending; the uses of money; the price of presidential nominations and elections as well as of congressional politics; the financing of national party committees; the sources and sizes of contributions; and the place of special interest money, especially from labor and business. Alexander also describes the state of American campaign finance laws, the history of funding reform, and the main issues that have surfaced in debates over new legislation.

These traditional subjects are cast, however, in a framework which favors the American political system, but looks to change the role of money in politics. Alexander espouses a democratic theory based upon political equality. Unlike Nichols, he does not hold that this necessarily means social or economic equality. For him, "the basic conflict in politics, then, has been between those who would use political power to protect or augment their economic power and those who would offset economic power with political power or votes."[31] The main issue in election financing is to reconcile the doctrine of political equality with the reality of unequal economic resources.

Alexander wisely acknowledges that money is only one of many political resources which are unevenly distributed. But he understands that it can be more unequally distributed than most, that it is more liquid than others, and that it can be converted into many other political resources. Hence greater equality in money influence is desirable, even though exact equality in financing or in other resources is impossible. Indeed, if ample money is available for campaigns, drawn on a roughly equal basis from all willing citizens, the effects of other, unequally distributed political resources may be reduced. In any case, limiting political spending does not advance democracy. Alexander favors vigorous politics, and that costs money.

Alexander's insistence on retelling the story of political finance sometimes obscures the theme of his book. But his recounting is informative and often fascinating. The real contribution of *Money In Politics,* however, is its search for means to make political financing conform to the principle of political equality in a society which also values political freedom, a society which as yet shows no sign of eliminating inequalities in its social or economic systems.

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[30] Nichols, pp. 93–94.

[31] Alexander, *Money in Politics*, p. 12.

Reform: The Legislative Process

Few issues have been thrust more persistently before Congress in recent years than campaign finance reform. The tax check-off was adopted in 1966 and rendered inoperative in 1967; the Honest Elections Act was reported by the Senate Finance Committee in the same year but was not acted on; the Political Broadcast Act passed Congress in 1970 but was vetoed by Richard Nixon; the Federal Election Campaign Act and the Revenue Act, revising the check-off and adopting tax incentives, were passed in 1971; the Federal Election Campaign Act described at the beginning of this essay became law in 1974; and the Federal Election Campaign Act Amendments of 1976, curing constitutional defects in existing laws, were enacted in the midst of last year's presidential campaign. Already there are legislative attempts to strengthen the law, especially by extending public financing to congressional races.

In *Money In Politics,* Herbert Alexander devotes one chapter to describing federal and state campaign finance regulations before 1971 and another to tracing in detail attempts to pass new federal legislation, especially after the 1962 report of President Kennedy's Commission on Campaign Costs.[32] (Alexander served as its director.) He attaches an epilogue spelling out the provisions of the 1971 Act, which was passed after his book manuscript was complete.

Alexander supplies a valuable assessment of the 1971 Campaign Act in his *Financing the 1972 Elections.*[33] He recounts the role of the General Accounting Office, the House and Senate filing officers, and the Justice Department in 1972. There plainly emerges a picture of the GAO struggling to devise effective reporting requirements and to forward serious violations to the Justice Department for prosecution. These efforts were hampered by the division of authority between the Comptroller General, on the one hand, and the Senate Secretary and House Clerk, on the other. The congressional officers were, in addition, less militant about enforcement, as they carefully watched over their shoulders the reaction of their congressional employers, especially Wayne Hays, whom they were supposed to regulate. The Justice Department, for its part, did so little to prosecute reported violations that the Comptroller General was compelled to urge that "the Attorney General take the initiative with regard to reported violations of the Federal Election Campaign Act."[34]

Alexander's descriptions of congressional action on the various campaign laws sometimes seem too favorable toward congressional Republicans, who as the minority party could more easily posture on the issue than the majority-party Democrats.[35] Further, Alexander's long-standing preference for full disclosure was more consistent with Republican strategy, which opposed expenditure ceilings, contribution limits, or public financing that might ameliorate their consistent advantage in campaign funds. He also judges Senator Russell Long much too harshly for his unrelenting, even obdurate, insistence on the tax check-off in 1966 and 1967. I believe that Long will be praised by history for almost singlehandedly forcing the public financing option into the center ring of public and legislative attention.

Robert Peabody, Jeffrey Berry, William Frasure, and Jerry Goldman have written of the Broadcast Act of 1970 in much the same manner as Stephen K. Bailey's *Congress Makes A Law* or Daniel Berman's *A Bill Becomes A Law. To Enact A Law* opens by announcing its dual objectives: "first, to introduce the student to the process of how a bill becomes or fails to become a law and, second, to shed light on the thorny, complex, and vital problem of campaign finance reform."[36]

The authors have written about a measure unusual in at least two respects. First, there was little interest-group activity surrounding the 1970 measure. The NCEC was a driving force; the National Association of Broadcasters became active late in the process; and the United Auto Workers gave the bill nominal endorsement. The Political Broadcast Act, like other campaign finance measures, is different because all congressmen are, or at least fancy themselves, experts on the issue. The room for interest group or executive influence is consequently diminished. Second, the bill was vetoed by Mr. Nixon and his veto was sustained. Both aspects of this study will make it unusual in the legislative process literature and especially useful.

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[32] President's Commission on Campaign Costs, *Financing Presidential Campaigns* (Washington, 1962).

[33] Alexander, *Financing the 1972 Election,* ch. 2. For the Comptroller General's perspective, see Elmer Staats, "Impact of the Federal Election Campaign Act of 1971," *The Annals* 425 (May, 1976), 98–113.

[34] Alexander, *Financing the 1972 Election,* p. 29.

[35] Alexander, *Money in Politics,* chap. 12.

[36] Peabody et al., *To Enact a Law,* p. 5.

There is, as yet, no comprehensive treatment of the politics of the 1971 or 1974 Acts. Jeffrey Berry and Jerry Goldman have followed *To Enact A Law* with an insightful and lively essay on the congressional maneuvering that finally led to the Federal Election Campaign Act of 1971, requiring sweeping full disclosure of political money.[37] George Agree and I have written a brief essay commenting on the provisions of the 1974 Act as well as touching on the interest group activity and Byzantine congressional maneuvers that attended its passage.[38] More extensive scholarly treatments of these laws would be warranted, for both, and especially the 1974 Act's public financing provisions, represent the kind of turning point in American elections that previously occurred, for instance, in the Voting Rights Act of 1965. Finally, an invaluable report from the Federal Election Commission summarizes the provisions of new state and federal laws, current to December, 1975, and including statutory citations for reference purposes.[39] A brief perusal of this document alerts the reader to the rich diversity of campaign finance regulations that have been undertaken by state governments.

Toward Reform

Spending and Contribution Limits. Students of political finance are apparently more inclined to recommend policy than are other political scientists. Seven of the books reviewed here include more or less complete reform programs. There is virtually a consensus that expenditure limits are unwise and unconstitutional. Alexander, Dunn, Penniman, and Winter persuasively argue the policy merits. It would be difficult to devise equitable limits for varying districts and political situations; expenditures increase the level (if not always the quality) of political debate and should therefore be encouraged; spending is usually not decisive in campaigns, so the need for limits is minimal. Max McCarthy supports a ban on the purchase of television time. (He favors free time for candidates.)

The scholarly debate over contribution limits has all too often been simply a rehash of the dispute over spending ceilings. Only Max McCarthy favors limits on giving, and then with the prayer that some day all private contributions will be banned. The others tend to follow Herbert Alexander's reasoning that the "constitutional arguments against limiting campaign spending also apply against limiting contributions; specifically, it is the right of an individual to spend his money to support a congenial viewpoint. . . ."[40] This argument overlooks entirely the power of government to curb action related to speech for the compelling purpose of protecting the integrity of elections and of government. Threats to that integrity include contributors' special access to voters and to officeholders in a political system based on the one-man/one-vote principle. And, unlike spending limits, contribution curbs do not cut off entirely anyone's opportunity to be heard: everyone can be heard to the same limit.

This distinction between spending and contribution limits has, of course, been adopted by the Supreme Court.[41] The justices view spending limits as a curb on the freedom of expression. And they follow Professor Ralph Winter's argument that the mechanisms necessary to enforce spending limits would cut off some viewpoints entirely. To cap spending, all monev must flow through the candidate to whom the expenditure ceiling applies. But this means that he must control what money is spent for, denying individuals the opportunity to air views different from his own. Contribution limits, on the other hand, can be distinguished from expenditures. "A contribution serves as a general expression of support for the candidate and his views, but does not communicate the underlying basis for the support."[42] Here the free speech interest is less urgent, and the Supreme Court held that it is "unnecessary to look beyond the Act's primary purpose—to limit the actuality and appearance of corruption resulting from large individual financial contributions—in order to find a constitutionally sufficient justification [for contribution limits]."[43]

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[37] Jeffrey M. Berry and Jerry Goldman, "Congress and Public Policy: A Study of the Federal Election Campaign Act of 1971," *Harvard Journal on Legislation* 10 (February, 1973), 331–365.

[38] David Adamany and George Agree, "Election Campaign Financing: The 1974 Reforms," *Political Science Quarterly* 90 (Summer, 1975), 202-211.

[39] Federal Election Commission, *Analysis of Federal and State Campaign Finance Law* (Washington, 1975).

[40] Alexander, *Money in Politics,* p. 234. Although the Supreme Court's decision in *Buckley* will modify much of the commentary, the most thoughtful analyses of the congressional power to regulate campaign finance, the constitutional limits on such regulations, and the specific problems of expenditure limits, contribution ceilings, disclosure, and public subsidies are Albert J. Rosenthal, *Federal Regulation of Campaign Finance: Some Constitutional Questions* (Princeton: Citizens' Research Foundation, 1972); Joel L. Fleishman, "Freedom of Speech and Equality of Political Opportunity: The Constitutionality of the Federal Election Campaign Act of 1971," *North Carolina Law Review* 51 (January, 1973), 389–483; Joel L. Fleishman, "Public Financing of Election Campaigns: Constitutional Constraints on Steps Toward Equality of Political Influence of Citizens," *North Carolina Law Review* 52 (December, 1973), 349–416; and Joseph R. Biden, Jr., "Public Financing of Elections: Legislative Proposals and Constitutional Questions," *Northwestern University Law Review* 69 (March–April, 1974), 1–70.

[41] 96 S.Ct. 612, at 634-635. My own view that the constitutional issues surrounding expenditure and contribution limits are separable is spelled out in Adamany and Agree, *Political Money,* pp. 62–70.

In *Buckley*, the Supreme Court either ignored or gave little weight to arguments relying on theoretical concepts of voter equality in a political democracy or on the specific guarantees of the Equal Protection Clause of the Fourteenth Amendment. Albert Rosenthal has pointed out that "where equal protection and First Amendment values clash, the latter are consistently given precedence."[44] Even the approval of contribution limits was based on the congressional authority to protect the integrity of elections and government rather than on concepts of citizen equality. And equal protection arguments were rejected or ignored by the justices when they struck down limits on a candidate's own contributions to his campaign and invalidated spending ceilings that might have equalized the political advantages of incumbents over challengers or of candidates supported by wealthy interests over those without such support. Nor were equal protection interests given serious weight when the Court upheld the 1974 Campaign Act's provision of federal matching grants up to $250 for each private contribution of like amount in presidential nomination races, as against the claim that the law unduly advantaged well-off citizens who could afford to make such generous gifts.

Despite the short shrift given equality arguments by scholars and justices, Professor Marlene Arnold Nicholson has provocatively insisted that such a basis will support at least contribution limits and might, indeed, make public financing mandatory.[45] Professor Nicholson begins with the argument of the reapportionment cases that voters should have an "equally effective voice in elections;" and she continues by relying on Supreme Court decisions striking down large filing fees for candidates because they discriminate against poor voters (not candidates) whose favorites cannot qualify for the ballot. Congress could impose contribution limits to guarantee equality for voters, not candidates, she argues; and the congressional finding of discrimination would be given heavy weight by the Supreme Court because of the legislative expertise in campaigning. But beyond congressional action, there might well be a Fourteenth Amendment mandate to substitute public money for private contributions, because only in this way can poor voters prevent wealthy candidates, large contributors, and established economic interests from swamping their equal voice in elections and overbearing those candidates who represent their interests.

Disclosure of Political Money. The reform favored by virtually all writers is full disclosure of campaign receipts and expenditures. Thayer also expresses the enthusiasm of Alexander, Dunn, and McCarthy when he argues that "disclosure is the vital core of campaign finance law."[46] Armed with the facts about political money, voters will deal harshly at the polls with candidates who spend too much or receive large sums from disfavored interests.

These advocates have not, however, allayed significant doubts about the worth of full disclosure. Can the underfinanced opposition afford to campaign on disclosures? Will news media use finance disclosures which embarrass their editorially preferred candidates? Can citizens be expected to understand the mass of detailed information that emerges from finance disclosures?[47] And if they do, should they weight financing practices heavily in casting ballots that also resolve questions of foreign policy, economic and social welfare policy, race relations and civil liberties and so forth?[48]

Only Ralph Winter applies the same strict constitutional standards to full disclosure that all used in judging expenditure and contribution limits. Noting Supreme Court decisions that the First Amendment freedom of association is abridged by state requirements that the NAACP file membership lists or that teachers reveal all organizational affiliations, Winter argues that disclosure of contributors also impairs their right to associate for political purposes by opening them to retaliation for their campaign gifts. He acknowledges that there is a public interest in disclosure, but only for very large gifts that might jeopardize the integrity of elections or government.

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[42] 96 S.Ct. 612, at 635.

[43] 96 S.Ct. 612, at 638.

[44] Albert J. Rosenthal, "The Constitution and Campaign Finance Regulation After *Buckley v. Valeo*," *The Annals* 425 (May, 1976), 130.

[45] Marlene Arnold Nicholson, "Campaign Financing and Equal Protection," *Stanford Law Review* 26 (April, 1974), 815–854.

[46] Thayer, *Who Shakes The Money Tree?*, p. 296.

[47] The difficulty of making effective use of federal campaign finance reports is illustrated by the fact that 269,500 pages of data were received by the filing officers during the 1972 campaign. How can this volume of information be examined and the crucial information transmitted to the public during the short term of a political campaign? See Alexander, *Financing the 1972 Election*, p. 12.

[48] My own reservations about the effectiveness of disclosure have been expressed in Adamany and Agree, *Political Money*, pp. 103–115.

In *Buckley,* the Supreme Court acknowledged many of Winter's objections but finally balanced the constitutional interests in favor of the sweeping disclosure provisions of the 1971 and 1974 Acts. The justices found three overriding public interests in disclosure: first, providing voters with information on the sources and uses of campaign money; second, deterring actual corruption and avoiding the appearance of wrong doing by exposing large contributions and expenditures; third, enforcing the contribution limits. To protect associational interests, however, the Court narrowed disclosure to candidates, to committees which were exclusively political, and to other groups to the extent that they were engaged in "express advocacy" of the election or defeat of office-seekers. Mere comment on public questions could not trigger disclosure requirements without cutting too deeply into First Amendment guarantees.

In addition, the justices expressed discomfort at the requirements for recording all sums exceeding $10 and disclosing those exceeding $100; nonetheless, the Court bowed to the congressional judgment because it could not say "on this bare record, that the limits designated are wholly without rationality."[49] Finally, acknowledging that unpopular minor parties might suffer from disclosure, the justices held that they must constitutionally be exempt from disclosure when they can offer evidence to show "a reasonable probability that the compelled disclosure of a party's contributors' names will subject them to threats, harassment or reprisals from either government officials or private parties."[50]

Public Financing. Government subsidies are also favored by many writers, but there is little agreement on their form. Alexander, Dunn, and Thayer favor tax incentives for campaign contributions, with the objective of encouraging more small gifts. Their focus ignores substantial social science evidence that socioeconomic status, level of political information, sense of political efficacy, and comparable variables are crucial to political participation, including giving money. A small tax incentive is unlikely, therefore, to change giving patterns. Moreover, none of these writers closely analyzes the effects of tax incentives already adopted by several states. There is reason to believe that tax incentives do not stimulate additional contributions, but simply create tax windfalls for those who give anyway.[51]

Alexander, Dunn, and Thayer also urge matching grants. These have the advantage of relying on citizen participation and following citizen choices of parties or candidates. They avoid the troublesome question of determining a candidate's or party's eligibility for public financing. On the other hand, they might not stimulate additional giving. And they would double the financial advantage of those with support among groups which already give—upper socioeconomic classes, ideological movements, Republicans, and conservatives.

Max McCarthy, on the other hand, urges flat grants and free media time to candidates. Dunn does not go quite as far, urging reduced media broadcast rates for candidates, a subsidy to presidential candidates for the purchase of broadcast time, and a "services package" which includes the franking privilege, low postal rates, and free voting lists. McCarthy ignores the prickly question of who shall qualify for public subsidy; and his proposal for substantial free broadcast time to all major presidential, Senate, House, gubernatorial, and mayoral candidates would impose on New York viewers 24-hour-a-day political messages for at least a month before the election.

Dunn suggests a subsidy for general election presidential candidates whose party received five per cent or more in the prior polling. Support for candidates may upset delicate balances between office-holders and parties; aid only to presidential contenders may alter the separation of powers; and Dunn's five per cent rule ignores newly emerging movements with substantial support and artificially perpetuates parties when they no longer have a popular following. Despite his large 1968 vote, George Wallace would not have qualified under Dunn's proposal; but because of that large vote, John Schmitz would have received bountiful support in 1972, despite his minimal electoral support. Subsidy in general elections, but not primaries, would simply move existing evils back to the nomination stage, where money has more impact.

Howard Penniman argues that any public financing would be wasted in safe districts and that it would not diminish nonmonetary disparities in political resources. Ralph Winter opposes public financing on similar grounds, adding that it might discourage private participation. He also worries about devising a fair and constitutional formula for determining eligibility. Further, when "an individual is forced, in effect, to make a contribution to a political movement to which he is indifferent or which he finds distasteful, it may fairly be said that a basic freedom is infringed."[52] The same may fairly be said about the extraction of tax dollars to support public education, public broadcasting services, fluoridation of water, or warfare in Viet Nam. Public financing of campaigns serves the "general welfare" no less than these outlays, and perhaps more.

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[49] 96 S.Ct. 612, at 665.

[50] 96 S.Ct. 612, at 661.

[51] Adamany and Agree, *Political Money,* pp. 123–128.

The most unusual reform is urged by David Nichols. Democratic financing of campaigns depends, he argues, on a mass, dues-paying party. He takes the pre-war German Social Democrats as his model. Nichols apparently does not know about current studies showing that European Socialist parties, including the SPD, have suffered declining dues support and have been unable to rely on this source to finance campaigns.[53] Attempts to organize dues-paying middle-class parties in the United States have also failed, despite the greater ability and propensity of those classes to make political contributions.[54]

Congress has, of course, enacted some public financing of campaigns. Tax deductions and credits are now law. The income tax check-off has shown growing support among Americans, with 25 per cent checking off in 1975. The Campaign Act of 1974 provides public matching grants up to $250 to presidential nomination candidates of qualified parties. The presidential nominees of those parties are primarily financed from check-off funds—a subsidy amounting to $21.8 million in 1976. National, state, and local parties can spend small additional sums of private money in support of their presidential nominees. And, finally, national party conventions receive public subsidies.

The Supreme Court sustained these provisions in *Buckley*. Public financing of campaigns is a constitutionally valid expenditure under the General Welfare Clause of the Constitution. The five per cent eligibility requirement for minor parties and 25 per cent threshold for major parties serve valid constitutional interests: to withhold public funds from those with no significant public support and to avoid governmental incentives to "splintered parties and unrestrained factionalism."[55] The $250 matching grant provision reduces the influence of large givers as compared to past campaigns, and it can therefore be sustained. The requirement that a pre-nomination candidate qualify by raising $5000 in matchable grants in 20 states is also acceptable, since the Electoral College is organized by states. Finally, the argument that emerging political movements are disadvantaged by qualifying thresholds based on voting in the last election is answered because they can also qualify for postelection grants if they receive sufficient votes in the present balloting.

What may be constitutional may not necessarily be wise. Public funding only for presidential campaigns may alter the balance between Congress and the President. Funding based on a prior election may encourage declining parties to contest elections. And the incongruity between those who supply public funds by checking off one dollar and those who allocate public funds in nomination contests by contributing $250 is readily apparent. Finally, the attempt to eliminate private funding in general election campaigns unwisely reduces opportunities for citizen participation.

Where Next?

The Federal Election Campaign Act Amendments of 1976 must be regarded as primarily corrective.[56] They eliminated congressional appointment of certain members of the Federal Election Commission because the Supreme Court had decided in *Buckley* that such appointees could not exercise quasi-judicial or executive functions. Only presidential appointees can wield such powers. Limits on expenditures and on candidates' contributions to their own campaigns were repealed, in accordance with the *Buckley* decision. Such limits remain in force only for candidates who accept public subsidies, an arrangement sanctioned by the justices. Contribution ceilings were revised to apply to gifts to political party committees and other political groups as well as to candidates. In presidential nomination contests, candidates become ineligible for further public funding if they receive less than 10 per cent of the vote in two consecutive primaries in which they participate. (They may reestablish eligibility by receiving 20 per cent in a subsequent contest.) Finally, since the Court held that expenditure limits in presidential races could not be applied to outlays by individuals and groups made without consultation or cooperation with the candidate, Congress more carefully defined such "independent expenditures" and imposed special disclaimer requirements on them.

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[52] Winter, *Campaign Financing and Political Freedom*, p. 25.

[53] A survey of the failure of parties, including Socialist parties, in many nations to meet campaign costs from dues payments is found in Arnold J. Heidenheimer, "Comparative Party Finance: Notes on Practices and Toward A Theory," *Journal of Politics* 25 (November, 1963), 791–795. Also, Leon D. Epstein, *Political Parties in Western Democracies* (New York: Praeger, 1967, pp. 245–246. Increasingly the German Social Democrats have relied on public subsidies rather than membership dues. Ulrich Duebber and Gerard Braunthal, "West Germany," *Journal of Politics* 25 (November, 1963), 784–786. Also, Uwe Schleth and Michael Pinto-Duschinsky, "Why Public Subsidies Have Become the Major Sources of Party Funds in West Germany, but Not in Great Britain," in *Comparative Political Finance*, ed. Arnold J. Heidenheimer (Lexington, Mass.: D.C. Heath, 1970), pp. 43–46.

[54] Adamany, *Financing Politics*, pp. 175–181.

[55] 96 S.Ct. 612, at 671 (1976).

[56] The Federal Election Campaign Act Amendments of 1976, Public Law 94–283, 94th Congress, 2d Sess., 1976.

Large questions remain unanswered. Even if constitutional, are the nation's campaign finance regulations fair and wise? With the 1976 election behind us, the time is ripe to ask whether the new campaign finance laws are effective. The diverse experiments in the states must also be described, analyzed, and evaluated.

Major constitutional questions linger. Edwin Epstein has pointed out that the Court declined in *Buckley* to decide whether long-standing expenditure limits on corporations, labor unions, and government contractors are valid. Albert Rosenthal has warned that even the rules laid down in *Buckley* may not be settled. The Court took the case under special legislative provisions giving it jurisdiction without a fully developed factual record. As a result, the justices mainly sustained the campaign laws because "they were not unconstitutional 'on their face.' " The Court "expressly or implicitly left open the possibility that later attacks . . . predicated upon evidentiary showings of probable injury to designated individuals, parties, or classes would be given fresh consideration."[57] Among those issues are attacks on contribution limits by major or minor party challengers alleging they are handicapped in combating incumbents, and claims that public financing of major party presidential nominees discriminates against minor party candidates.

During the 1976 campaign, candidates and their managers complained that the public subsidies provided were too small. This lends weight to the contention that in the United States, unlike other democracies, government subsidies were mistakenly "conceived and promoted as a means of replacing, rather than supplementing, private funding, and of capping, rather than helping to meet, the rising costs of campaigning for votes."[58] Even these intentions may, however, have been vitiated by the Supreme Court's invalidation of limits on independent expenditures. "Unchecked streams of big money now can continue to flow, albeit somewhat indirectly through unauthorized committees. Every precedent in the history of campaign finance regulation supports the expectation that whatever is permitted will be done."[59] Indeed, such developments would leave us in worsened circumstances, for they would permit organized interests to continue to flex their political muscles by acting through independent committees while diminishing the participation of small contributors who have usually acted directly through the candidate's own campaign organization.

Scholars are faced on every side with dilemmas and challenges. Constitutional restrictions block some avenues of change. Public expectations block others. There is ambivalence about the goals of political finance reform. And there is very little analysis of the consequences that occur when reform is enacted. It is apparent that scholarship on political finance falls far short of properly advising policymakers and the public about campaign finance practices, the possible goals of reform, the avenues available for meeting those goals, and the consequences of such legislative steps.[60] Yet the question of how to regulate political finance is now before the nation, and it will not wait long for further action. There is a rare opportunity here to harness scholarship to the public interest. But it must be done quickly.

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[57] Rosenthal, "The Constitution and Campaign Finance Regulations after *Buckley v. Valeo,*" p, 132.

[58] George Agree, "Public Financing After the Supreme Court Decision," *The Annals* 425 (May, 1976), 135.

[59] Agree, p. 135.

[60] My views on policy goals and measures to achieve those goals, especially a voucher system for public financing, are advanced in Adamany and Agree, *Political Money,* esp. chaps. 10 and 11. Some of these proposals and their supporting arguments are elaborated in Agree, "Public Financing After the Supreme Court Decision."

Political Statistics for the United States: Observations on Some Major Data Sources*†

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> "For so it is, oh Lord my God, I can measure it, but what it is that I measure I do not know."
>
> –St. Augustine

> "Are not two sparrows sold for a farthing?"
>
> –Matthew, 10:29

> "Are not five sparrows sold for two farthings . . ."
>
> –Luke, 12:6

> "There is no safety in numbers, or in anything else."
>
> –James Thurber

The saintly epigraphs point to the central issues in evaluating these compilations–all 56 pounds worth–of political statistics:

(1) Just what is it that is measured? Why measure it? Who cares? Do the numbers have any political meaning?

(2) What is the error structure of the measure? Does the ratio of an honest estimate of error to a formal estimate of error exceed two? Three? Five? Or is the ratio completely unknown?

(3) How do the conceptual specification and the error structure affect what we might learn from the numbers, as well as the inferential security of what we might learn?[1]

Conceptual Specification

A collection of statistical information is valuable, from the user's point of view, if it

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**The Almanac of American Politics, 1976.* By Michael Barone, Grant Ujifusa, and Douglas Matthews. (New York: E. P. Dutton, 1975. Pp. xviii, 1054. $7.95.)
Congressional District Data Book, 93d Congress. Bureau of the Census, Social and Economic Statistics Administration, U.S. Department of Commerce. (Washington, D.C.: U.S. Government Printing Office, 1973. Pp. xvii, 550. $8.30.)
Congressional Directory, 1976, 94th Congress, 2nd Session. Joint Committee on Printing. (Washington, D.C.: U.S. Government Printing Office, 1976. Pp. xxxii, 1146. $8.50 in cloth, $6.50 in paper.)
County and City Data Book, 1972. Bureau of the Census, Social and Economic Statistics Administration, U.S. Department of Commerce. (Washington, D.C.: U.S. Government Printing Office, 1973. Pp. lvi, 1020. $18.65.)
The Gallup Poll: Public Opinion 1935–1971. By George H. Gallup. (New York: Random House, 1972. Pp. xliv, 2388, in 3 volumes. $95.00.)
Guide to U.S. Elections. Edited by Robert A. Diamond. (Washington, D.C.: Congressional Quarterly, 1975. Pp. xvi, 1103. $48.50.)
Party Strength in the United States, 1872–1970. By Paul T. David. (Charlottesville, Virginia: University of Virginia Press, 1972. Pp. xiii, 310. $9.75.)
The Political Marketplace. Edited by David L. Rosenbloom. (New York: Quadrangle Books and Campaign Communications Institute for Politics, Inc., 1972. Pp. xix, 949. $25.00.)
1973 Republican Almanac. Produced by the Political/Research Division, Republican National Committee, 310 First Street, S.E., Washington, D.C. 20003. (Washington, D.C.: 1973. Pp. 422. No price.)
1970, 1972, and 1974 Congressional Vote Statistics. National Republican Congressional Committee, Pierre M. Purves, Director, Statistics. (Washington, D.C.: National Republican Congressional Committee, 1971, 1973, and 1975. No pagination, each volume approximately 500 pages. No price.)
State and National Voting in Federal Elections, 1910–1970. By Edward Franklin Cox. (Hamden, Connecticut: Archon Books, Shoe String Press, 1972. Pp. xv, 280. $15.00.)
Statistical Abstract of the United States, 1975. Bureau of the Census, U.S. Department of Commerce. (Washington, D.C.: U.S. Government Printing Office, 1975. Pp. xx, 1050. $8.00 in paper, $10.50 in cloth.)
Voting and Registration in the Election of November 1972. U.S. Bureau of the Census, *Current Population Reports,* Series P-20, No. 253. (Washington, D.C.: U.S. Government Printing Office, 1973, Pp. iii, 214. $3.30.)

†I am indebted to Vincent Barabba, former Director of the Bureau of the Census, for his very detailed comments on an early draft of the manuscript. We do, however, remain in disagreement on some issues. Criticism was also provided by John McCarthy (University of California, Berkeley), Margaret E. Martin (NAS-NRC Committee on National Statistics), and Donald Stokes and Dennis Thompson at Princeton. William Kruskal (University of Chicago) and Robert Russell, O.S.A. (St. Thomas Monastery) made valuable epigraphical contributions. Financial support came from the Woodrow Wilson School of Public Affairs at Princeton University and from a fellowship at the Center for Advanced Study in the Behavioral Sciences. I wish also to thank several anonymous reviewers and Dr. Ellen Y. Siegelman of the *Review* for their helpful comments. These individuals and institutions bear no responsibility for the faults or the views of this report.

[1] Good discussions of these perspectives are found in Tjalling C. Koopmans, "Measurement without Theory," reprinted with replies and counter-replies in *The Scientific Papers of Tjalling C. Koopmans* (New

provides the right measures for the right units of observation. For the compilers of data, the basic choices—within the limits of budgets and data availability—are, then, what to measure and tabulate, for what units of observation, with what editorial and supporting notes. In the documents at hand, four conceptual themes have guided the choices in data collection and compilation:

(1) *Data Dumping, or the Putting Together of Statistical Compilations with a Shovel.* What's important is whatever has been collected for whatever reason, if any.

(2) *Data of Fashion.* What's important is that which is topical at the time of the data collection and which soon passes away—for example, the public's attitudes, measured and transcribed by pollsters, toward fallout shelters, skirt lengths, and John Connally.

(3) *Data for Political Action.* What's important is what helps win elections, attack enemies, or fight out issues.

(4) *Data for Political Understanding.* What's important is what helps describe and understand political life with a relevance over a longer run than the last fad or the next election.

While everybody is for Understanding, the perspectives chosen in practice depend naturally enough on the compiler's patrons and purposes: the biennial volume of electoral statistics published by the Republican National Congressional Committee revolves around Action; the Gallup Poll compilation around Fashion and sometimes Understanding; and the Census Bureau publications around Dumping and Understanding, with an occasional unfortunate venture into Action.

The Dumping approach to statistical publication is all too well illustrated by the Census Bureau's *Voting and Registration in the Election of November 1972,* which contains 12 uninformative pages of text and 202 pages of tables, each containing nearly 400 separate pieces of data. It is all based on a few questions included in the "November voting supplement" to the Current Population Survey (CPS) executed in November, 1972; somewhat similar supplements have been used since 1964. In 1972, the following questions were asked of about 47,000 households (the exact sample size and the response rate are not given): Was the respondent registered to vote in 1972? If not registered, why not? Did the respondent vote in 1972? At what time of day did the respondent vote?[2] If the respondent didn't vote, why not? Did the respondent vote in the 1968 election? Several of these questions require heroic recall, doubly so since fully 47 per cent of the responses obtained are based on proxy interviews in which one household member was asked to respond for other members of the family. All this, along with a few demographic items, leads to pages and pages of cross-tabulations containing such findings as 2.5 per cent (no sample size is reported for the percentage base) of the black males over age 75 who said (or somebody in their household said) they voted in 1972 reported they did so after 6 p.m. local time. It is measurement without theory, measurement made in complete isolation from professional research dealing with the very issues the Census Bureau is apparently seeking to study in this massive survey. This is a shame, both because of the waste of resources (imagine having seven questions on a really large, well-designed survey in November of an election year) and also because in other times and in other places the Census Bureau has set standards of statistical excellence. The idea behind the on-going series on voting and registration is probably a good one; the fault is in a lack of skill and professionalism in its execution.

A more informative exercise in the Dumping approach to statistical publications is the monumental (and expensive—$90) three-volume, 2388-page listing of responses to the Gallup poll questions from 1935 to 1971. The questions are presented in chronological order without reference to content—which accentuates the major weakness of the enterprise for political research: an overwhelming concern with

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York: Springer-Verlag, 1970), pp. 112–163; Stephen E. Feinberg and Leo Goodman, "Social Indicators, 1973: Statistical Considerations," in *Social Indicators, 1973: A Review Symposium,* ed. Roxann A. Van Dusen (Washington, D.C.: Social Science Research Council, 1974), pp. 63–82; and Oskar Morgenstern, *On the Accuracy of Economic Observations,* 2nd edition (Princeton: Princeton University Press, 1963).

[2] This question was newly added in 1972, apparently as a contribution to the dialogue between the networks and the administration with respect to the "question whether voting turnout in the three West Coast States (and even more so in Alaska and Hawaii) might be affected by the fact that the polls were closed in the populous Northeast and election returns and media projections were being disseminated throughout the Nation, while the polling places in these Western States were still open" (p. 7). No substantive findings on this issue are reported although there is a cryptic bar chart showing the turnout at 6 p.m. (local time) for different regions of the country. The previous literature on the effects of election forecasts and on the time of day people vote is not cited; for example, Harold Mendelsohn and Irving Crespi, *Polls, Television, and the New Politics* (Scranton, Pennsylvania: Chandler, 1970) and the many items cited there; also Douglas A. Fuchs and Jules Becker, "A Brief Report on the Time of Day When People Vote," *Public Opinion Quarterly,* 22 (Fall, 1968), 437–440.

terribly ephemeral matters. The political analysis in *The Gallup Poll,* presented in three chapters by Paul Perry that preface the collection, is concerned entirely with short-run forecasting. Missing from the poll are many themes that could have been dealt with usefully on a regular basis—ticket splitting, the value of the vice-presidential nominee to the national ticket, the structure of political belief systems, and repeated measures of political trust.[3] Nevertheless, the sheer magnitude and persistence of the poll make the volumes a useful resource for the study of American popular culture, history, and public opinion. And, if you like to pour through volumes of congressional hearings or exquisitely detailed compilations such as *The Baseball Encyclopedia,* then you will like these eleven pounds of old Gallup polls. Still, given all those questions tapping attitudes on non-issues ("Do you think American soldiers in Germany should be allowed to have dates with German girls?"—59 per cent no in 1945. "Do you think the schools should require boys to keep their hair cut short?"—80 per cent yes in 1965.) mixed in with all those questions tapping non-attitudes on serious issues ("If a serious war were to start in the Mideast in the next year or two, what should the United States do?"), even the most ambitious inductivist or zealous trivialist will find it necessary to consume the material in modest doses. Two samples of the good things to be found:

—The questions tapping political information reveal sublime gaps: 42 per cent of those interviewed in 1966 were unable to identify Robert McNamara; 57 per cent did not know the name of their representative in the House of Representatives.

—By some canons, President Roosevelt must have been unelectable, diverging greatly from a public that identified itself in 1939 as

"Radical"	2%
"Liberal"	46%
"Conservative"	52%.

The same sample then described Roosevelt as

"Radical	37%
"Liberal"	62%
"Conservative"	1%.

While *The Gallup Poll* demonstrates how a preoccupation with Fashion can weaken the collection and compilation of data, the well-known *Almanac of American Politics* by Michael Barone, Grant Ujifusa, and Douglas Matthews reveals Fashion's best side. The volume, a scorecard for congressional politics and elections, combines a good deal of statistical description of each congressional district along with well-written political biographies. With its gatherings from unacknowledged statistical sources coupled with a few errors in transcription, this almanac of congressional (not "American" as claimed) politics is useful as a background for research rather than as a trusted source of statistical materials. In short, the volume is a scorecard, but it is a classic scorecard.

The Republican National Committee has published a series of compilations of electoral and other political data. In the *1973 Republican Almanac,* the data are put together so that they might speak to the task of winning elections: congressional district voting records are ordered by vulnerability in upcoming elections, voting returns structured by the swing for or against the Republicans, and the state descriptions record media coverage, registration systems, and areas that might be targeted for special attention during a campaign. (*The Political Marketplace,* a massive volume, records in even greater detail information for running political campaigns: addresses of party officials and media outlets, lists of suppliers for campaign materials, and an annotated list of professional campaign managers.) These volumes make it clear how effective the theme of Action is for organizing large amounts of political data, helping the reader persist through piles and piles of numbers—especially the reader satiated after a few tables produced under the Dumping approach or disoriented and bored after a statistical parade of now-dead Fashion.

The strong Republican research operation has made an additional contribution to electoral statistics: the three volumes, *1970, 1972,* and *1974 Congressional Vote Statistics* contain detailed data for recent congressional elections. These 500-page compilations, prepared by

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[3] What continuity there has been in the Gallup surveys has yielded many useful inquiries, including the rapidly multiplying studies of whatever it is that determines presidential approval ratings over the years. A recent example is James A. Stimson, "Public Support for American Presidents: A Cyclical Model," *Public Opinion Quarterly,* 40 (Spring, 1976), 1-21. Although all of the current discussions of approval ratings begin with the Truman years, the Gallup data actually go back to 1935 and are available in Wesley C. Clark, *Economic Aspects of a President's Popularity* (Ph.D. dissertation, University of Pennsylvania, 1943). *The Gallup Poll* also reports some tabulations similar to Clark's as does the valuable compilation of Gallup electoral material in "Campaign '76," *The Gallup Opinion Index,* 125 (November-December, 1975). James R. Beniger also makes good use of the continuity in the Gallup polls in his "Winning the Presidential Nomination: National Polls and State Primary Elections, 1936–1972," *Public Opinion Quarterly,* 40 (Spring, 1976), 22–38.

Pierre M. Purves of the National Republican Congressional Committee, contain district maps, registration data, and, most important, a crude measure of the long-run partisan division in each congressional district. Calculated over the last four elections, the historical partisan division of the vote must be imputed from historically reconstructed districts formed by adding up, with map overlays, old precinct-level election returns for the boundaries of the current congressional district. (The process is similar to that used in computer programs that redraw congressional district lines within partisan or bipartisan constraints, where the past precinct returns are used to construct a variety of potential statewide districtings.) Despite these labors, the resulting numbers are only a crude measure of the normal vote in a congressional district because two very powerful influences on that vote—the incumbency effect across districts in a given election and the impact of national tides across elections in a given district—are not adjusted out of the data, either before or after aggregation. These numbers, then, probably serve the Action perspective fairly well—for they roughly indicate, to the current incumbent, the vulnerability of the district to national tides. The numbers are less useful for Understanding, for they lack comparability across districts or across elections. Assuming that incumbency effects operate uniformly over the entire district (which they may not, particularly if there are friends-and-neighbors effects within districts) and that they do not interact with the national tide in particular election years (as they may have in the 1974 midterm), then such influences might be adjusted out from the published numbers.

The unadjusted historical reconstructions of the congressional returns are also included in the Census Bureau's *Congressional District Data Book*, giving that volume a few election returns in what is otherwise a massive data dump of 303 demographic and economic characteristics of congressional districts.[4] The world of the *Congressional District Data Book* (which is updated by state supplements when a state redistricts) is a stark and barren one compared to those of the *Almanac of American Politics* and the *1973 Republican Almanac*, but it is a world that can be usefully explored by students of congressional elections and of congressional influences on program allocations. The *Data Book* is available on a computer tape.

Most of the numbers collected in these volumes are political ephemera, at least in the flimsy context in which they are usually found. In a year or two, the new edition of the volume will appear and the numbers now before us will have the magnetism of the 1967 Sears Roebuck catalog. Many of these numbers have lost their meaning and long-run interest because they are isolated from their political, historical, and geographic context—digits adrift in a sea of digits. Data are less alienated from themselves if organized for constant units of analysis over long periods of time. In the United States, this has meant data collected for counties or states—both statistically wretched units of analysis, since such comparisons are made across very heterogeneous units of wildly differing sizes. The counties are the worst, especially for unweighted cross-section computations because, unlike the states at least, few have any political life of their own congruent with their boundaries. Nevertheless, counties have persisted unchanged for a very long time. And the county is the only geographic unit with a match between demographic and voting data—that is, if county demographic data are merged with the voting data tapes.

Perhaps there is a great historical study in all this county material, a study which might begin with the *County and City Data Book, 1972.* This attractive volume, the best buy of all the books at hand (6½ pounds of data, more than 1.5 million separate figures, all for $18.65),[5] compiles 196 characteristics for the 3,100 counties, along with extensive data for cities, standard metropolitan statistical areas, urbanized areas, and unincorporated places. If combined with voting data, a lot of correlations could be ground out from the volume's data tape ($80 from the Bureau of the Census) or its ghastly equivalent of 107,000 punched cards ($1,100 plus shipping)—and, no telling, some fool with a computer just might try it. Most of the numbers in the *County and City Data Book* record detailed economic returns (for example, the 1967 receipts by county of hotels, motels, and camps), a reminder that the book originated within the Department of Commerce. The volume contains nothing of direct political relevance except for the percentage of the vote for president in 1968; reported only for the *leading* party's candidate in the county—not much help for the three-candidate race of 1968. A few more election returns would immensely enhance the value of the *County and City Data Book* for political analysis.

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[4] The *Congressional District Data Book* is supplemented by the Bureau of the Census *Congressional District Data Profile* (available in the form of a computer printout) for each district. The *Profile* provides many additional district variables.

[5] *The County and City Data Book* yields 800 figures per penny; the worst buy, numerically speaking, is *The Gallup Poll,* which, at $95 for the set, yields 8.5 figures per penny.

Detailed election returns by county for the entire century are available on data tapes from the Inter-University Consortium for Political Research (ICPR). My own experience with those tapes was that they were not completely cleaned and somewhat awkward to use—but that usable numbers were obtained with perhaps less trouble than from other equally large data tapes. For more recent elections, the *America Votes* series records, among other things, county-level returns in human-readable, although not machine-readable, form. Current volumes of *America Votes* are distinguished by especially good tabling and formating of the election returns. In addition, precinct-level returns are tabulated for some of the larger counties—thereby breaking the idiot county pattern of reporting data.

Much of the full ICPR data tape has been printed up in Congressional Quarterly's 1103-page *Guide to U.S. Elections,* a massive compilation of 150 years of election returns from congressional, presidential, and gubernatorial races. Particularly interesting are the cumulative vote totals for presidential primaries over the years. The *Guide* is a fundamental reference book for students of American politics, ranking with the *Almanac of American Politics* and the *Statistical Abstract.*[6]

State-level aggregations of federal election returns (both raw votes and percentages) are found in the fantastically detailed and careful compilation by Edward Franklin Cox, *State and National Voting in Federal Elections, 1910–1970.* Cox's volume is among the few that provide an adequate description of the definitions and conventions used in compiling the votes. Paul T. David's *Party Strength in the United States, 1872–1970* records statewide percentages of the vote in federal elections; it also contains a useful set of rules for handling the elaborate party coding on the ICPR data tape.[7]

Finally, let us turn to the highest level of aggregation, the compilation of measures for the United States as a whole over the years. Such materials are found in the 32 tables in the section on elections in the Census Bureau's *Statistical Abstract of the United States.* This section represents the best short numerical description of U.S. politics available. The tables have sufficient depth and vitality to be suggestive for experienced researchers and informative for beginning students. Many of the series are carried back several decades, although with each new yearly edition of the *Abstract,* the oldest figure in the series is deleted to make space for the most recent. Thus previous editions of the *Abstract,* along with *Historical Statistics of the United States, Colonial Times to 1957* and its supplement, are necessary to reconstruct the full data series. (A new *Historical Statistics,* compiling 12,500 statistical time series from colonial times to 1970, was published in May, 1976.) Since 1960, the elections section of the *Statistical Abstract* has expanded at the rate of about one additional table per year, introducing increasingly diverse data sources and moving well beyond reprinting aggregate election returns. Progress has slowed in the last four editions, however—although a good table on congressional incumbency was added in 1974. What is needed is an imaginative, fresh, professionally informed effort at improvement and expansion. Probably the biggest current gap is the lack of a table reflecting the long-run history of party affiliation among the electorate (regional and demographic breakdowns would be helpful here), material available from the archives of the Gallup poll and the University of Michigan electoral studies. Ticket-splitting indicators would be helpful. The *Abstract* now totals up the number of black elected officials over the years; similar computations for other groups might be added. A series recording the proportion of citizens able to identify their representatives would add another dimension.[8] Although there is substantial room for improvement, the small section on elections in the *Statistical Abstract* best exemplifies statistical publication for Understanding in the volumes at hand.[9] That section should be appended to every textbook of American politics.

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[6] A helpful bibliography is John H. Runyon, Jennefer Verdini, and Sally S. Runyon, *Source Book of American Presidential Campaign and Election Statistics, 1948–1968* (New York: Frederick Ungar, 1971). Also Edward R. Tufte, *Data Analysis for Politics and Policy* (Englewood Cliffs, New Jersey: Prentice-Hall, 1974), pp. 164–170.

[7] The book is now supplemented by Paul T. David, "Party Strength in the United States: Changes in 1972," *Journal of Politics,* 36 (August, 1974), 785–796; and Paul T. David, "Party Strength in the United States: Some Corrections," *Journal of Politics,* 37 (May, 1975), 641–642. David's volume has been more widely reviewed than most electoral compilations; see the lengthy discussions by Walter Dean Burnham, *American Political Science Review,* 67 (March, 1973), 218–220; Milton G. Cummings, Jr., "Elections in the United States: Some Interesting Findings," *Virginia Quarterly Review,* 48 (Autumn, 1972), 590–594; and John L. McCarthy, *American Historical Review,* 78 (June, 1973), 736–737. A fine guide to state electoral data is Burnham's manuscript, "Sources of American Election Data."

[8] These data can be pieced together from *The Gallup Poll,* and University of Michigan elections studies, and from a remarkable 858-page data dump published by the Subcommittee on Intergovernmental Relations, Senate Committee on Government Operations, *Confidence and Concern: Citizens View American Government—A Survey of Public Attitudes* (Washington, D.C.: U.S. Government Printing Office, 1973).

Error Structure

Not a great deal is known about the error structure of most political indicators; and, consequently, many have made magisterial pronouncements on how data errors must affect the use of analytical techniques and the credibility of inferences. Probably the most frequent incorrect generalization concerning errors in data is that "badly measured and inadequate data render useless fancy statistical techniques." This has got it backwards, as Mosteller and Bush, along with Wallis, point out:

> In no circumstances do we think that sophisticated analytical devices should replace clean design and careful execution, unless very unusual economic considerations arise. However, it may be worth remarking that crude data collected as best the investigator could may require the most advanced statistical tools. Here a quotation from Wallis may be appropriate:
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> > "In general, if a statistical investigation . . . is well planned and the data properly collected the interpretation will pretty well take care of itself. So-called 'high-powered,' 'refined,' or 'elaborate' statistical techniques are generally called for when the data are crude and inadequate—exactly the opposite, if I may be permitted an *obiter dictum,* of what crude and inadequate statisticians usually think."[10]

Sometimes errors in measurement are viewed by the critics of quantitative work as compromising the validity of many, and perhaps most, quantitative studies of political life. This is, in fact, an extraordinarily difficult criticism to answer. The truth is that because the error structure of most political indicators is not known, we are simply unable to make informed statements about the impact of errors of measurement and processing on the credibility of inference. While some of the several million numbers in all these data books at hand do have appended to them a formal statistical estimate of sampling error, virtually none of the numbers come with an honest estimate of error.

Probably the political indicator with the best understood error structure—mainly because something is clearly wrong with the numbers that do turn up—is the proportion of respondents in national surveys who report that they have voted in the last election. In most surveys, including the biennial voting studies in the Current Population Survey and the University of Michigan election studies, the self-reported turnout runs 7 to 15 per cent higher than the aggregate turnout computed by totaling up all the votes and dividing by the voting age population. The overestimate of turnout in the survey reports can be traced, as Clausen has carefully done, to errors both in the numerator and the denominator of the turnout estimate.[11] The numerator is too large because respondents (or their proxies) report having voted when they actually did not; the denominator is, in effect, too small because survey sampling designs tend to exclude people not directly attached to a household—and these are people less likely to register and vote. Given the multiple sources of error, some of which can be linked to particular population subgroups, the discussion found in the CPS reports of 1970 and 1972 on voting and registration seems very optimistic:

> Estimates of voter participation that are higher than the counts have been the common experience of other survey organizations which have studied voting behavior. Explanations similar to those cited in this report have been given by these groups to account for the discrepancies. However, in spite of these limitations, the differences in levels of voting and registration behavior among various subgroups in the popula-

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[9] One additional item of political interest in the *Statistical Abstract* must be mentioned. Page 835 of the 1973 *Abstract* recorded a testament to the statistical wisdom of Richard Nixon for his creation of ten standard federal administrative regions for which, among other things, statistics can be compiled. He was the first president ever to be eulogized in the long history of the *Abstract,* published in 1973 by the Bureau of the Census, in the Social and Economic Statistics Administration, of the Department of Commerce. (Since the new regions were states, the new statistical compilations consisted entirely of adding up the numbers for the four or five states contained in each region.) The eulogy, reprinted in the 1974 *Abstract* (p. 847), was altered in the 1975 *Abstract* (p. 867) by purging Mr. Nixon's name entirely and putting the passage in the passive voice.

[10] Frederick Mosteller and Robert R. Bush, "Selected Quantitative Techniques," in *Handbook of Social Psychology: Volume I, Theory and Method,* ed. Gardner Lindzey (Cambridge, Massachusetts: Addison-Wesley, 1954), p. 331. The internal quotation is from W. Allen Wallis, "Statistics of the Kinsey Report," *Journal of the American Statistical Association,* 44 (December, 1949), 471.

[11] Aage R. Clausen, "Response Validity: Vote Report," *Public Opinion Quarterly,* 32 (Winter, 1968–1969), 588–606. An interesting variation on Clausen's theme is David Adamany and Philip DuBois, "The 'Forgetful' Voter and an Underreported Vote," *Public Opinion Quarterly,* 39 (Summer, 1975), 227–231. Additional research and citations are in E. M. Schreiber, "Dirty Data in Britain and the USA: The Reliability of 'Invariant' Characteristics Reported in Surveys," *Public Opinion Quarterly,* 39 (Winter, 1975–1976), 493–506.

> tion presented here are believed to be genuine differences.[12]

So, even though it is known that the subgroup estimates are wrong, let us believe that they are wrong by the same amount. That is a belief supported by faith, not evidence. It is common hope, held by those whose measures have clear biases in their marginal distributions, but who wish to save the day by claiming that the joint relationships are unbiased. But it is a claim that demands empirical support, especially because it is so crucial to the validity of the analysis. Or consider a similar argument, which also requires empirical support, applied to the sampling problems of the Gallup polls of the 1930s and 1940s:

> . . . the underrepresentation of blacks, females, southerners, and low-education persons in the early Gallup samples is relatively unimportant if the responses to a question were not highly correlated with race, sex, region or education; and, contrary to widespread belief among social scientists, none of these demographic variables typically explains much of the variance in responses to attitudinal questions.[13]

Just a few studies have examined errors in the count of the vote. For the 1968 and 1970 elections in Texas, Tarrance computed the differences between the precinct totals added up by the Secretary of State with the precinct totals added up by computer. The mistabulations occurring in this single step—of aggregation from the precinct to the state—lead to the conclusion that "on the basis of the mistabulations disclosed here, any future candidate who lost a close statewide election (by fewer than five thousand votes) in Texas most assuredly would want to file for a recount of the vote to determine the rightful winner."[14] None of the volumes under review provide similar statements about margins of error in their vote counts.

If the data published in these volumes are used in research studies, the numbers require processing—transcription into machine-readable form if necessary, analysis of data tapes, tabulating the results, and the like.[15] All these steps introduce error: errors in computer programming, in the definitions of variables, and in arithmetic. A good many examples of processing errors arise in the various computations done on the data tapes from the Inter-University Consortium for Political Research. Consider these two sets of computations (one presented in a convention paper, the other done for classroom use) executed by experienced researchers running identical data tapes (one at the University of California, Berkeley, computer center and the other at Princeton). In their studies, they both computed the same simple number for eight elections: the percentage of voters in each election who reported that they voted consistent with their party identification. I have compiled the two sets of results in Table 1—and at least they agree in half of the elections. While the errors introduced here are fairly small—an average of two percentage points when they disagree, the results emphasize the importance of the independent replication not only of key findings but even of simple calculations.[16] And let us imagine—after seeing the errors arising in a single step in a well-controlled research context—the magnitude of errors arising in the hasty, unreplicated, decentralized, and sometimes highly partisan count of the vote.[17]

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[12] The paragraph is found both in U.S. Bureau of the Census, *Current Population Reports: Voting and Registration in the Election of November 1970*, Series P-20, No. 228 (Washington, D.C.: U.S. Government Printing Office, 1971), p. 6; and *Voting and Registration in the Election of November 1972*, p. 8.

[13] Norval D. Glenn, "Trend Studies with Survey Data: Opportunities and Pitfalls," *Social Indicators Newsletter*, 5 (October, 1974), 4–5. A useful statement concerning the details of error structure in large-scale surveys is found in Philip E. Converse, "The Availability and Quality of Sample Survey Data in Archives within the United States," in Richard L. Merritt and Stein Rokkan, eds., *Comparing Nations: The Use of Quantitative Data in Cross-National Research* (New Haven: Yale University Press, 1966), pp. 419–440. See also Leslie Kish, *Survey Sampling* (New York: Wiley, 1965), chap. 13; Norval D. Glenn, "Trend Studies with Available Survey Data: Opportunities and Pitfalls," in Social Science Research Council, *Data for Trend Analysis* (Williamstown, Massachusetts: Roper Public Opinion Research Center, 1975), pp. 6–35; and Herbert B. Asher, "Some Consequences of Measurement Error in Survey Data," *American Journal of Political Science*, 18 (May, 1974), 469–485.

[14] V. Lance Tarrance, Jr., *Texas Precinct Votes '68* (Dallas: Southern Methodist University Press, 1970), p. 181. See also his *Texas Precinct Votes '70* (Austin: Foundation for the Study of Democratic Processes, 1972), pp. xi–xii, 185–188.

[15] Useful considerations are found in David C. Hoaglin and David F. Andrews, "The Reporting of Computation-Based Results in Statistics," *The American Statistician*, 29 (August, 1975), 122–126. A subtle computational problem has risen with respect to random number generators; see Robert Lynn Coldwell, "Correlational Defects in the Standard IBM 360 Random Number Generator and the Classical Ideal Gas Correlation Function," *Journal of Computational Physics*, 14 (February, 1974), 223–226.

[16] Would political research fare better in replication than the studies described in this report published in 1962? "Last spring a graduate student at Iowa State University required data of a particular kind in order to carry out a study for his master's thesis. In order to obtain these data he wrote to 37 authors whose journal articles appeared . . . between 1959 and 1961. Of these authors, 32 replied. Twenty-one of these reported the data misplaced, lost, or inadvertently destroyed. Two of the remaining 11 offered their data on the conditions that they be notified of our intended use of their data, and stated that they have control of anything that we would publish involving these data. We met the former condition but refused the latter for those two authors since we felt the raw

Table 1. Percentage of Voters Voting Consistent with Their Party Affiliation in Congressional Elections: Two Independent Computations from the Consortium Data Tapes

Year	Political Scientist A	Political Scientist B
1956	82%	82%
1958	84%	84%
1960	80%	80%
1962	81%	83%
1964	79%	79%
1966	74%	76%
1968	73%	74%
1970	73%	76%

Some conclusions about error structure:

(1) Substantial errors occur in the collection and analysis of data.[18] My own rough estimate, based on what literature there is and from a file of data errors, is that such errors affect the substantive conclusions of at least one-fourth of current data-based research in political science.

(2) In most cases, knowledge about error structure comes from empirical investigation. Few general morals about the direction and magnitude of errors will carry us through. A mathematical analysis of the effects of measurement errors on statistical estimation concludes that the usual hopes about errors and biases require evidence; Cochran writes:

> In conclusion, this review indicates that errors of measurement can sometimes seriously vitiate most standard statistical techniques and at other times have only trivial effects—it depends on the sizes of the relevant variances and covariances. Though further model-building and checking is important, what seems most needed in the present state of development of this area are many studies that permit estimation of these variances and covariances.[19]

(3) Next to the independent replication of the collection and analysis of the data, graphical displays of the data are probably the best method for detecting errors and inconsistencies in the data.

(4) A frequent mistake is to report more significant digits in the published output than exist in the original data—described by Morgenstern as "specious accuracy." The study of politics, like the study of economics, is usually a one-digit science at best; in fact, we do well to get the sign right more than half the time. How then can anyone be asked to take the third, fourth, and fifth significant digits seriously? Why do professional journals publish that computational debris?

Empirical studies of the error structure of political indicators are difficult, time-consuming, and probably professionally underappreciated. Yet the few such studies that have been done are clearly valuable.[20] Perhaps as quan-

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data from published research should be made public upon request when possible and economically feasible. Thus raw data from 9 authors were obtained. From these 9 authors, 11 analyses were obtained, four of these were not analyzed by us since they were made available several months after our request. Of the remaining 7 studies, 3 involved gross errors. One involved an analysis of variance on transformed data where the transformation was clearly inappropriate. Another analysis contained a gross computational error so that several F ratios near one were reported to be highly significant. The third analysis incorrectly reported insignificant results due to the use of an inappropriate error term. . . ." Leroy Wolins, "Responsibility for Raw Data," *American Psychologist*, 17 (1962), p. 657.

[17] See James Farmer, Colby Springer, and Michael J. Strumwasser, "Cheating the Vote-Count Systems," *Datamation*, May, 1970, pp. 76–80; and Robert L. Patrick and Aubrey Dahl, "Voting Systems," *Datamation*, May 1970, pp. 81–82. The currently definitive document is Roy G. Saltman, *Effective Use of Computing Technology in Vote-Tallying* (Washington, D.C.: National Bureau of Standards, March, 1975), prepared for the Clearinghouse on Election Administration, Office of Federal Elections.

[18] Persistent problems with data errors and failures to replicate have cropped up even in the technically sophisticated examinations of the correlations between electoral and economic time series; see the papers by Goodman and Kramer and by Arcelus and Meltzer in the December, 1975 *American Political Science Review*. For a study of the correlation between time series chosen at random from *Historical Statistics of the United States*, see Edward Ames and Stanley Reiter, "Distributions of Correlation Coefficients in Economic Time Series," *Journal of the American Statistical Association*, 56 (September, 1961), 637–656.

[19] W. G. Cochran, "Errors of Measurement in Statistics," *Technometrics*, 10 (November 1968), 665.

[20] See Clausen, "Response Validity: Vote Report," and Frederick Mosteller et al., *The Pre-Election Polls of 1948* (New York: Social Science Research Council, 1949), chap. 5. Other worthwhile reports on error are Morgenstern, *On the Accuracy of Economic Observations*; William H. Kruskal and Lester G. Telser, "Food Prices and the Bureau of Labor Statistics," *The Journal of Business*, 33 (July, 1960), 258–279; a thorough study of the population undercount in the 1970 census and its consequences is Jacob S. Siegel, "Coverage of the Population in the 1970 Census and Come Implications for Public Programs," U.S. Bureau of the Census, *Current Population Reports, Series P-23* (Washington, D.C.: U.S. Government Printing Office, 1975); for data problems in economic policy making, Edward F. Denison, *Accounting for United States Economic Growth 1929–1969* (Washington, D.C.: The Brookings Institution, 1974), pp. 84–101. Also of interest is U.S. Bureau of the Census, "Standards for Discussion and Presentation of Errors in Data,"

titative inquiries into political life become more sophisticated, more effort will be applied to the task of producing honest estimates of error associated with key political measures. And, if previous work is a good guide, such studies will probably tell us as much about politics as about statistics.

Conclusions and Recommendations

(1) A basic minimum personal library of data sources for American politics should consist of *The Almanac of American Politics* and the *Statistical Abstract.* The deluxe minimum library would also feature the *Guide to U.S. Elections.*

(2) None of the remaining volumes at hand are strong candidates for inclusion in a minimum personal library; the choices after the *Almanac* and the *Abstract* depend on research and teaching interests. Worth contemplating are the expensive volumes in the *America Votes* series and in *The Gallup Poll.* Both these sets belong in paperback. The *Congressional Directory* is often quite useful, as a guide to Congress and to the rest of the government.

(3) All these detailed collections of aggregate data make clear that the biggest gap in American political statistics is the lack of an adequate statistical summary of the election surveys of the University of Michigan. The availability of the ICPR computer tapes does not fill the needs of teaching and research; and there is no reason why so many of the basic descriptive statistics of American electoral behavior have to be generated afresh (sometimes, as we have seen, with error) even by those with access to the tapes.[21] Flanigan and Zingale's *Political Behavior of the American Electorate* is a start in the right direction, but far more is needed.[22]

(4) Except for Cox's *State and National Voting in Federal Elections, 1910–1970,* none of the volumes provides adequate supporting material for their statistics: definitions of measures are sketchy, data-editing choices not explained, and theoretical motivations absent.

(5) With the partial exception of the Census Bureau's *Voting and Registration in the Election of November 1972,* none of the volumes contains a worthwhile discussion of the error structure of their numbers or an honest estimate of the magnitude of the errors. Nonsampling errors are almost completely ignored.

(6) The historical development of the volumes that are part of a series is almost always in the direction of expansion, and not in the direction of improving old measures. There is often *more,* but rarely *better.*

(7) Although there are many well-established series of data books, the field of publication of American political statistics is one open to innovation. New developments can be well rewarded, as the success of *The Almanac of American Politics* indicates.

(8) Three of the usual mechanisms generating incentives for quality work in scientific publication—personal responsibility for a work, informal peer review, and professional public review of the final product—operate weakly with respect to the 13 volumes at hand. Half have no clear or credible attribution of authorship: title pages bear the name of the originating bureaucratic divisions or the names of those happening to occupy slots in the hierarchy above the originating division at the time of publication. Thus the name of the Secretary of Commerce graces the title pages of many of these publications. The Census Bureau, to its credit, has also put the names of some of those who actually did the work on the back of the title page. Several other volumes show only corporate authorship. Kruskal has described the problem of the proper attribution of authorship in statistical publications:

> The underplaying of personal authorship in government publications has not, to my knowledge, been studied systematically.... One might conjecture that the stronger the professional staff of a government agency, the more frequent is clear ascription of personal authorship, although I expect that causation runs in both directions at once. One might also expect that pressure for personal authorship is a force towards recruitment and retention of better civil servants, and surely personal authorship fosters increased personal responsibility.[23]

In addition, many of the data compilations are isolated from the research community; the numbers are collected apart from the needs of the users of the data and apart from the

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Technical Paper No. 32 (Washington, D.C.: U.S. Government Printing Office, 1974). Good general discussions of error are Churchill Eisenhart, "Expression of the Uncertainties of Final Results," *Science,* 160 (June 14, 1968), 1201–1204; and W. J. Youden, "Enduring Values," *Technometrics,* 14 (February, 1972), 1–11.

[21] Such a project is now moving forward: Warren E. Miller and Arthur H. Miller are preparing *American Political Trends: A Sourcebook of Political Indicators, 1952–1974.*

[22] William H. Flanigan and Nancy H. Zingale, *Political Behavior of the American Electorate,* 3rd edition (Boston: Allyn and Bacon, 1975).

[23] William Kruskal, "Science Indicators: Themes and Variations," in *Toward a Metric of Science: Reflections on Science Indicators,* ed. Yehuda Elkana, Joshua Lederberg, Robert K. Merton, Arnold Thackray, and Harriet Zuckerman (New York: Wiley, 1977).

technical literature clearly relevant to the data. Many of the citations in these volumes are to internal and often unpublished documents. Finally, the isolation is too often mutual: despite the hundreds of studies pulling numbers out of the *Statistical Abstract,* the *County and City Data Book,* the *Congressional District Data Book,* and the like, virtually no reviews—other than routine summary descriptions—of the volumes at hand are to be found in the statistical or social science literature. And so some of the deficiencies in these volumes have not been remedied because there is little professional curiosity about where all these numbers that we rely on might come from, what their error structure might be, and how they might be improved.

Everything counts, but nothing matters.

BOOK REVIEWS

Process and Outcome of Negotiations. By O. J. Bartos. (New York: Columbia University Press, 1974. Pp. 451. $15.00.)

The general aim of this effort is to move toward a more complex theory of negotiations: one that aims at prediction and prescription, that deals with the process and the outcome, that assumes that negotiators are both past- and future-oriented. This type of model could serve as a bridge between the static, game-theory approaches to negotiation and the more dynamic process theories in social science. Falling short of this goal, the volume does accomplish a very important first step, viz., an appreciation for what is necessary to reach this level of maturity. In approaching his task Professor Bartos proceeds in steps, taking first simple models of the process, then simple models of the outcome, and then moving to a consideration of how the theory of negotiation can be advanced beyond the simple model stage. In so doing, he has collected empirical data to provide estimates of model parameters and tests of competing hypotheses suggested by alternative models.

Some 700 experiments conducted over a ten-year period provide an enormous data base for testing the implications and hypotheses derived from the models. The models provide an organizing framework for interpreting the data. At issue is the *match* between models and data. The models chosen for investigation were originally constructed to characterize other processes (e.g., arms races, learning); the bargaining game used for data collection represents one particular *construal* of negotiation. But both converge on the same referent. By making a number of assumptions, the models are adapted to bargaining; by incorporating features of the "real world" referent, the experiments reach out to a broader conceptualization of negotiation. Bartos spells out all of the restrictions he places on the data; he makes explicit the dimensions of difference between his construction and the referent. The match seems reasonable. He proceeds to compare the models in terms of predictive accuracy. More conjectural, of course, is the relevance of the match for other models and for other "construals" of negotiation.

In describing his progress, the author offers a clear and forthright presentation style. He anticipates reader suspicions, presents arguments for and against the use of a technique, and seeks out alternatives when interpretations appear to suffer from a questionable match between tool and data. He thinks out loud, letting the reader in on his reservations or advocacy of a particular analytic strategy. In so doing, however, he runs the risk of making apparent certain shortcomings such as using the same data set to provide parameter estimates and to test predictions derived from the models and, on occasion, *overinterpreting* low correlations (e.g., see pp. 152–153; also, such correlations as .16 between perceived toughness and actual toughness are interpreted as evidence for the suggestion that "our subjects did in fact do what they professed to have done"—p. 164). On the other hand, some issues are not broached, leaving the reader to conjecture about what was done. For example, it is not clear in what manner the data were aggregated for the various analyses: combining data from disparate experiments necessitates a demonstration of no significant difference between these data sets on the dimensions of interest; such demonstrations are not provided. More basically, the concept "experiment" is not delineated sufficiently to know whether Bartos is referring to replicated runs or to just a single replication: the number of replications per experiment is not provided. For these reasons, we are left to wonder just what "chunk" of data was used in any particular analysis.

Bartos is at his best when he searches for a theory to explain the nonlinear relationship between toughness and payoffs. Systematically and painstakingly, he proceeds toward a clarification of this relationship. His strategy uncovers a rather subtle "law" which he refers to as the "principle of diminishing returns:" toughness has different implications depending upon its intensity and the extent of time pressures. One implication of this principle is that toughness leads to high payoffs when an agreement is reached but tends to decrease the probability of agreement. Another implication is that the larger the minimum payoff and the less free the negotiator is to vary his toughness, the less profitable it is to be tough. In arriving at these conclusions, the author estimates more parameters, examines more levels of variables, and spells out in greater detail the various assumptions that place restrictions on the interpretation of the data than I have seen done to date. Moreover, conclusions that appear to be intuitively "obvious" (e.g., toughness is more profitable when it generates softness in an opponent) are shown to be only one of several possibilities that could emerge.

While he is very skillful in elaborating the *consequences* of toughness, he has more trouble ascertaining the *causes* of toughness. The strategic question is, How do we know whether an opponent will be tough? Are there cues that can be used as reliable indices of toughness? This issue is explored by considering the effects of background factors on the process. Strong relationships obtained between these variables should be instructive. The evidence, however, does not permit us to make such "inferential leaps." Although a number of relationships are found, these are weak, accounting overall for only 5 per cent of the variance in process variables. But all is not lost! The more general implication of these findings leads to, and bolsters, a conclusion arrived at earlier: a negotiator depends more on his strategic analysis of the situation than on some inherent tendency to be soft or tough.

In concluding, the author returns to the question asked initially: How far have we come toward an adequate model of negotiations? How does this effort stack up? He admits that he only dealt with the simplest models of the process and by so doing avoided confronting some of the issues. But, perhaps he is being too modest. Clearly his work has sensitized us to the necessary ingredients for a faultless model. Such a model would take account of beliefs about the future, would predict the Nash outcome, would take expectations into account, and would permit the derivation of Bartos's concept of toughness. The only model proposed to date that comes close is J. G. Cross's *The Economics of Bargaining* (New York: Basic Books, 1969). Although this too falls short of the ideal (e.g., some of the parameters are difficult to estimate from empirical data), it could, with certain modifications, be the best candidate. Unfortunately, Bartos considered this model only as an afterthought.

Finally, by juxtaposing formal models and experimental data within a more general framework, Bartos has given us the most elegant treatment of the subject to date. His style of presentation allows the reader to appreciate the intricacies of model development and evaluation without being overwhelmed by minute details. Many of these details are reserved for appendices where complex derivations and technical procedures are elaborated. I look forward to the further work outlined in his final chapter.

DANIEL DRUCKMAN

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The Organization of Academic Work. By Peter M. Blau. (New York: John Wiley and Sons, 1973. Pp. 310. $12.95.)

In this book, Blau continues the research he has carried out in unemployment offices, other governmental bureaus, and other organizations. Here he studies a representative sample (N = 115) of four-year educational institutions granting liberal arts degrees in the United States in the mid-1960s. He calls such a study "comparative," the reference being to the comparison of such organizations with each other, rather than among countries or cultures. The data were gathered from (1) interviews with the president or his representative plus the vice-president or person in a similar position, and by self-administered questionnaires filled out by deans; (2) standard reference works (e.g., *American Universities and Colleges,* for data on size, SAT scores, etc.) plus other special studies (e.g., Astin's measure of institutional reputations among outstanding students); and (3) a survey of 2577 faculty members at these 115 institutions, conducted by the University of Michigan Survey Research Center.

Perhaps the major distinguishing feature of the study lies in the rigorous method used to analyze the data—namely, path analysis, a method Professor Blau also used in his previous works, reflecting his earlier association with Otis D. Duncan. There is no path diagram in the book because Blau is dealing with so many variables that a path diagram would be confusing. Instead he lists all 57 variables in a presumed time sequence and then takes them up serially. Hence, he does not test causal hypotheses but rather uses path analysis to analyze spuriousness and intermediate effects. If a variable is explainable in terms of a variable already treated, it is spurious; if intervening, it is intermediate. This procedure enables Blau to handle some enormously complex relationships with impressive results. For example, the Pearsonian correlation between size and faculty contacts with undergraduates is found to be −.50, which sounds obvious (the bigger the institution, the less time faculty spend with undergraduates). But it turns out that the beta weight is practically zero, meaning that size as such has practically nothing to do with it. Instead variables *correlated* with size, namely: number of specialized departments, degree of emphasis on teaching and number of graduate students, account for just about all of the correlation—and that is *far* from obvious. Such findings are presented compactly in the form of a small number of data-packed tables.

The book cannot be skimmed, nor can one get by with a reading of chapter summaries or the concluding chapter. Blau is forever drawing new implications or comparing a finding to another in the literature, often with surprising consequences. Examples include the following: large universities turn out to be less bureaucratic, in most respects, than do small ones; leading institutions are no more innovative in pioneering new fields than are lesser ones; using publications as a basis for promotions does not motivate persons to engage in research, but such a practice does indirectly intensify research activities through its effect on selection and through producing a research climate; of all variables examined, size turns out to be strongly related to more other variables than any other variable. Blau's final chapter represents a superb summary in which *every* relationship mentioned is related to a research finding.

Universities and colleges are found to be structurally homologous to government bureaucracies in major ways. Blau focuses mainly on the relationships among size, differentiation, and proportion of administrators. He confirms findings in his previous studies that these work in different ways to control one another (larger institutions have proportionately fewer administrators, but larger institutions also are more differentiated, which creates a need for more administrators. The first effect is stronger than the second, with the net results that proportion of administrators shows an economy-of-scale effect, but at a decreasing rate). The author makes important theoretical sense of these findings, for example, concluding (from his data rather than speculation) that size results in differentiation in educational institutions though *not* because of reduction of labor costs through routinization nor because such differentiation generates interdependence and hence integrates the institution.

The title of the book (if "in the U.S.A." were added) describes well the book's contents. The *institutions* are treated as the units and the variables are, for the most part, formal or official (size, age, affluence, enrollment, number of levels, selectivity, percentage of faculty in the senate, administrative influence on appointments, faculty research productivity, for example). As such, the book represents well where the pendulum has now swung historically: it has gone from Weberian concern with formal structure, to human relations and psychological concern with interpersonal relations and informal structure—and now has swung back to formal again. It also typifies the current research focus on the study of complex organizations. The book definitively treats the formal structure of American academic institutions. It is hard to imagine anyone—for some time—using any other method of data analysis, though, of course, others may well utilize new variables (especially informal ones). Nor can one imagine a responsible academic policy proposal being made without first consulting this book (e.g., a proposal was made in the AAUP Bulletin that small colleges should elect senates rather than, as is common, meeting as a single body, since the latter practice tends to result in domination by administrators. Blau's data show just the reverse). The book is essential reading for students of organizations and bureaucracies, and for all of us who make our living working in universities or colleges.

EDWARD GROSS

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Direct Action and Liberal Democracy. By April Carter. (New York: Harper & Row, 1973. Pp. 169. $9.00.)

The notion of "direct action" within a democratic state raises at least three obvious questions:

(1) Is there room within a democracy for nonpolitical forms of action which look not to government but to intermediate associations (e.g., labor unions) to win demands from nonpolitical powers (e.g., employers), or for direct appeals to government through legally sanctioned private displays of power (e.g., lobbying, demonstrations, strikes, etc.)?

Apart from borderline cases—where demonstrations entail or provoke, or seem to provoke, violence—and apart from the historical fact that conceptions of legality were not always what they are today—relative to strikes and boycotts—this question is affirmatively answered on its face and is therefore uninteresting. April Carter's book deals extensively, and on the whole sanely, with this matter.

(2) If the rules of the game are reasonably just (i.e., democratic), but one can't win under the rules, is he warranted in defying or subverting the rules and engaging in unlawful forms of direct action—civil disobedience, trespass, guerrilla warfare, etc.? Should we respect those who play the game only if they win, who otherwise will not abide by the judgment of the majority or work within the rules to alter that judgment?

This question poses moral and strategic rather than legal issues. Hence for the strict constitutionalist it is a meaningless question; the answer is clearly negative and requires no response other than effective enforcement of the law. But for philosophers concerned with moral justification, or for political strategists and tacticians seeking effective techniques for the attainment of their goals, this question cannot be set aside that readily.

At the philosophical level, little need be said of April Carter's treatment. It is slight and superficial and does not come seriously to terms with the many difficulties and complexities of the issues. At the strategic or tactical level, she recites the relative advantages and disadvantages of direct action in a fashion suitable to a debater's handbook. Certain necessary distinctions are indeed drawn: between diverse forms of direct action—massive sit-ins, civil disobedience, nonviolent resistance, and guerrilla tactics (ranging from the disruption of meetings to the systematic use of armed violence); between different contexts in which such direct action takes place—urban ghettoes, universities, military draft centers, Trafalgar Square; and between stated goals of the groups resorting to direct action—to achieve basic constitutional rights or to gain new ones, to win limited and specific reforms, to express a symbolic act of protest, to overthrow the regime. To enter such distinctions is to recognize that everything depends on the specific combination found in each situation. But Carter's book offers no searching inquiry into, or comparison between, the many varieties of such specific combinations.

(3) If the rules of the game have been corrupted or subverted, so that the so-called liberal or democratic state does not in fact embody the principles—of freedom, rationality, and tolerance—it avows, is direct action a proper recourse for those committed to the restoration of liberal values and democracy?

Here carefully evaluated empirical evidence and rigorous philosophical analysis must work together to produce a sensible answer. Neither is to be found in the two final chapters of Carter's book, which deal with this question. She paints a picture of repression in America that will be recognizable only to the doyens of the New Left, who—like Tom Hayden and Herbert Marcuse—are by and large the authorities she invokes. In terms of *that* portrait she argues that direct action is not a threat to liberal values but a means to their restoration, and that violence has as its purpose liberation! She accepts much, almost all, of Marcuse's idea of repressive tolerance; attacks compromise and gradual reforms as "necessarily" favoring those who have most power and privilege in society; and justifies—though with qualification—direct action "as an intrinsically valuable mode of expressing independence, practising resistance and exercising popular sovereignty" (p. 159).

Two final comments are perhaps in order. First, though Carter recognizes that direct action is not a technique peculiar to the Left, she devotes only two pages to the Right. Had she indulged less in her sympathies with the Left and dealt more extensively with the Right, I suspect she would have been far more critical of the uses and consequences of direct action. Second, while direct action is a response to a sense of injustice and a conviction that constitutional means are ineffectual in redressing that injustice, it does not follow that those who make these judgments are correct or that the consequences will be what they imagine. Carter is not unaware of this but fails, nonetheless, to provide criteria by which one can determine the rightness of such judgments and the effectiveness of direct action.

DAVID SPITZ

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The Politics of Policy in Local Government. By John Dearlove. (New York: Cambridge University Press, 1973. Pp. 287. $12.50.)

"The bulk of governmental activity," John Dearlove reminds us in his introduction, "may well involve the maintenance and implementation of established policies where there is little or no 'policy-making' as such," and he suggests that, if we are interested in "public policy," we must investigate not only the forces that encourage and lead to policy decisions but also those factors that inhibit change and allow present commitments to stay unchallenged. The author has chosen as the empirical field for his own study a single local authority in Britain, the Royal Borough of Kensington and Chelsea, a London borough with a population of some quarter of a million and, in 1973, a rateable value of £25 million. This, by itself, might seem stony ground, but Dr. Dearlove has so thoroughly mined the rich seams of existing literature on British local government, public policy making, role theory, interest groups, ideology, and information theory that he is well armored with techniques, ideas, and critical tools for the extraction of new insights.

Part One of his book reviews particular theories—work on the importance of elections and the electorate, on pressure groups, and on the socioeconomic environment of governments—which have been offered to explain why government activity and public policy have taken this form or that. Orthodox theory, he concludes, implies that government is weak and that those who govern are open and responsive to "environmental" influences. Too little attention has been paid to the structures of government and to factors internal to the governing body or to the ideological predispositions of its members which restrict their vision, confine the scope of their activity, control disturbance, and squash change.

The author's own research, presented in Part Two, challenges the literature which he inspects so meticulously (and orders usefully with lucid footnotes and a sensibly arranged bibliography). The Borough Council of Kenginston and Chelsea is strong (with an entrenched Conservative majority), closed, and unresponsive; the majority of councillors are selected, schooled, and promoted within a system of procedures and an environment of rarely questioned conventions that minimize change and stifle innovation; and crises seldom occur. The smooth rhythm of routine is only jarred, in fact, by the occasional irruption of new interest groups, the regular election of new councillors who might threaten established methods, and, as actually happened in 1968, the fortuitous election of a new Leader who has proved to be particularly sympathetic to fresh ideas. Even these groups and individuals, however, soon assume collaborative relations with the organization and, as they become part of it, adopt the overwhelmingly passive, "policy-maintaining" attitudes that prevail.

The author makes a brave case to support his critique of previous theoretical writing and what he shows us is that in a local authority we find the same features of hierarchy, specialization, rule-constrained behavior, government by precedent, resistance to outside pressures and sources of complaint, and timidity toward change that characterize other bureaucratic systems of social organization. It is therefore the more sad that, as the author himself admits, he includes little discussion of the part that Borough officials play in making and maintaining council policy. We are told that it is largely to their professional officers that councillors look for information and advice, but it would be helpful to have, for example, some exploration of the procedures and conventions that the permanent officials observe and foster, consciously or not.

Nor does the author make more than passing reference to policy issues, substantive decisions and detailed programs pursued or enacted by the councillors. This is a deliberate omission, for it is with the forces which inhibit change, rather than with case studies of action and disturbance, that Dr. Dearlove is concerned. Nevertheless, we do feel a need for some description of what the councillors are actually *doing*, or a report of what they believe they are doing, in committee and in full council. An account of procedures, or the preparation of the agenda, the ordering and control of the discussion, the way in which items are considered and ratification is obtained, would add greatly to the author's portrayal of an essentially static, self-preserving, protective body. This useful and thought-provoking book is, after all, called *The Politics of Policy in Local Government*. The author is well equipped to show how power is allocated and retained in conservative hands; perhaps he will now describe the routines of its everyday exercises.

JANET MORGAN

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Contemporary Political Philosophers. Edited by Anthony de Crespigny and Kenneth R. Minogue. (New York: Dodd-Mead, 1975. Pp. 296. $6.95, paper.)

The appearance of *Contemporary Political Philosophers* invites comparison with its predecessor volume, *Contemporary Political Theory*, not only because of the similarity of titles but also because Anthony de Crespigny was one of the editors on each occasion. The difference is greater than the titles might suggest. The earlier volume was a collection of articles dealing with major political theoretical concepts. The present book comprises eleven chapters, by as many different authors, each dealing with a major political philosopher of our time. In such a context, "theorist" and "philosopher" are often used interchangeably; but not so here. In this volume, the editors tell us "the term 'philosophy' describes a theoretical inquiry into the logical relations holding between the most general ideas used in the understanding of some area of reality" (p. ix). While it is not clear that this statement accurately describes each of the philosophers whose work is here subjected to scrutiny, it is true that they are all trying to see politics and "political things," as Strauss would say, in a very wide setting. Another definitional attempt by the editors is more general and hence more successful. According to it, "political philosophy is a speculative inquiry into the assumptions of the practical activity of politics" (p. ix). Only one author from the first volume appears as a subject of one of the essays here, John Rawls. Curiously enough, a fact worth pondering, he is the only professional philosopher in the list. (Or would Michael Oakeshott, in view of his early career, protest?)

The editors provide a brief introduction containing a number of arguable statements. To debate them here, however, would be to elevate the importance of these pages beyond the editors' intention. The editors' claim that their authors are seeking to understand rather than to persuade can be evaluated by anyone who is knowledgeable in this field by examining the list of authors whose works are discussed. The cast of characters, in the order of their appearance, governed by the year of their birth, is as follows: Marcuse, Hayek, Strauss, Voegelin, Oakeshott, Popper, de Jouvenel, Aron, Sartre, Arendt, Macpherson, and Rawls.

What do these eleven persons have in common that marks them off from other political "philosophers"? Not much, it would seem. Yet probably the second of the definitions quoted above comes close to the mark, although the one professional philosopher of the group, John Rawls, fits that description with difficulty, if at all. Clearly though, they are all persons who have taken a large, inquiring and probing view of the whole, or some important aspect of, the political scene, and who have dealt with their subject extensively. A common approach of political "theorists" today, the so-called "economic" approach, is to be sure ruled out by editors' terms of reference. Even the work of Charles E. Lindblom thus escapes notice. This exclusion is probably right, but when the names of Lindblom and Hayek are juxtaposed one realizes that difficult choices had to be made. Hayek takes the broader, more "philosophical" (in the sense used here) view; Lindblom has worked out a somewhat less encompassing position with a great deal more care.

The editors have wisely not sought to impose a common pattern on their contributors. Generally the authors attempt some sort of summary of the works of the philosopher in question. This function is most valuable in cases where the ideas of the person in question are either widely scattered (e.g., Sartre) or extremely abstruse (e.g., Marcuse). Critical evaluation is fairly extensive in some cases. Maurice Cranston provides an example of this method in his extremely good essay on Sartre ("Solitary Man in a Hostile Universe"). On the other hand, the expositions of the works of Strauss and Voegelin, by Eugene Miller and Dante Germino, respectively, are almost completely devoid of criticism, perhaps because expositor and philosopher are in each case in complete agreement. Nonetheless, Miller's sympathetic treatment of Strauss ("The Recovery of Political Philosophy") and of his attacks on positivism and historicism as the great enemies of political philosophy brings order and clarity to the ideas of a man whose own ideas often lie buried in his discussion of the works of others. On the other hand, Germino's very brief essay on Voegelin ("The In-Between of Human Life") leaves this reader unsatisfied. We have Voegelin's rejection of the fact/value dichotomy and his insistence on the all-pervasive tensions of life as quite proper foci for the discussion, but the conclusion that in Voegelin we are "in the presence of one of the most creative, sensitive, erudite, and profound philosophical minds of our century" (p. 119) needs elaboration and support.

David Kettler's discussion of Marcuse, under the subheading "Alienation and Negativity," is by a wide margin the longest essay in the book (48 pages) as opposed, for instance, to Germino's 20. The editors' generosity was well deserved. This thoughtful, probing, synthetic and analytical, expository and critical essay is the best in the volume. Its combination of sympathetic exposition and understanding with penetrating criticism is superb. Kettler finds Marcuse's wrestling with the problem of the politics of creating a new man to be profound and stimulating to political theorists, while being in the final analysis inadequate. This inadequacy may flow partly from three incompatible tendencies that appear in his work: progressive developmentalism, the "Stoic" tendency, and the "apocalyptic."

Carl Slevin declares that de Jouvenel's works "belong to the great tradition of political philosophy from Plato to Marx" (p. 168), yet his own statements regarding the "incompleteness" and "confusing" nature of much of his work lead one to wonder whether this author was properly included in the volume. Neal O'Sullivan's chapter on "Hannah Arendt: Hellenic Nostalgia and Industrial Society" is quite successful in capturing the essence (shall we say?) of this modern romantic. "She may be said," O'Sullivan writes, "to have provided a timely reminder of the autonomy of politics" (p. 249). The word "reminder" is well chosen, for certainly the position is not argued in any systematic fashion. Michael Weinstein performs something of a *tour de force* in writing an expository and critical account of Macpherson without even mentioning Marx or Marxism. From this account one would never guess that Macpherson's political theory relies almost completely upon his acceptance of Marxian economics. Space constraints rule out any discussion of chapters not mentioned above. The subtitles may give a hint of their content: "The Boundless Ocean of Politics" (Oakeshott); "Politics Without Essences" (Popper); "A Modern Classicist" (Aron); and "A Theory of Justice" (Rawls).

J. ROLAND PENNOCK

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Liberals, Radicals and Social Politics, 1892–1914. By H. V. Emy. (New York: Cambridge University Press, 1973. Pp. 318. $19.50.)

This is an interesting and ambitious, but by no means entirely successful, work. The author's aim is to explain the growth of commitment in the Liberal party to policies of advanced social reform, the extent to which that commitment was translated into legislation and positive governmental action, and the ideological basis that distinguished this so-called New Liberalism from the contemporaneous Socialist and Conservative approaches to the social problems of Great Britain. The main thrust of the book is to identify a body of thought which Dr. Emy, Lecturer in Politics at Monash University, Australia, calls "Social Radicalism" and to attribute to it the major responsibility for the shift in the emphases of Liberal policy.

Unfortunately, the timing of the publication of this book deprives it of a certain amount of its value. It originated as a Cambridge doctoral thesis in 1966, and, the author tells us, "both the reading and my practical interest in this period finished in late 1969." At that time very little had been published on the relationship between Liberal politics and social policy in the period 1892–1914, and the author had to investigate many different facets of the history of the Liberal party without assistance from specialized monographs on them. No sooner had he completed his thesis, however, than these monographs began to pour off the presses, and within a few years the landscape of the subject had been transformed. Indeed, he acknowledges in a preface which endeavors to take account of the implications of a few works published down to early 1972 that the concept of the "new Liberalism" is well on its way to acquiring the status of the new conventional wisdom.

From the perspective afforded by these more recent writings it is clear that Dr. Emy tackled, and indeed at the time probably had to tackle, too much. Works such as Jóse Harris's *Unemployment and Politics: A Study in English Social Policy 1886–1914* (Oxford: Clarendon Press, 1972), and P. F. Clarke's *Lancashire and the New Liberalism* (Cambridge: Cambridge University Press, 1971) explore aspects of the subject in far greater depth than was possible in his own wider-ranging survey. And yet it is also clear, again from the same perspective, that he left out too much. The omissions are all the more striking because of the obviousness of his attempt to say something about many different aspects of Liberal politics. One of them is the theme of the relevance of "national efficiency" and Liberal Imperialism to the relationship between the Liberal party and social politics. Dr. Emy attempts to justify his almost complete omission of this, but only in the preface and as an afterthought. The reader must also use two other recently published works—G. R. Searle, *The Quest for National Efficiency. A Study in British Politics and Political Thought, 1899–1914* (Oxford: Oxford University Press, 1971), and H. C. G. Matthew, *The Liberal Imperialists. The ideas and politics of a post-Gladstonian élite* (Oxford: Basil Blackwell Press, 1973)—if he wishes to have a balanced picture of the forces contributing to the growth of Liberal concern for social reform.

Second, Dr. Emy inadequately treats the nature of the transition from "traditional" Radicalism to the "Social Radicalism" of the early twentieth century. This may derive from his tendency to dwell too much in the realms of social and political theory when explaining this transition. He has too little to say about the questions which mattered to the nineteenth-century Radicals and about how their interest in these often provided a bridge to the themes of twentieth-century Radical politics. An example is the temperance question. For many nineteenth-century Radicals, temperance reform was itself "social Radicalism," and much of the debate on it focused on the causes of poverty and the need for legislative action, even of quite a coercive character, to remedy them. Dr. Emy says almost nothing about this very powerful current in late nineteenth-century Radicalism. He might have pondered the significance of the fact, which he cites, that whereas Herbert Samuel's *Liberalism*, published in 1902, had sold only 2,250 copies by July 1904, A. J. Sherwell's *Temperance and the Social Problem* had sold 90,000 copies and gone through ten editions since 1899.

There is much that is useful in this book, for instance the analysis of the backgrounds and voting records of Liberal M.P.s, and the discussion of changing trade-union attitudes to state intervention. But the book can be heavy going in places, and the style awkward and plodding, especially when the author is summarizing theories. The overall development of the book lacks momentum and vigor. One feels that, useful though many of the parts are, they are doomed gradually to be superseded by more substantial analyses so that a new synthesis will have to be written in its place.

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Money and Economic Development: The Horowitz Lectures of 1972. By Milton Friedman. (New York: Praeger Publishers, 1973. Pp. 80. $8.50.)

Money and Economic Development is a brief introduction to Milton Friedman's monetarist doctrines. Friedman's basic point is that the quantity of money in circulation is of paramount importance in determining both short term macroeconomic fluctuations and the long term trend in prices. This idea challenges the Keynesian macroeconomic model in which money plays merely a minor role. The policy prescriptions of these two models are reflected in their relative emphasis on monetary and fiscal tools.

The first lecture in this volume surveys evidence that money matters in determining the level of income and prices. Much of the evidence is given in the form of tables indicating positive correlations within countries between the money stock and income and between the money stock and prices. Friedman asserts that a change in the rate of monetary growth produces a change in the rate of growth in (nominal) income about six to nine months later and an effect on prices some nine to fifteen months after the change in income. The six- to nine-month money-income lag appears to be quite robust across countries. In addition to the tables, Friedman cites further evidence for the importance of money, the most impressive example of which refers to the Great Depression. Between 1929 and 1933 the quantity of money declined by one-third in the United States. Given both the relationship between money and income and the control of the money stock by the Federal Reserve Bank, it follows that the Great Depression reflects, at least in part, the pernicious effect of inept monetary policy.

Friedman's second lecture is concerned with monetary policy in developing countries. He notes that there appears to be no necessary relationship between the rate of inflation and the rate of economic development. The most striking examples of development in fact have occurred without inflation: Great Britain in the eighteenth and nineteenth centuries, the United States in the nineteenth century, Japan after the Meiji Restoration in 1867 up to World War I, Greece in the late 1950s and early 1960s and Hong Kong, Malaysia, and Singapore in the past thirty years. Friedman also points out that inflation can be viewed as a tax on money balances and that open inflation is a lesser evil than repressed inflation in which prices are prevented by governmental measures from rising to clear the market. With price controls in effect, resources that could be utilized productively are instead employed in circumventing the government regulations; in extreme cases, individuals will eschew the legal currency and resort to barter. Friedman's policy prescriptions for developing countries are to maintain a stable and moderate growth in the monetary stock and to impose no controls on the levels of foreign exchange rates, interest rates, prices, and wages.

Both lectures are followed by questions and answers. Some discussion, especially in the second lecture, refers specifically to Israel, the host country for the annual Horowitz Lectures.

Individuals already familiar with Friedman's views will find much of this book redundant. Those wishing to probe in some detail the analytical foundations and empirical studies on which Friedman's conclusions are based will do better to consult Friedman's more technical writings. *Money and Economic Development* is an ideal book, however, for readers desiring a refresher course in or an introduction to monetarism. It is a clearly written volume by one of the world's outstanding and increasingly influential economists.

FARRELL E. BLOCH

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Die Neubestimmung des subjektiven Faktors im Neomarxismus. Eine Analyse des voluntaristischen Geschichtsverstaendnisses der Neuen Linken. By Bernd Guggenberger. (Freiburg und Muenchen: Verlag Karl Alber, 1973. Pp. 444. DM 43.-)

The question of how much space objective conditions leave for human action is of particular and rather painful importance for Marxism, which on the one hand sees itself as a philosophy of action, a revolutionary philosophy, and on the other hand believes in historical inevitabilities, based mainly on the growth of technology. In the classical works of Marxism this contradiction remains essentially unresolved, nor have the twentieth-century social scientists of the so-called Frankfurt school—men like Horkheimer, Marcuse, Habermas—who regard themselves as Marxists, been able to achieve more than a questionable compromise between determinism and revolutionary voluntarism. They have transmitted the great Marxist dilemma to the radical youngsters of the New Left, whom they have supplied with some rationalizations for their emotional extremism.

It is Guggenberger's main proposition, supported by much evidence, that the basic orientation of the New Left is far more voluntaristic than that of classical Marxism and has remained so even after the movement has more or less abandoned the "anti-authoritarian" label and put itself largely under the Marxist flag. Guggenberger distinguishes between two kinds of voluntarism. The first is "functional" voluntarism, which recognizes the restriction of "the possibilities of the human will by the effectiveness of historical tendencies" (p. 25); into this category he puts classical Marxism. The second kind is "voluntarism as a matter of principle" *[prinzipieller Voluntarismus]*, the main target of his criticism, which he correctly sees as the philosophy of the New Left. *Prinzipieller Voluntarismus* "takes as its point of orientation neither the facts of the material world nor the valid technological insights" and seeks salvation in "the disregard of *[Abkehr von]* the 'scandalous' *[niedertraechtigen]* reality" (p. 33).

As Guggenberger convincingly explains, this disregard of the obvious limitations of human action is psychologically facilitated by two characteristics of the New Left attitude. The first is "the conscious, voluntaristic anticipation of the final, ideal state of affairs *[willentlich-bewusste Vorwegnahme eines idealen Endzustands]*" (p. 239). The exclusively future-oriented mind can easily overlook the obstacles inherent in the present. The second factor is the prevalence of the desire for self-expression over the wish for the achievement of goals, however important the role of the latter may be in the movement's verbiage. "It is a tendency of the radical approach to pay less attention to the result than to the purity and constancy of the demands *[Anspruch]*" (p. 238).

In reality, human action in the present is limited not only by elements in the physical environment as it now exists, but also by the heritage from the past. The voluntarism of the New Left does not recognize these limitations either, and therefore—in strong contrast to classical Marxism—is necessarily anti-historical. "The interaction *[Verschraenkung]* of continuity and [dialectical] rupture which in historical materialism characterizes historical reality, is dissolved for the New Left in the maximalism of postulates. . ." (p. 372).

All this is very well explained by Guggenberger, although occasionally his language is unnecessarily difficult. One might raise the question, however, whether the title of the book is really justified: Since the radical voluntarism of the New Left is so fundamentally opposed to basic tenets of Marxism, should one really put the movement in the category of neo-Marxism? More important still, although the job which Guggenberger has done in analyzing the ideas taken over by the New Left from the Frankfurt school and showing their differences from true Marxism is very useful, should not a warning have been added against attributing to these ideas a causal significance which they do not possess? The New Left has not been created by the writings of Horkheimer, Adorno, Habermas or Marcuse, but by realities such as the state of the universities, by racial problems, by the Vietnam War and by disgust with the smugness of a consumer society in a state of relative abundance. The glaring inadequacy of the New Left's approach to these problems must not obliterate the fact that these realities of life, however thoroughly misunderstood, and not the writings of the Frankfurt school, constitute the real origin of the movement.

In spite of these few doubtful points, Guggenberger's book is a very valuable guide to the understanding of a movement which, though perhaps not destined for a long duration, has at a historical moment brought about a crisis in the thinking and feeling of a generation and especially in the life of our institutions of higher learning. The book certainly deserves an English translation.

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Selections from the Smuts Papers, Vols. I–IV. Edited by W. K. Hancock and Jean van der Poel. 1966; Vols. V–VII. Edited by Jean van der Poel, 1973. (New York: Cambridge University Press. Price of each volume $47.50.)

In 1955 Sir Keith Hancock, in his Creighton lecture in the University of London, told how the Smuts papers were then being assembled, arranged, catalogued and provided for. Sir Keith had been commissioned by the Cambridge University Press to write the biography of General Smuts, a task which he accepted after consultation with Smuts's widow and their son J. C. Smuts, Jr., the literary executor. After he had looked at the papers in the family's possession, and discussed the problem with the heads of two South African universities, a decision was made to set up a Trust "to be responsible for the safe-keeping and management of the Collection." Thus the Smuts Archive began. In the following years it was steadily enlarged, mainly by the acquisition of out-letters from their recipients. Dr. Jean van der Poel, given leave from the University of Cape Town to become a full-time archivist for two and a half years, brought order out of the original chaos.

The two volumes of the biography appeared in 1962 and 1968. It had been decided also to publish a selection from the papers. The first four volumes of these, edited by Sir Keith and Dr. van der Poel and covering the period up to 1919, appeared in 1966; the remaining three, by Dr. van der Poel alone, in 1973.

How is this monumental work to be assessed? Its value for the historian is necessarily uneven, but the editors cannot be criticized for their selection; they alone know what was in the documents they did not publish. The collection includes nearly two thousand items. We are not told the number in the Archive, except that 45,000 had been catalogued by 1964. With a few exceptions official papers have been excluded by the editors. There are memoranda, jottings from notebooks, a few articles that were published at the time, and drafts of others that were not; but the bulk of the collection consists of private correspondence. Much of this was with the Gilletts and Clarks and other friends who were not politicians, though there was plenty of politics in the letters. Partly for this reason the greatest achievement of this work is to portray Smuts the man, as only fifty years of outpourings and unburdenings to intimate friends could do. For that very reason it is impossible to summarize the portrayal here.

What most historians look for in their primary sources is the origin and explanation of decisions. Some of Smuts's most important decisions, especially his successive refusals to face the intractable problems of color, were negative ones. Some explanation of this is found in his correspondence with Merriman, included in these volumes but available earlier in the Merriman papers edited by Phyllis Lewsen. More is revealed here, especially in his reactions to Hertzog's "Native Bills," from their conception to their enactment. Smuts's opposition to them was ambivalent at all stages, and in the end he voted for them. "I view these bills in a detached spirit, as a matter decided for me in far-off days by my temperament and outlook which cannot now be affected by what passes or happens to-day. But of course I see the rocks ahead quite clearly" (VI, 32). This paradox is the tragedy in his career; when he tried, in the end, to steer away from the rocks it was too late.

Smuts, the philosopher of *Holism,* could never confine his thought or activity to the petty theater of his own or any other country. The collection is very rich in self-revelations which recount and partly explain his operations on the world stage. For instance, his hasty recognition of the state of Israel, just before the election of 1948 which ended his career, has been regarded as one of the factors in that defeat; but in these volumes we follow his support of Zionism over many years. In the "explosive" speech in London in November, 1943, he belittled the role of France in the postwar world, thereby giving great offense. This explosion, too, had a long fuse behind it, which can be traced at least to French intransigence over the reparations question in 1919 and thereafter. It may be fanciful to see the beginning of his pro-American feeling in the youthful essay on Walt Whitman. Even his association with Woodrow Wilson must be seen in the context of later American isolationism. But the visit to the United States in January, 1930, produced a lasting effect which Smuts described at the time in his speech to the Empire Parliamentary Association: "I found a very good spirit in the United States. . . . I found a kindliness of feeling and sympathy, a real drawing towards this country [Britain], that no reading of mine had led me to infer" (V, 451-52).

In this he was swimming with the tide of history; there were times when he swam against it, notably when the tide was flowing against the banks of racial discrimination which were a part of Smuts's hereditary environment. At one point he opposed Hertzog's proposal of communal representation in Parliament for Africans on the ground that these representatives might hold the balance of power if the white parties were fairly evenly divided. In 1924 he was pleading with L. S. Amery for a "white highlands" policy in East Africa: "All the highlands of eastern Africa from the Union to Abyssinia are healthy for Europeans and can be made into a great European state or system of states during the next three or four generations . . . the danger is that one of the greatest chances in our history will be missed" (X, 238.)

Some might argue that the struggle to strengthen the British Empire or Commonwealth was another example of resistance to the forces of history. However that might be, Smuts's plea to Churchill in 1949 to reject India's request to remain in the Commonwealth as a republic was really a piece of South African politics. The plea was rejected, and the Indian example did, as Smuts feared, provide ammunition for his enemies at home.

Historians will look to this collection, however, not merely for clues to Smuts's thinking but for explanations of specific events. In this regard there are some disappointments, partly because few official documents have been included, partly because many important discussions were not recorded. Smuts was said to have remarked on some occasion that, for this reason, historians never knew the truth. Thus, we are tantalized by the absence of revealing documents in connection with the strikes of 1913, 1914 and 1922; with the negotiations for coalition and fusion in 1933 and 1934; and with the undertakings (or nonundertakings) about war and neutrality in 1938 and 1939. The only important information we are given on this last point is taken from *Hansard.*

It would be wrong, however, to emphasize the gaps too much. Even on the events leading to the second World War there are letters which help to explain the misunderstanding with Hertzog when war came. Smuts disapproved of Chamberlain's guarantee to Poland—no wonder, for in 1919 in a memorandum to Lloyd George he had written: "I am convinced that in the undue enlargement of Poland we are not only reversing the verdict of history, but committing a cardinal error of policy which history will yet avenge." In 1939 the Germans published this memorandum. At the time of the Austrian *Anschluss* Smuts wrote to Amery that "as regards the Dominions, they will fight for Great Britain if attacked; they will not fight in the battles of central or south-eastern Europe. I even have my doubts whether they will fight again for France and Belgium." (VI, 125). Earlier still he had thought that the best policy was to try to work with Hitler, but against Mussolini. By September, 1939, he had come to see the situation in a different light, whereas for Hertzog nothing had changed. Still, we should have liked to hear from Smuts during the Munich crisis; but at that point there is a gap of more than a month in the correspondence.

There is no gap at the critical moment in Ireland. At the special urging of George V, Smuts saw de Valera in Dublin in July, 1921; the meeting and the subsequent report to the King are well documented. "General Smuts explained that he did not come as an emissary of the British government, nor did he bring any offer from them. In fact he had nothing to do with the British government. He came as a friend who had passed through very similar circumstances. . ." (V, 95). If anyone could have persuaded de Valera to compromise, it was the Boer General who had fought imperial Britain to the bitter end, but had since seen his early ideals sublimated and enlarged. De Valera, who was a Hertzog and not a Smuts, would not compromise; but some other Irishmen, partly through Smuts's influence, did.

In 1924 Smuts, who throughout his public life had been in power, was defeated in a general election. He half expected to be thrown into opposition again in 1939; in 1948, when he did not expect it, it happened. On these occasions, and on others when no change was really in sight, he expressed a longing to lay down the burden, and some gratification when it was laid down. As he went into the election campaign of 1948 he wrote to Margaret Gillett: ". . . I hope I shall stand and survive this last fight of mine. My heart is not in it, but compulsion is laid upon me" (VII, 192). Was this hypocrisy? No, it was the ambivalence that appears again and again as he kicked against the limitations of time and space. In 1917 he turned down the offer of the Palestine command after much hesitation. He was a member of the British War Cabinet, but decided not to regularize that position by getting elected to the House of Commons. In South Africa he longed for the wider world; when he was abroad, the pull of South Africa became irresistible.

To illustrate the value of the collection with examples, or even categories of examples, is, however, to give a grossly inadequate impression. One more instance, because it does not fit into a category, may make this point. It is well known that the evidence of Joseph Chamberlain's complicity in the Jameson Raid was first revealed in the papers left by Sir Graham Bower to be opened in 1946. We now know that Smuts learned the story from Bower himself when he visited him in England in 1931.

One can only conclude with the epitaph written by Arthur Gillett after the defeat of 1948: "From a historical point of view this lets Oom [Uncle] Jannie out. What he stood for and what he achieved will be, no doubt, regarded as a Golden Age in South Africa's history. What will be done now will corrupt and disgrace the name of South Africa and it will be the past that will be all that is left" (VII, 283n.).

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Political Science Bibliographies, Vol. I. By Robert B. Harmon. (Metuchen, N.J.: The Scarecrow Press, 1973. Pp. 181. $6.00.)

While most bibliographies in political science are appended to books or periodical articles dealing with a particular subject matter, a considerable number of bibliographies, because of their size or scope, are published separately in book form. Robert B. Harmon, a librarian at San Jose State University and author of several bibliographic works, has attempted to list as many of these separately published bibliographies as possible. The result is a partially annotated list of 790 bibliographies considered to be of possible interest to students of politics. The bibliographies are listed under political subject terms or by countries in order to provide convenient access to the literature. Separate indexes by author and title are also provided.

Regrettably, the present volume offers by no means a comprehensive or exhaustive listing of separately published bibliographies, since many important titles, such as the *Handbook of Latin American Studies, U.S. and Canadian Publications on Africa,* Comitas's *Caribbeana 1900–1965,* Hammond's *Soviet Foreign Relations and World Communism,* and others, are not included. In spite of these omissions, the reference work does provide a valuable subject, author, and title approach to many useful bibliographies in political science which might otherwise be difficult to identify.

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Naturzustand und Naturgeschichte der Bürgerlichen Gesellschaft. By Hans Medick. (Göttigen: Vandenhoeck & Ruprecht, 1973. Pp. 330. 38 DM.)

In this fifth volume of the *Critical Studies in History*, Hans Medick's contribution, *The State of Nature and the Natural History of Bourgeois Society*, explores the dialectic of forms of knowledge and guiding historical "interests" or values. His intent is to expose the implicit "cognitive interests" which inform and legitimate every methodology, especially those in the social sciences. Medick focuses on early sources of bourgeois social science, ranging from Pufendorf's *State of Human Nature* to Locke's *Two Treatises on Government*, Dugald Stewart's *Natural History of Society*, and a number of Adam Smith's contributions to the formulation of social science.

(In an exemplary section, Medick considers "the Adam Smith problem": namely historians' tendency to ignore the practical thrust of Smith's political-economic analyses and the normative focus of his methodological formulations. Smith explicitly and emphatically sustained this normative focus, even as he began dividing the comprehensive domain of moral philosophy into empirically oriented single "disciplines").

Medick locates the origins of the scientific study of social phenomena in the emancipatory impact of enlightenment theories of natural law. According to Medick, these theories understood scientific control over nature and a scientifically administered society as a means to a more humane body politic. The values of the good society were thus not beyond scientific rational discourse but were emphatically kept within that arena of discourse. The analysis of the status quo, indicted as imperfect, was preparatory but not the actual task of social science. The origins and functions of values and laws themselves had to be continuously reassessed in view of the changing historical context—changing, to a large extent, because of the ever-increasing scientific and technical control over this context.

Emulating his chosen authors in their interdependent methodological and substantive considerations, Medick makes their point that a particular method presupposes a particular delimitation of an object domain. The domain of the "social," carved out by these writers, differed considerably from that cut to size by and for those later scholars claiming this heritage. For the first time, "society" constituted an entity sui generis, acting through and above the heads of its participating individuals. These founders "discovered" society as a proper object of scientific study; but they certainly did not feel that a methodology should, or even could, abstract a pure technique from the values that constituted its own selectivity—a selectivity emanating from the interests and demands of its very object domain. We cannot simply reify a (scientific) method, and then declare a certain part of the object domain delimited by that method inaccessible to it.

Far from intending to describe an existing domain with eternalistic and absolutistic models, concepts of the "state of nature" and "natural history" presented methodical devices and catalysts for conceptualizing the slowly emerging bourgeois society, according to Medick. Arguing against Habermas's charges of the "naive evolutionism" of the natural historians, Medick investigates "natural history" as "a specific science of the new bourgeois society" whose intent was not only to systematize and rationalize the current interest patterns of that society; rather, the self-definition of this science entailed a new form of theory and practice of social enlightenment, which conceived of bourgeois society as a mere stage on the way to the formation of a society of the "democratic intellect" (p. 116).

Given such an understanding, we can explain the methodological and political priority of domination-free (not "value-free") rational public discourse, for which "scientific" discourse was to be a model. The way to secure the *autonomy* of "private" individuals constituting such an ideal "public" was, under those conditions, clearly through economic independence; thus, the emphasis on the contracted sacredness of property. The freedom it granted was the sole guarantor of moral autonomy. The generation of material autonomy for everyone, and a "scientifically" advised and administered body politic to vouchsafe this autonomy, were *immediately* necessary, but not sufficient conditions for the "good society." The purpose of social "science," for these writers, was to unearth the historical and palpable potential of their society for self-transcedence toward the "good society," through accurate and systematic description and a methodology informed by and conscious of that value.

On the whole, Medick's argument is tightly woven, albeit redundant in parts. His points are both very well documented and interpreted with keen imagination. Not intended as a survey study, the approach is unavoidably selective, invoking precisely those passages often neglected as momentary aberrations for these writers. Medick persuades us that there is method in these passages (as there is in their neglect). The method is the model of self-reflexive scientific discourse, for which its own premises and techniques remain integral parts of the object under investigation. Scientifically minded enlightenment philosophers, the founders of modern social science that Medick reviews, seem to have shared a vision of a practical science which had not yet been dissolved in an "applied" science.

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Environment: A Bibliography of Social Science and Related Literature. Compiled by Denton E. Morrison, Kenneth E. Hornback and W. Keith Warner. (Washington, D.C.: U.S. Government Printing Office. U.S. Environmental Protection Agency, 1973. Pp. 860. $7.45.)

The focus of this bibliography, much needed by researchers and decision makers alike, is "literature that deals directly or indirectly with broadly conceived social science aspects of man and his activities in relation to the *natural* environment" (p. 1). Happily, the compilers have also included items dealing with social, political, technological and other environments which are in some way "relevant to the man-natural environmental relationship" (p. 1).

The first two-thirds of the book provides a numbered list of 4,892 titles, arranged alphabetically by author. The remainder cross-lists the numbered titles (nonpunctuated, sometimes abbreviated, in computer type) under 42 categories. These are neither mutually exclusive nor logically parallel. Nor do they conform to conventional divisions of the various social sciences. The category titles reflect the complexity, overlappings and interrelatedness of the hundreds of concepts and theories included under the rubric "social science and environment." As noted inside the front cover, rejection of traditional labels (as in other volumes of the EPA Research Reporting Series) was done with specific intent to "foster technology transfer and a maximum interface in related fields."

Environmental problems are unlikely to be treated effectively without large inputs from the social sciences—not least from political science. Most remedial and preventive programs involve common boundaries between some aspect of political science and other fields of knowledge. The political scientist, scanning the list of category titles, will discover only a few which obviously fall within his discipline. Perusal of other related categories, however, is likely to turn up many items of political relevance. A few hours spent going through this bibliography—the compiler suggests a procedure for using it (pp. 2–3)—should save countless hours searching for materials, and it should also alert more political scientists to the wide spectrum of environmental problems to which their professional expertise can contribute greatly.

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The Measurement of Economic and Social Performance. Edited by Milton Moss. (New York: National Bureau of Economic Research, Inc. and Columbia University Press, 1973. Pp. 605. $17.50.)

The political scientist who peruses this book will be in for a mild disappointment, though a thorough reading will considerably assuage his or her initial regrets. As Simon Kuznets, Nobel economist and father of our modern national income accounts, notes in the conclusion, the essays in *Measurement* are concerned largely with more comprehensive and accurate *market* valuations. The valuation of *social* phenomena, i.e., the interface between economics, politics, and sociology, is neglected in more than half the essays and most of the comments.

The measurement of real income is a hoary activity among economists. Sir William Petty, the first serious practitioner of "political arithmetick," predated Adam Smith by almost a century. Since that time controversy over the appropriate scope and method of social income accounting has been vigorous and continuous. Specifically, this discussion has pursued three conceptually separate, but nonetheless parallel, questions: Do the sectors covered in the income accounts exhaust what is generally considered to be exchange transactions? Do changes in the components of income and aggregates reflect anything about the associated welfare of the various individuals composing the group(s) whose transactions are enumerated. Finally, can measures of income, if taken as proxies for welfare, be fruitfully compared over time and place?

The latest NBER collection on this subject (there have been several before) ignores these latter two questions, but concentrates on extending the class of actions that usefully might be included as real income. Analytic notions of household durable capital yielding present and future streams of implicit rental income, the family as a jointly maximizing firm taking advantage of specialization economies in market and household production, the value of leisure as a consumer good, urban congestion and environmental pollution as negative goods or social bads, and the government as a nonmarket firm supplying heterogeneous outputs are developed and explored both for their impact on the structure of national income accounting and for reassessing growth measures of the recent past.

Moss's introduction admirably surveys each essay and its ensuing comments. The reader is advised to study this summary closely. The initial section, "Proposals for New Frameworks for the Measurement of Economic and Social Performance,"makes a number of proposals for incorporation of more *social* transactions into the national income accounts. The two papers by J. Thomas Juster and by Nancy and Richard Ruggles provoked vigorous and cogent dissents by the commentators, highlighting the practical, and to a lesser extent, theoretical, difficulties of deciding whether an action is a final or intermediate good. (If the latter, its inclusion in the GNP may lead to double counting as its value will also be embedded in some final product.)

The second section, "The Household and Business Sectors," should greatly interest the noneconomist social scientist reader. Reuben Gronau's paper on valuation of Israel housewives' time employs Becker's theory of time allocation and notions of household production. Gronau clearly demonstrates the powerful methodology of economics. Nonobservable "tastes" are carefully set aside *(de gustibus non disputandum)* and changes in "constraints," those observable trade-offs, are noted. The theory's predicted outcomes are subjected to empirical refutation. The essay is a model of careful and relevant social-economic research. Dan Usher's measurement of the value of a longer life span and its implication for Canadian economic growth are original and useful. Robert Willis's comments make worthwhile reading, too. Laurits Christensen and Dale Jorgenson's highly competent essay presents a new accounting system which holds prices constant, but does so with their own construction, "the standard of living" index. The presentation of private national income data for the period 1929–69 attests to the operational character of their new technique.

The third section, "The Public Sector," has two essays. The second by Alice Rivlin explores the difficulties of treating a many-dimensioned product like education as if it were a homogeneous service of constant cost and unvarying quality. Mancur Olson also discusses the problem that this absence of product measurement presents for public sector evaluations. He argues that much, if not all, inefficiency in the public sector exists because of the inability to monitor what kind, what quality, and how much of each item is supplied or purchased by a public agency. Surprisingly, he neglects to consider problems of political demand articulation, the separation of "ownership" from "control," and the effects of an activist bureaucracy on budgets and outputs as well as the level of unit costs. Schultze briefly notes some of these omissions in his commentary.

The absence, however, of any reference in this section to the current wealth of applied-theoretical and empirical studies on public choice seems a pity. For instance, the notion of political action as resource using is not developed, nor are the findings of scholars in the area cited. If government is viewed as the most important, if not the final, arbiter of property rights, individuals will devote time, energy, and other resources to influencing public choice. Lobbying, supplying information (often biased), organizing voting blocks, rearranging the family's, firm's, or group's factor or product mix, engaging in bribery, contributing to political parties and candidates, supplying of passive resistance, terrorism, rebellion, and international aggression are just some of the devices individuals and groups use to redistribute income toward themselves. Neglecting the cost of acquiring these transfers, regulations, special-interest spendings, and market restrictions vastly understates the cost of government.

In the three papers in the last section, "Measuring the Amenities and Disamenities of Economic Growth," Orris Herfindahl and Allen Kneese, William Nordhaus and James Tobin, and Nobel laureate Wassily Leontieff variously attempt to measure and deduct the social cost of certain externalities (pollution and urban congestion) and national defense (an intermediate good) while adding the positive value of leisure (a nonmarket good) to the GNP accounts. Herfindahl and Kneese seem to despair of accurate inclusion of environmental damages into GNP measures, but do press for separate preparation of series on waste control and other emission-related costs. Nordhaus and Tobin recalculate the U.S. growth rate of their MEW ("measure of economic welfare"), their amenity-disamenity adjusted GNP measure for 1929–65. Again, the commentators provide lively criticisms.

It is interesting to note that nowhere in this or the initial section is perhaps the most important nonmarket amenity measured discussed or even briefly considered, consumers' gains from exchange. Factor rents, the surplus over and above the opportunity cost necessary to keep an input from going to its next best alternative, are largely captured in the market valuation process; hence, they are included in GNP. Consumer surplus is necessarily psychic in nature, and is determined by measuring the difference between what an individual would pay for an item rather than do without it and what the market requires that he pay. Techniques for its measurement, part of every standard cost-benefit analyst's tool kit, are available, though the "cost" of such measure is probably not low because of theoretical and measurement difficulties. At present, the implicit assumption of those using GNP as an indicator of real income is that the ratio of consumer surplus to measured income is more or less fixed. Unfortunately, I know of no theoretical proposition that makes such an assertion remotely plausible.

The final conclusions by Kuznets note the severe limitations placed upon social income by the present level of theoretical understanding. He points out that this latter constraint, together with the high cost of getting the requisite information, and the inherent ambiguities of classification result in inevitable compromises. He does not feel, however, that these accommodations vitiate the usefulness of the national income accounts, but he hopes that the suggestions in *Measurement* prove useful for future real-world applications in social accounting.

One hopes that interest in this volume will prompt the NBER to sponsor further work in "political arithmetick" even more along lines suggested by the title of this latest volume.

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Jean-Jacques Rousseau on the Individual and Society. By Merle L. Perkins. (Lexington, Kentucky: The University Press of Kentucky, 1974. Pp. 342. Price unknown.)

The author tells us that his initial intent was to do a study of Rousseau's treatment of international politics, more specifically to use Rousseau's perspective on this subject as a kind of window through which to look at his larger work. I confess, I am sorry he did not stay with this intent. Although, as Perkins notes, Rousseau's ideas on international relations have been the subject of at least two major works, those by the French scholars, J. L. Windenberger (1900) and Lassûdrie-Duchêne (1906) and are certainly not completely unknown to the ordinary Rousseau reader, they could nevertheless have stood re-examination, and would have made for a somewhat more valuable work than the one before us where the subject occupies only two brief chapters.

The book consists of successive chapters of explication of Rousseau's basic works, beginning with the posthumous *Confessions* from which a number of leading ideas are gleaned, continuing through the three discourses (a separate chapter on each), the famous letter to D'Alembert, *La Nouvelle Héloise, Emile, The Social Contract,* each given some degree of illumination from letters and other writings. If this sounds rather mechanical, it is offset by at least the author's effort to supply some kind of continuing theme of inquiry. He tells us that "what the present book tries to offer is a full reevaluation of the great themes of Rousseau's social and political thought, of which international relations form no more than a part, albeit an essential part." Perkins continues: "Rousseau's principles of individual liberty and political power within the nation are closely, even inseparably, tied to his ideas about the bonds and frictions existing between nations. Influences move back and forth between these two levels of his thought. The two levels have an organic relationship and structure and must be treated as a unit." That is commendable enough, and each reader will have to be his own judge of how well Perkins has fulfilled his intent. My own reaction is that altogether too much that is without real insight or use has gone into elaboration of the first level, that of "Rousseau's principles of individual liberty and political power."

Useful insights are certainly to be had from time to time in the book. Perkins clearly has read his texts. The structure of the book makes it a valuable one, for the most part, for anyone seeking overview of Rousseau's major political writings. And I agree entirely that the order in which these are set forth and described is the proper one. We are able to see early appearances of ideas which were to become central, as, for example, in the *Confessions,* the illumination which came over Rousseau while still an attaché of the French government in Venice, in 1743—the illumination that politics alone was the way of redemption, that however one looked, morality could always be seen the handmaiden of political government. Through Perkins's organization of his book, we can see the development and amplification of this initial revelation through Rousseau's successive works.

Less successful is Perkins's effort to supply "integration" as well as chronological analysis of the major works. To be sure any writer seeking such integration must have the reader's sympathy. The sheer variety and number of such searches in the vast bibliography that now exists in Rousseau's name attests to that. But sympathy aside, it has to be said that one struggles vainly in this book for firm grip on the author's controlling idea or theme. There is nothing comparable to what Ernest Wright did a generation ago with the idea of nature in his *The Meaning of Rousseau,* a work found in Perkins's bibliography but not, alas, in the text, or what Lester Crocker has done with the idea of power in his very recent study of Rousseau's *Social Contract.* Too often a promising lead is dealt with briefly, then dropped when Perkins reaches his next chapter. Much of the reason for indecision and fuzziness here lies, I think, in Perkins's less than forthright stand on Rousseau's commitment to absolute political power, a commitment announced clearly enough in the third discourse and made the subject of extended treatment in the *Social Contract.* I don't question that a respectable case can be made for Rousseau's commitment also to individual freedom, but this has to be seen against more than a theory of virtue, more than a conception of the self; it has to be dealt with without flinching against the theory of total power advanced in the key political works.

A second failing of the book flows from a perhaps too intense utilization of explication text by text. There wasn't room enough apparently for the kind of illumination of Rousseau's ideas that can come only from setting his method, premises, and major conclusions against the main intellectual and cultural currents of his time. A single illustration will serve here: Perkins's effort to explain the method used by Rousseau in the second discourse, the method of "hypothetical" history, without so much as a reference to the very common use of this method in the eighteenth century on both sides of the Channel, no matter what it was called—"hypothetical," "conjectural," "natural" or whatever. I realize one can't accomplish everything in a single book. But the yield of Perkins's explication of the successive texts is, I fear, too slender to fully justify his dependence on this always perilous approach.

For myself I find the two chapters on international relations the most profitable in the book. Perkins shows us in detail the extent—surprising to this reader at least—of Rousseau's interest in continents and peoples other than those of the Western world. The order in which Perkins presents these non-Western peoples and places is his own and seems to be admirable, moving from Africa to South America, to Asia, and finally to Europe. Rousseau was far from being the best-read or most learned writer of his time, but what he read on distant places, whether in space or time, stayed with him, and, as is true of just about everything Rousseau absorbed, he knew how to make a little bit of fact go a long way with reference to philosophical design. I hope very much that, with this book behind him, Perkins will turn now to what I think still interests him the most: a full-dress treatment of Rousseau's theory of international relations, with all else made subordinate.

ROBERT NISBET

Columbia University

Rational Legitimacy: A Theory of Political Support. By Ronald Rogowski. (Princeton, N.J.: Princeton University Press, 1974. Pp. 313. $13.50.)

In a small and elegant book, replete with mathematization and logically-derived hypotheses, and full of illustrative case histories, Rogowski argues that legitimate constitutions take forms which are consistent with the rational choices of their members, or with what those rational choices ought to be if they made them consciously. He offers his theory of rational legitimacy as an explanation of why peoples, differently constituted in their social structures, should and do prefer and support constitutional arrangements rationally consistent with those social structures, as a superior alternative (stronger theoretically) to political culture and system performance explanations for the phenomena of legitimacy and political support.

While he argues that a rational choice theory of constitutional legitimacy is a stronger theory, it is not quite clear how far he is prepared to go in arguing for its strength. While he clearly represents it as superior to political culture and system performance explanations, he draws back a bit from the claim that his work actually produces lawful explanations of the relationships between rational choice based on interest and the structure of constitutional arrangements. He makes the lesser claim (p. 34) that his theory generates a variety of quite specific confirmable and disconfirmable hypotheses. But he does not himself undertake to disconfirm his hypotheses. The case study strategy that he preferred was one designed to show that ". . . the otherwise rather abstract discussion has some connection with social and political reality, and is at least plausible in light of that reality" (p. 34).

The concluding sentences of his book again leave the reader in doubt as to what level of explanation he is claiming: "The theory I have advanced here is a possible way of predicting, and even prescribing, political and constitutional arrangements from a knowledge of occupational stratification and mobility, and of such factors affecting factional division as language, race, religion, and the structure of communications. It is an interpretation, not a vision, of history." The terms *a possible way* and *interpretation* suggest that Rogowski is hedging his bets. If a test of his hypotheses should turn out with less than lawful outcomes, then his contribution could be judged on its heuristic value.

As a heuristic exercise there can be no question of the great merit of this work, of the imaginativeness, the resourcefulness, and the analytic rigor of its author. In making out his case for a rational choice theory of legitimacy, Rogowski first disposes of two competing theoretical approaches to legitimacy; the political culture and socialization approach, and the deprivation or performance approach. In rejecting the political culture approach he does a bit of straw-man construction. He throws all of the theorists in this category together and imputes to them the proposition that political *actions* can be explained by *value* socialization in early life and then proceeds to demonstrate by the recent history of Germany that radically different forms of political structure have been associated with basically similar socialization processes. The fact that political culture theory includes a cognitive, rational, problem-solving component, and that socialization has been defined as including adult experiences in working situations, in voluntary association experiences, in political party and electoral experiences, and in exposure to the media of communication, is brushed aside by Rogowski as reducing the strength of the theory to the point ". . . where nothing may safely be neglected" (p. 17). The point needs to be made that political culture theory never claimed to explain or to predict political action. It has usually been defined as the set of propensities or orientations toward action in a given population, propensities which would include affective, evaluative, and cognitive or rational problem-solving components. It is intriguing that Rogowski found it necessary to eliminate the rational component from the political culture approach, tested it on what was left of the definition, and then proceeded to make out the case for the superiority of the rational choice approach.

He also rejects as weak legitimacy theory the approach suggested in the work of Easton and Gurr, which stresses the effects of performance on political support. He attributes to these theorists the view that political system support varies with performance success and failure. He criticizes this work again as weak theory, since output failure includes such a large number of variables. In attacking political culture and political system theory as "weak theory," Rogowski is undoubtedly correct. Where he goes wrong, however, is in the claim that rational choice theory is "strong theory." A careful reading of his own hypotheses and formulations demonstrates that he generally hedges his claims, and where he doesn't hedge his claims, a careful consideration of some of his hypotheses suggests that he ought to have hedged them.

As an example we might take his first hypothesis: "The more important governmental decisions are to members of the society—i.e., the greater the utility that they attach to influencing these decisions—the more likely are perceptions of rational illegitimacy to lead to overt actions against the existing government; and the more serious the loss of legitimacy (i.e., the lower the legitimacy quotient) in a given society, the more likely is overt action against the existing government" (p. 45). The second German Reich had a life of almost 50 years. In Rogowski's terms both the German middle classes and working classes in the course of this history should have viewed the political arrangements of the Reich as illegitimate. Bismarck succeeded in buying off the dissatisfaction of the middle classes with their political power by giving them the satisfactions of national power and of material interests. Similarly in dealing with the working class, Bismarck was successful in buying off dissatisfaction with the political arrangements by welfare outputs and national power and glory. We must keep in mind that Rogowski limits rational legitimacy to the relationship between internal social structure and governmental and political structure. Interest in national security, glory and power are not included in his model of rational choice, just as the willingness to exchange political influence for economic and social benefits is not included in his model. These are very serious weaknesses in his theory, indeed. War and the threat of war have been and continue to be the very stuff of political life, and bargaining and logrolling are the common currency of exchange between political elites and their citizens and subjects. In his search for parsimony Rogowski has presented a very narrow model of rational choice. He is asking us to assume that international issues have no long-run salience, and that in the long run all that is involved in the exchanges between elites and citizens is the structure of political arrangements. One would have to ask Rogowski as he asks of the political culture and systems theorists when he argues that internal social structure in the long run wins out, How long is the long run?

It is surprising that Rogowski fails to deal with Max Weber's sociological theory of legitimacy. It is evident from references here and there in the book that Rogowski is familiar with the works of Weber. Indeed at one point he cites with approval Weber's approach to ideal type analysis, which contains the caution that real social action inevitably consists of a mixture of ideal types. Weber's theory is surely the most widely known approach to legitimacy in the social science literature. It would seem to have been elementary for Rogowski, in presenting his rational choice theory of legitimacy, to have contrasted it or to have related it to Weber's legitimacy concept. Weber's threefold classification of types of legitimacy—the traditional, the rational-legal, and the charismatic—are presented as the three different bases for according legitimacy to authority on the part of followers. People accept authority out of custom and long habituation, because it is based upon law and operates within the framework of law, or because of a belief in the extraordinary gifts, powers, capacities, of a leader. What position does Rogowski take on the Weberian approach to legitimacy? Would he thrust it aside as being so comprehensive that it is no theory at all? Doesn't a serious reader have a right to expect that an advocate of a new theory of legitimacy should confront the principal competitor in the literature and relate the new theory to the old one? Would it be unfair to say of Rogowski's work that he has disposed of competing approaches either by caricaturing them, or by failing to consider them at all?

Thus the principal criticism of this first important work of an obviously original and talented political scientist must be that he has couched his creative contribution in the form of a polemic. This reader would predict that as Rogowski goes beyond seeking illustrations of a confirming, reality-testing sort, he will have to come to grips with the explanatory shortfall of his theory of rational legitimacy, and plug himself in to the complexities of the relationships between political culture, political socialization, system performance, and types and degrees of support. Somewhere in this set of explanations of why people accept the regimes they do, Rogowski's rational choice model will prove to have some important explanatory power.

GABRIEL A. ALMOND

Stanford University

For a New Liberty. By Murray Rothbard. (New York: MacMillan, 1973. Pp. 327. $7.95.)

This book synthesizes an advocacy of Lockean rights to life, liberty, property, and defense, an appeal to the free market as the most efficient and decentralized "social" device for the allocation of resources, and a sociological and historical analysis of the State as being inherently aggressive and exploitive. The product of this synthesis is Rothbard's free market anarchism.

Professor Rothbard begins his synthesis with a Lockean state-of-nature theory according to which each person by nature possesses moral rights to life and liberty and each person may, furthermore, acquire rights to goods and to the fulfillment of specific contracts. Each individual also possesses by nature a right to defend himself and to punish aggressors. Individuals are morally free to authorize various defense agencies to exercise their respective rights to defend and punish. Everyone might freely give authorization to the same agency; but such unanimity, should it ever occur, would be short-lived. Some persons would remain contractually free to switch to resurgent competing agencies, and new customers for defensive services would be constantly entering the market. In fact, unanimity in authorization has never occurred and is exceedingly unlikely ever to occur. The divergence of persons' preferences with regard to quality, certainty, range, speed, and style of legitimate defensive services virtually insures a multiplicity of suppliers. Under a system of free purchase of defensive services, resources will tend to be devoted to defense in general and specific defensive activities in particular in proportion to the relative market value of these activities. Taxation is replaced by freely contracted payments.

Each legitimate defense agency, as a legitimate agency, restricts its activities and the activities of its competitors to defensive, restitutive, and retaliative measures. To reduce the risk of unprofitable conflict, such agencies reach agreements about standards of evidence, arbitration procedures and so on. These risk-reducing agreements increase the value for each individual of being the client of one of the participating agencies. Aggressive, "bandit," agencies survive only to the extent that the demand for injustice is high, relative to the demand for justice. Furthermore, there exists no state mechanism or reverence for the state to be captured or used by political power-seekers. Collectively, the legitimate defense agencies sustain the reign of laissez-faire. There are no coercive barriers to competition—even competition among those who provide protection from coercion.

In contrast to legitimate defense agencies, States are conceived as inherently committed to robbery and violence—to the "political means" rather than the "economic means" of the free market. The State is a device for plunder, and its existence calls forth two classes: the net tax-payers and the net tax-consumers. State activity is not simply a poor means of resource allocation and a necessary violation of moral rights, but also a struggle to obtain and exploit positions within the tax-consuming class and to cement the existence of this class-generating system. Rothbard argues that State activity in, e.g., education, welfare, economic regulation, and foreign affairs, repeatedly commits the tripartite sin of violating moral rights, misallocating resources, and contributing to a system of oppression and exploitation. Indeed, legitimate defense agencies are so relatively pacific and socially nonintrusive that Rothbard's incidental claim that a large area (e.g., North America) could survive as a free market anarchy even while nation-states like the Soviet Union continued to exist, forces Rothbard to embrace a surprisingly sanguine view of the dispositions of the leaders of *these* States. Lockean rights, of course, are not limited to economic or property rights. Rothbard inveighs against the attempt to separate "property rights" from "human rights." Property rights are human rights, and private property is a necessary condition for the exercise, as an individual right, of any traditionally acknowledged human right, e.g., the right to assembly. All defense agencies must refrain from, and may defend against, any violation of rights, e.g., those involved in conscription, compulsory psychiatric commitment, censorship, pollution, and all victimless crime laws.

Although *For a New Liberty* is written more as a manifesto than as a treatise in political theory, this does not wholely excuse its theoretical shortcomings. While it is clear what rights Rothbard claims for men, the arguments which he provides for men's having these rights are not compelling. Among these arguments, however, the argument for natural self-ownership is the most interesting and deserves further development. Both utilitarianism and subjectivism are rejected. And this rejection is alleged to leave three essential candidates for our fundamental political principle: by nature some persons, but only some persons, own others; by nature everyone has part ownership in everyone; by nature each person is the owner of his own person. The first is dismissed since it presupposes natural moral inequality. The second is dismissed since if it were true no human action could ever be morally permissible. For any action, to be permissible, would have to have universal prior consent. Thus, Rothbard affirms the natural self-ownership alternative. Rothbard's invocation of the "Libertarian Code" as the complete moral guide for defense agencies is too bold. His unfounded assumptions are that Lockean state-of-nature theory can provide, and needs to provide, a complete specification of the moral permissibility of any particular action. Still, this book's considerable strength is to be sought and found in its powerful unification of Lockean, anarchist, laissez-faire, and anti-Statist theories and in its employment of this unified view. The current revival of natural rights and state-of-nature theorizing may furnish the detailed philosophical underpinnings which are not provided here.

ERIC MACK

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Nonviolent Power: Active and Passive Resistance in America. By Judith Stiehm. (Lexington, Mass.: D.C. Heath and Co., 1972. Pp. 132. $2.95, paper.)

Professor Stiehm believes that nonviolent resistance is a promising method of social change but that its promise is unlikely to be realized until its use is better understood. Hence she seeks to clarify the nature of nonviolent resistance so that those who use it can do so more skillfully and more successfully—and also so that those who do not use it can respond intelligently to its use by others. This essay on nonviolent power is thus addressed to all of us, as citizens. It is not, however, a "how to" manual but an analytical study which falls squarely within the domain of political theory and provides an instructive introduction to the theory and practice of nonviolent resistance in America.

Stiehm distinguishes two quite different forms of nonviolent resistance, an individual form and a group form. The first assumes the individual value and basic unity of all persons and asserts that nonviolent resistance, as part of a total individual way of life, is the appropriate and indeed the only way to disclose mankind's unity and eliminate conflict. The second assumes the inevitability of social conflict and asserts that nonviolent resistance is a practical and economical way of managing conflict to secure social peace and social justice. The first emphasizes the purity of individual conscience and the power of individual example. The second emphasizes "the intelligent development of power through deliberate, disciplined, collective action" (p. 81). Stiehm systematically explores the ramifications of each. Her sympathies and, one suspects, her hopes lie with the strategic practice of group nonviolent resistance. But she points out the high individual costs of both and advocates the use of neither.

The value of the study lies primarily in its perceptive analysis of these two forms of nonviolent resistance. It is also, I think, important because it takes nonviolent resistance seriously as a mode of political participation. It is not, however, completely successful in explaining when nonviolent resistance can work. Stiehm shows clearly that the two forms she delineates are so opposed that they cannot be combined in practice, which means that effectiveness requires strict adherence to one or the other. She thinks they can nevertheless be tied together theoretically, by asking *who* can best use *which*. She argues that the individual form is appropriate for members of a society's elite, who are individually powerful, while the group form is appropriate for members of the mass, who can be powerful only through collective action. The suggestion is intriguing, but I do not find it wholly convincing. It would seem to me that individual nonviolent resistance as part of a way of life *must* flow out of a set of convictions which are quite unrelated to one's position in society and which cannot easily be the product of tactical considerations. This reservation aside, the book gives us an illuminating look at a timeless and hence timely subject. And it contains an excellent bibliography on nonviolence and related topics to serve as a basis for further study.

KAREN JOHNSON

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Conflict Behavior and Linkage Politics. Edited by Jonathan Wilkenfeld. (New York: David McKay Co., 1973. Pp. 388. $12.50, cloth; $5.95, paper.)

Conflict Behavior and Linkage Politics, edited by Jonathan Wilkenfeld, is a collection of 11 essays which attempt to deal with the notion that the domestic and foreign conflict behaviors of nations are related to each other in some way. These essays represent yet another attempt to investigate whether, under certain conditions of domestic instability, the leadership of a nation-state will engage in foreign conflict behavior in order to divert the attention of the population and thereby reduce or postpone domestic upheavals. These essays yield findings contradictory to those of R. J. Rummel (1963), who found no linkage between domestic and foreign conflict. Wilkenfeld sums up the findings reported in these articles as follows: "Conflict linkages do in fact exist, and they take on various forms, depending upon (1) the type of nation under consideration, (2) the type of conflict behavior involved, (3) the type of temporal relationship, and (4) the conflict situation and context" (p. 4).

The book is divided into three parts. The first part focuses on the meaning of linkages. It includes an excerpt from Lewis Coser's *The Function of Social Conflict* and an article by James Rosenau entitled "Theorizing Across Systems: Linkage Politics Revisited." Although these two eminent scholars posit important analytical perspectives, the second part of the book (i.e., the empirical part) does not make much use of these contributions.

The eight articles in Part Two are quite provocative. Rummel's 1963 article is reprinted, and Wilkenfeld's "Domestic and Foreign Conflict" article, which challenges Rummel's work by controlling for the *type* of nation involved, is a well-written and thoughtful piece. Warren Phillips's "The Conflict Environment of Nations; A Study of Conflict Inputs to Nations in 1963" starts from the simple hypothesis that foreign conflict behavior results from a stimulus-response type of interaction, and via factor analysis, canonical regression, and stepwise regression, Phillips finds that there is a direct and strong relationship between what a nation sends out and what it receives—e.g., war brings war, diplomatic notes bring diplomatic notes, etc. The domestic stress measure was found to be useful in explaining residuals.

In "Externalizing Systemic Stress: International Conflict as Adaptive Behavior," Leo Hazelwood pursues Phillips's idea a step further, conceptualizing foreign conflict behavior as one of a number of various responses a system may make to internal stress. Hazelwood's analysis, canonical correlations, and path analysis lead him to conclude that significant relationships exist between some domestic and some foreign clusters of national attributes and behavior. Michael Haas focuses on the dynamics of political development as a cause of external conflict, since development produces a need for unity and fostering of a sense of national identity. Using both factor and cluster analysis, he concludes that "it is the role accorded to the military that is crucial in accounting for wars. As a country develops, new societal problems may—if mishandled—lead to wars between nations" (p. 207).

Richard Van Atta, John Collins, Robert Burrowes, and B. Spector are other contributors to Wilkenfeld's volume. Whereas Van Atta and Collins do find some significant relationships between domestic and international behavior, Burrowes and Spector's study of Syria leads them to conclude that their results "uniformly brought into serious question the commonly held notion that the two domains are *strongly* related" (reviewer's emphasis).

Finally, in a limited third part, there is one article by Wilkenfeld and Dina Zinnes, "A Linkage Model of Domestic Conflict Behavior," which tries specifically to assess the foreign-domestic conflict behavioral links. The authors construct a Markov process as the means for looking at the question, and find that while the more intense forms of domestic conflict—e.g., internal wars—are generally unaffected by foreign conflict behavior, there are a number of significant ties among the less intense levels of domestic conflict and foreign conflict behavior.

In general, the volume is well organized, with the methods and substance of the articles following each other sensibly. Evaluating the substantive merit of the book, however, depends, as always, on the perspective from which one approaches it. For the quantitatively oriented political scientist, the book presents a wide variety of sophisticated methodological techniques employed to study an important substantive area. There are, of course, areas for methodological debate, such as data making and measurement. But such questions need to be raised, so this should be a thought-provoking volume for those interested in these kinds of questions.

The nonquantitatively oriented political scientist, however, will probably see things somewhat differently. The almost universal reliance on factor analysis to produce the variable clusters for the final models can leave one with the distinct impression that the studies were undertaken with only the very basic notion that domestic and foreign conflict are somehow related, but with little idea of how that relationship might operate or why it would exist at all. It can be argued that there should be some theoretical reason for hypothesizing that any given variable belongs in a model rather than just plugging a data-set into a factor analysis program and using the clusters which a program produces.

Another and perhaps more serious question about the relevance of the studies can be raised. Surely no one would argue that it is irrelevant to study conflict behavior, but the real question is whether it is useful and relevant to study the problem in this particular way. If one argues that enough is now known about conflict behavior so that all its major sources can be isolated, measured, and used in a comprehensive model, the aggregate data analysis of the type employed in the present volume will seem eminently useful. If, however, one believes that much is yet to be uncovered, that there are many potential ways in which conflict situations can arise, and most important, that some of these findings seem contradictory, one may entertain doubts about this approach. In spite of these objections, I find the book so useful that, although it was not designed as a text, I recommend it as a reader in a course on conflict behavior.

EDWARD E. AZAR

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The Law of Freedom in a Platform. By Gerrard Winstanley. Edited by Robert W. Kenny. (New York: Schocken Books, 1973. Pp. 160. $12.00, cloth; $2.95, paper.)

Gerrard Winstanley was a leader and the principal propagandist of a band of radicals during the English civil war. On April 1, 1649 he and others began the best known 'Digger' community by squatting on St. George's Hill, Walton, Surrey (less than twenty miles from London). There they cultivated the earth, holding that God—i.e., Reason—had given the earth to be a common treasury.

The *Law of Freedom in a Platform* is Winstanley's most systematic exposition of the Diggers' program, but it is also the voice of the ghost of the Digger movement, published almost two years after local hostility had evicted the colony. In it, Winstanley appeals to Cromwell to establish a true commonwealth by doing away with "kingly power." For Winstanley, who had come to digging by a religious vision, the defeat of the king and the establishment of a republic provided the opportunity to crush the Red Dragon, his symbolic figure for all evil, including inequality, domination, conflict, and buying and selling. True freedom would be established in a (literal) commonwealth where all would share in the earth and its fruits, drawing from stocks to which all would contribute by work on the land and in trades. Private property in land, power over other men, hiring, and buying and selling were all based on conquest—in England on the Norman Conquest which had now been overturned. In Winstanley's utopia, none would beg and none would live idle; lawyers and clergy would be no more.

The *Law of Freedom* goes beyond earlier Digger tracts by outlining a scheme of government and law. Winstanley planned to retain the patriarchal monogamous family as the basic unit in his society. In each parish, males over twenty would elect Peace-makers who would deal with conflict in the first instance. There would also be Overseers to supervise agriculture, the various trades and the storehouses, Taskmasters in charge of those sentenced to servitude for serious crimes, and a Postmaster to collect the local news and send it to a central office for publication. In the *Law of Freedom*, Winstanley, perhaps toughened by his experiences, provides for a local and a national armed force and even for the death penalty for the incorrigible. To regulate the localities there would be a County Senate composed of the local peacemakers and overseers presided over by a judge, and above that a national Parliament. All these officials would be elected each year, thus ensuring that they would never become proud and oppressive. The government would have a mildly gerontocratic flavor, since those eligible for office would be males over forty. Indeed, Winstanley proposed that those forty and over be relieved of compulsory work to be roving overseers.

Winstanley's religious establishment is perhaps the most curious part of his scheme. In place of a hierarchical church establishment supported by tithes and inculcating a spiritual religion, Winstanley would have an annually elected ministry reading the nationally circulated news, and, like the ancient Israelites, reading the laws to the people. Winstanley's god, the spirit of the world revealed in creation, would be worshipped by educational discussions on the day of rest.

It is an impressive scheme, both for its moral commitment to equality, liberty, and toleration and for its elaboration; far more comprehensive than the Levellers' proposals, it approaches the detail of Harrington's *Oceana*, published four years later.

The text is a facsimile of the version in Sabine's edition (1941) of Winstanley's works. It will inevitably be compared with Christopher Hill's recent edition, which includes other writings of Winstanley as well as the *Law of Freedom*. Kenny's edition is printed on more substantial paper in larger type. Moreover, the careful scholarly introduction is a useful essay on Winstanley. In it, Professor Kenny tells what is known about Winstanley's life, sketches the history of the Diggers, and briefly sets that movement in the traditions of sectarian protestantism, peasant revolt and mid-seventeenth-century radicalism. Professor Kenny has decorated this edition with contemporary woodcuts showing agricultural implements and tasks. Perhaps these will serve to remind readers that agriculture was the occupation of the overwhelming majority of the population and thus underline the realism and the significance of Winstanley's digging.

M. M. GOLDSMITH

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American Students: A Selected Bibliography on Student Activism and Related Topics. By Philip G. Altbach and David H. Kelly. (Lexington, Mass.: Lexington Books, D.C. Heath and Company, 1973. Pp. xiv, 537. $25.00.)

That American students have achieved unprecedented involvement in political and university affairs during the last decade or so is a fact no longer open to serious question—although it is a phenomenon both praised and condemned by scholars and laymen alike. Serious research related to this involvement did not occur much before 1960; yet the literature now available is burgeoning. This becomes especially clear upon examination of this volume—a volume which furnishes an admittedly selected, yet superbly comprehensive bibliography of materials concerning student involvement in American politics, higher education, and related fields.

This bibliography (a revision of a 1968 publication by Professor Altbach) includes both readily available, and some more difficult to obtain, items—"more than 9,000 separate listings" (p. xiv). The compilers inform us that "nothing published after November of 1972 is included" (p. xiv). Also excluded are, among other things, unpublished works and articles which appeared in the more transitory student journals.

Organization of the volume is simple and logical; hence it may be easily used. The listings may suffer somewhat from lack of annotations, but this minor shortcoming is offset by an excellent introductory essay in which some of the more significant items are discussed in greater detail. Virtually all political and scholarly viewpoints are represented in the bibliography.

Theoretical understanding of student activism will come only after much more work has been done; indeed, the number of variables which need examination may preclude full knowledge of the subject. Yet, the bibliography by Altbach and Kelly will provide a highly valuable tool for serious study of the subject. Careful perusal of the volume will put one in touch with most of the significant literature of the field.

EARL R. KRUSCHKE

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Convention Decisions and Voting Records. Second Edition. By Richard C. Bain and Judith H. Parris. (Washington, D.C.: The Brookings Institution, 1973. Pp. x, 470–partially unnumbered. $14.95.)

Scholars and journalists dealing with presidential nominating conventions will welcome the second edition of *Convention Decisions and Voting Records.* Adding descriptions of the last four sets of party conventions to the first edition's history of conventions from 1832 to 1956, and updating the brief introductory chapter to reflect recent developments in primaries, judicial regulation, media coverage, and convention size, the new volume provides readers with a concise yet sufficiently complete reference work that is extremely well written and–for such a book–often entertaining.

Bain and Parris treat each convention methodically, providing first a short summary of its political background and then recounting the debates and decisions on credentials, organization, rules, platform, and nominations for president and vice-president. Through a balanced selection of quotations and anecdotes, and some speculation on motives, strategies, and tactics, the authors enliven what could easily have become a dull recital of facts and figures. For all but the last four Democratic and Republican conventions, their analyses of "key votes" point out correlations between state alignments on procedural votes and their balloting for presidential nominees. Three appendices round out the book: presidential and vice-presidential nominees; convention officers; and nearly complete state-by-state balloting records. (The latter appendix employs sampling for conventions in which there were ten or more votes.)

Few will want to read the book from cover to cover. But even a brief perusal reveals that the many squabbles of recent years concerning convention size, apportionment of delegates among states, credentials challenges, and the unit rule–as well as the usual shenanigans and power plays–are nothing new at all. Some of the earliest problems of conventions dealt with size and apportionment, with the number of presidential electors in a state generally used to determine the state's allotment of delegates. Over- and under-representation–with the consequent question of who shall cast a state's votes–were perennial problems. Delegates sometimes were banned; and, at least once, others forced their way inside.

In a sense the book is a study of convention tactics: challenging delegates' credentials; using procedural arrangements to enhance or reduce chances of success on substantive issues; scheduling of important votes; and so on. In the limited space available, Bain and Parris do a fine job of relating national political issues to convention politics, and convention behavior and procedure to candidate selection and platform specification. The book is a constant reminder of how lively presidential nominating politics at the conventions themselves used to be, at least relative to the last twenty years–and, given recent Democratic party rules changes, it may be a harbinger of sorts of how exciting future conventions *might* be. One cannot help but notice also how frequently parties in the past turned to second or third choice candidates with "sound" records but without strong positions on the day's burning issues (Polk, Pierce, Buchanan, Garfield, Hughes, Harding).

Since the book is meant to be a reference work, and since other publications in the Brookings Institution's Studies in Presidential Selection series reach conclusions and make recommendations, Bain and Parris do not include what would, no doubt, be a most useful chapter of their own reflections and opinions. In a sense, that is unfortunate, for their narrative is rich in connotations and lessons. For example, would it prove beneficial for today's conventions to revert to the earlier practice of nominating candidates before trying to write a platform? Will recent court decisions force a return to the original practice of basing delegations exclusively (or nearly so) on a population criterion rather than on some form of loyalty bonus? These and dozens of other questions can be found in and between the lines of this useful volume. It is by no means comprehensive, and serious scholars will have to go beyond what is contained in its pages. Indeed, one improvement in the next edition would be to furnish at least a partial bibliography for each convention and to include references to scholarship bearing on presidential selection in that year. But as a single reference source, the second edition of *Convention Decisions and Voting Records* certainly is worth the purchase price and is a "must" for libraries and for those who routinely need to refer to the politics and decisions of past conventions.

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The United States Army in Transition. By Zeb B. Bradford, Jr. and Frederic J. Brown. (Beverly Hills, Cal.: Sage Publications, 1973. Pp. 256. $10.00.)

U.S. Reserve Forces: The Problem of the Week-End Warrior. By Martin Binkin. (Washington, D.C.: The Brookings Institution, 1974. Pp. 63. $1.95, paper.)

These two books should interest both specialists in military science and social scientists studying organizational change. Bradford and Brown believe that the controversy surrounding American goals in Vietnam has obscured evaluation of the means used to reach these goals. They argue that "technology has radically changed the dynamics of the battlefield" (p. 61). Therefore, America's future success in armed conflict depends on the armed forces' recognition that it possesses a superior technology (e.g., in communications systems) and that its soldiers are urban and educated and thus not prepared for guerrilla warfare. These characteristics permit mobility and, hence, flexibility that allow policy makers to exhaust all other means before committing ground forces.

Disenchantment with Vietnam and the low profile subsequently adopted by the Army has led to the establishment of an all-volunteer force and, concomitantly, to a drastically reduced level of manpower. In addition, requirements for America's participation in NATO have further drained the manpower available for a flexible response to various kinds of conflict in other geographic areas. In chapter 5, Bradford and Brown argue that both the experience in Asia and great changes in Europe suggest the need for a different organization and operation of the Army. Among other things, they suggest changes in the organization of units, rotation, location of forces and so forth. They spend an entire chapter discussing their belief that an all-volunteer force be backed by a reserve. They examine the difficulty of attracting volunteers for the reserves and then suggest changes in use of the reserves that might make service more attractive. Throughout the book, the authors assume that World War III is a possibility. However, since they believe that the Army should be designed for the *most likely* contingencies, they also argue that preparations for World War III are unrealistic.

Chapter 7 restates the need for the army to be a flexible instrument of foreign policy, this time emphasizing that other nations be seen not as either allies or enemies, but rather as entities with different characteristics and potential. To achieve this perspective they recommend, among other things, educational careers for officers that emphasize international affairs less and comparative defense policy and world economic geography more strongly. In discussing the effects of flexibility on costs, they point out that the army's budget is relatively constant while the cost of personnel is rising rapidly. As a result, there is a great "need to find ways of using less manpower, and of using it more efficiently" (p. 131). Unfortunately, using less manpower requires using more equipment which, in turn, "demands more sophisticated operator skills which must be better paid" (p. 135). Thus, it becomes imperative that nonessential bases be abandoned, and favored but dispensable programs no longer be funded. What the authors are implying is that persons and organizations with vested interests must set them aside and let military professionals determine the organizational structure most favorable to flexibility. The authors conclude that the reorganization they advocate is well within the range of likely funding levels.

In part III the authors argue, quite effectively, that the Army should respond to social change not as a leader but as a follower. They then present guidelines for response that are, unfortunately, unusually vague. For example, how do they intend that a concept such as "individual acceptance of responsibility" be translated into policy? Among their more practical recommendations are a condemnation of unions and a recommendation for decentralization. Significantly, these authors recommend adoption of a regimental system; an experiment with such a system is presently in progress and may eventually be adopted, if not by the whole army, at least by large parts of it. Many of the suggestions have been proposed before: reform of the army's educational system, lateral entry into the officer corps, reduction in the length of the chain of command.

Like Bradford and Brown, Binkin also argues that the reserves are not usefully integrated into the armed forces and that reorganization is needed to reduce the size and cost of the reserves. Most of this short monograph describes a possible reorganization, whose merits would be better evaluated by a military specialist than by a social scientist.

To me the most interesting points, but also the least developed, involve Binkin's hunches as to why no reorganization has yet occurred. The traditional rivalry between the active and the reserve forces is an obvious factor. More important, though, is the reserves' grass-roots support, which allows reservists to influence decisions that affect their welfare. This influence is bolstered by an active, effective lobby. Opposition from these sources make it potentially dangerous for either military professionals (whose motivation would automatically be suspect) or politicians (who might later need assistance from the lobby) to suggest reorganization. Consequently, the reserves have been able to prevent changes in the status quo. Unfortunately, Binkin does not develop the specifics of this theme. As a result the reader must view his facts, figures and recommendations with the suspicion that political considerations will continue to block major changes.

Both books leave me with a disturbing sense that the foreign-policy objectives military organizations supposedly serve are *not* necessarily established by political leaders. Conversely, political pressures improperly influence military decisions. Thus, it is difficult to harmonize military objectives and organizational arrangements. Certainly, organizations must deal with their environment, but military organizations seem pressured to an extent probably unknown by other kinds of organizations. It is no wonder, then, that military professionals are often deeply frustrated.

Neither of these books makes a major contribution to the study of the military. Nevertheless, both remind social scientists that the study of military organizations can provide valuable data with which to test old theories and formulate new and broader ones, especially on the topic of organizational change. The data and the theories should interest not only military professionals but social scientists. That the authors discuss important themes, write readably, and defend their ideas convincingly makes the books even more valuable.

MAURICE GARNIER

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Thomas Jefferson: An Intimate History. By Fawn M. Brodie. (New York: W.W. Norton, 1974. Pp. 571. $12.50.)

From the time of its publication, *Thomas Jefferson: An Intimate History* has been the object of lively crossfire. The debate between Fawn M. Brodie and her colleagues within the field of history has focused on whether the book represents a "serious" contribution to that discipline. Relatively little attention has been paid thus far to this work as a study in the intellectual genre of "psychohistory."

Those who are skeptical in principle about the application of theories of personality to historical personages will question the usefulness of Brodie's book in an attempt to discredit the validity of psychological analyses in the study of political history and biography. Yet, one need not be unsympathetic to personality and politics research—one may even be aware of the growing attempt to increase the rigor of political psychology—to have grave reservations about this book.

Professor Brodie is to be commended for her willingness to explore regions other Jefferson biographers have been reluctant to enter. Nevertheless, her book suffers from several flaws which prevent it from taking its place beside the classic works in the field of psychobiography as well as the standard historical analyses of Jefferson. These flaws include the disproportionate amount of space devoted to Jefferson's supposed liaison with his quadroon slave Sally Hemings, the absence of new and reliable evidence on this topic, and the lack of attention paid to forces other than personality that could account for Jefferson's behavior (such as the political climate in which he operated and the philosophical views he harbored). In addition, Brodie's methodological focus, especially as she draws upon Erikson, is open to criticism from within the circumference of her methodological approach—the application of psychoanalytic theory to history and politics.

Brodie's thesis concerning the Hemings affair is that Jefferson took a slave as his concubine while serving as Ambassador to France, that he sired five or six children born to her in the course of a relationship which lasted some 38 years, and that his increasing ambivalence toward the abolition of slavery was attributable to his reluctance to be separated from or having to acknowledge the existence of his "second family." As evidence of the alleged liaison, Brodie cites a memoir by one of Jefferson's supposed slave children, freed in his will; the political diatribes of Thomas Callender, a disappointed office seeker; and Jefferson's meticulously kept farmbook, which she finds significant for its deletions as well as for its contents.

It should be pointed out that all the incriminating evidence Brodie draws upon was known to scholars long before the appearance of her book. Winthrop Jordan (in *White Over Black: Attitudes Toward the Negro, 1551-1812*, University of North Carolina Press, Chapel Hill, 1968) concluded from the previously mentioned farmbook that in the case of four or five of Sally's children, Jefferson was at Monticello nine months prior to each birth.

To back up her assertion that Jefferson's involvement with Hemings was an "affair of the heart" and not one of exploitation, Brodie provides no documentation whatsoever, although such an assumption is a crucial link in her thesis. Even if one grants that the double meanings she finds in Jefferson's journal and letters represent admissions of the liaison, they could just as readily suggest a relationship involving less commitment than the one she believes took place.

I do not feel that Brodie's depth psychological speculations and symbolic interpretations come any closer to the truth than the blanket denials of a possible Jefferson-Hemings connection on the part of scholars she refers to as the "Jefferson Establishment." Indeed, one such member, Douglass Adair may have come closer to unveiling the true nature of the Hemings affair as well as the contradictory behavior and motivations of the participants without introducing personality theory to compensate for the lack of reliable evidence. (See Adair's "The Jefferson Scandals," in *Fame and the Founding Fathers,* edited by Trevor Colbourn, New York, W.W. Norton, 1974.)

After weighing the scant evidence available, Jordan cautioned his readers that "paternity can neither be refuted nor proved from the known circumstances or from the extant testimony of his (Jefferson's) overseer, his white descendants, or the descendants of Sally, each of them having fallible memories and personal interests at stake." Brodie leaves no doubt as to where she stands and takes on the tone of a partisan when she confronts the various denials which have been put forth.

Having considered two alternative hypotheses, Jordan dismissed the factual question of whether the liaison occurred as insolvable and of little importance to the understanding of Jefferson. He preferred to attribute Jefferson's ambivalence toward emancipation to conflicting intellectual opinions he held about the nature of blacks. While this explanation may not be definitive, Brodie's interpretation fails to take into account the transformations that had taken place in the southern economy in the years between 1776 and 1826 as well as Jefferson's conception of his role as a politician and elder statesman, and the similar vacillations of many of his contemporaries.

To have written a successful psychobiography, Brodie would have to had to base her explanations of Jefferson's public and private behavior on more long standing personality and situational variables than the "intimate history" he is said to have led after the age of 44. Throughout the book, a fundamental tension exists between what Brodie regards as her subject's "inner life" and "intimate history." She shows herself to be sensitive to the difference between the two in the foreword and even attempts in the opening chapter to see "intimate history" as the result of his "inner life." With the introduction of the Hemings speculations, however, the causal connection comes to a grinding halt. Thus Jefferson's behavior and policy pronouncements after 1787 are seen as a product of his "intimate history" alone rather than of forces which shaped it.

This tendency causes Brodie to overlook the linkages between many of Jefferson's actions as President and his character and worldview, both of which had been pretty well formed long before he is said to have entered into the relationship with Sally Hemings. Instead, Brodie devotes much attention to the issues of Jefferson's presidency that centered around personalities (such as the rift with Adams, the Burr conspiracy, and rumors of miscegenation). Issues that continue to fascinate students of the presidency (such as the Louisiana Purchase, the Embargo, and Marbury vs. Madison) are crammed into the fewest number of pages as possible.

When she does look for a causal connection between Jefferson's character and political behavior, Brodie applies Eriksonian theory without inquiring whether the circumstances she confronts are similar to those described in "Young Man Luther" or whether her subject exhibited personality traits Erikson's possessed. A case in point would be Brodie's discussion of Jefferson's "A Summary View of the Rights of British America." As suggested by its title, the "Summary View," which Jefferson wrote in 1774, did not embrace the cause of total independence from the mother country. Brodie, however, sees similarities between this document and Luther's "Ninety Five Treatises," which historians generally view as a more radical declaration. In her haste to draw comparisons between Erikson's Luther and her Jefferson, Brodie suggests that the "hostility" Jefferson expressed toward the crown (actually the king was being asked to alleviate parliamentary injustices) was initially intended for one or both parents. To say the least, this assertion is based not only upon a faulty comparison of Jefferson and Luther and their parents, but of their standing as "revolutionaries."

Unlike Luther, Gandhi, and Lenin, all of whom Brodie likens to him at some point, Jefferson was not viewed by his contemporaries as *the* leader of the revolution in which he participated. He was not regarded as the leader of any movement prior to the birth of the party system in the 1790's.

On the whole, Professor Brodie's study would have been a more useful contribution to a growing field of research had she been more skeptical about the ability of clinical insight to compensate for the lack of evidence and more cautious about applying Eriksonian analyses to situations that are not analogous to those to which they were meant to be applied.

ALVIN STEPHEN FELZENBERG

Princeton University

Collective Bargaining Comes to the Campus. By Robert K. Carr and Daniel K. VanEyke. (Washington, D.C.: American Council on Education, 1973. Pp. 314. $9.00.)

With a title reminiscent of *Mr. Smith Goes to Washington, Collective Bargaining Comes to the Campus* might well be preemptorily adjudged "simplistic," and passed over by most political scientists. That would be a mistake, for the book treats authoritatively a subject of vital academic interest. Its theme is the effect on academic self-government of faculty unionization; its conclusion is that faculty responsibility, autonomy, and rights of dissidence may be threatened, but that nevertheless the new form of government may be beneficial to the faculties of some campuses.

Faculties were brought under collective bargaining laws only in the late 1960s by state laws and a decision of the National Labor Relations Board. Collective bargaining laws *presuppose* and regulate *conflict* between the interests of management—those who set policies and supervise their execution—and labor. Labor is expected to have no interest other than the narrowly defined terms and conditions of employment, and management is expected to compromise all but the essential purposes of its organization: its viability and profitability. So the law regards faculties as labor without interest in the university enterprise, and the trustees and administrators as management who hold the university to its essential, especially financial, purposes.

The authors examine the benefits to the faculty as "labor" stemming from collective bargaining, and the costs they pay in their role as the buyers of the universities' integrity: They argue that the adoption of collective bargaining by a faculty is the adoption of a new university constitution which will re-form the alliances of power controlling the university's policies. Union-administration committees will replace faculty senate committees and even in private universities the administration will have, *by public law,* responsibility for hiring, promotion, and similar policies now properly regarded as faculty responsibilities.

The "faculty" as such will lose much of its functional identity. Because as labor the faculty does not make policy, there are no compelling reasons to prevent the lumping of the faculty and other laborers into the same bargaining unit. Thus units have been recognized by the NLRB in which the members of the faculty senate are less than one-quarter of the whole unit, and in which department chairmen and members of key faculty committees have been excluded. Matters most important to academics having to do with academic freedom, responsibilities, tenure, and relations with students, have already entered contracts. Collective bargaining could initiate a system of governance over these matters in which the faculty has only a minority vote.

In deciding to bargain collectively, the faculty unit must choose an agent, and frequently this has been the union which actively sought to organize the faculty. These run the gamut from the AAUP to the Teamsters'. The choice of agent vitally affects faculty interests, because the agent binds the union to the contract it writes with the administration. There is no requirement in law for ratification of a contract by the membership, although that is the general practice. Once a bargaining agent has been selected, the company (administration) must deal exclusively with that agent, and is guilty of unfair labor practices if it attempts to deal with any other agent *or to deal with the faculty directly* about matters pertaining to its contract. Matters in the contract cannot come before the senate.

Inevitably, disputes will arise over the contractual matters, for example the granting of tenure or the claim of a violation of academic freedom. Even if the initial complaint arose exclusively from a decision of an academic committee (e.g., a tenure decision), the grievance would be heard before a union-management committee. If appealed, it would go before a federal or state hearing examiner, and then a full board, and finally to the civil courts. Ultimately precedent-setting decisions regarding academic matters will be made in a setting removed from the campus and the profession, under an adversary proceeding, following the case law precedents of industrial relations.

The demands of unionization may even more strongly conflict with academic freedom. Variations of closed shop agreements can be expected to appear in colleges and universities once unions have become solidly established, just as they have in other settings. Under such contracts, employers must fire employees who do not pay union dues. The university may be required by contract to fire tenured professors who refuse to pay dues to a union with whose policies they differ, and which they have refused to support in an election. But indeed as contracts encompass more policies of general academic concern, the professor may find he must become active in the union in order to carry out his professional responsibilities.

The authors have used a case approach, drawing evidence from surveys of faculties, histories of unionization and contract fights (for which they conducted primary investigations) and analysis of academic collectively bargained contracts and proceedings under grievance petitions. They discuss the NLRB decisions by which faculties were included in collective bargaining, and examine the experience of other comparable professional groups under the law.

This review has been grimmer than the authors' presentation, in that they argue that collective bargaining has not produced the dire effects that are potential within it (nor the benefits it promises). It is a young innovation in academic governance, perhaps in some places useful, but carries serious liabilities.

THOMAS W. VITULLO-MARTIN

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Emergence of the Presidential Nominating Convention, 1798–1832. By James S. Chase. (Urbana: University of Illinois Press, 1973. Pp. 332. $8.95.)

Here is yet another work added to the conventional wisdom which holds that the Founding Fathers were opposed to parties, made no provision for them in their constitutional system, and could not conceive of a party system, let alone support such an abomination. Professor Chase uncritically accepts this view in explaining the origins of presidential nominating practices: "Neither Jefferson nor Madison, or anyone else, set out to create a party. The idea of a permanent, extralegal, and popular organization existing for the purpose of controlling the government would have repelled them in 1789 (provided they could have imagined such a novelty) and it continued to repel them after they had founded the Republican party" (p. 6).

Does no one take seriously Madison's view of party and faction in propounding the Constitution? "The regulation of these various and interfering interests forms the principal task of modern legislation, and involves the spirit of party and faction in the necessary and ordinary operations of government" (*Federalist* 10).

True, party and faction emerge from "interfering interests" which government must "regulate." And Washington, in his Farewell Address, warned against the "baneful effects of the spirit of party." But this did not differ essentially from Burke's classic defense of parties. The leading Framers—as all sensible citizens, then and now—wanted a national leadership that could rise when necessary above mere partisan interests. But like Burke and Madison, Washington also accepted party as inevitable: "A fire not to be quenched, it demands a uniform vigilance to prevent its bursting into a flame, lest instead of warming it should consume." The Constitution did not prescribe the party system, but it did more than neglect to proscribe such a development. It provided a road to power which knowingly made party organization inevitable, given political liberty. Hence, the constitutional system is so organized as to channel and contain this emerging mode of political organization. Indeed, the Framers could not foresee the modern party system, but the major characteristics which make American parties distinctive reflect the original conception of how to impose constitutional constraints upon the "violence of faction."

Many admirers of British parties have recognized (deploringly) this constitutional channeling of the American political stream (to recall E. E. Schattschneider's metaphor in *Party Government*). This is not to say that the presidential nominating convention was intended by the Framers or anticipated as such. And, certainly, modern mass-mediated electioneering organizations were not part of the constitutional design. But the constitutional system molded a party system with distinctively American aspects: organizational decentralization; undisciplined membership, whether of office holders or of general activists; moderate differences and overlapping stands on issues between two major parties. Again, not all of these were planned, but they are remarkably consistent with the Framers' views of how to constrain a majority faction from forming too quickly when based upon narrow interests and from obtaining all the powers of government in a single electoral victory.

Chase has trouble, as have all who deal with the category, of knowing what a party is. And he does not break new ground. Like W. N. Chambers (*Political Parties in a New Nation* [New York: Oxford, 1963]), Chase hews to a narrowly modern conceptualization which then, by definition, excludes from consideration preconstitutional colonial, state, and local electoral and legislative party organizations from serious consideration. Yet that is precisely the historical and political science issue. What did the Framers know of party, and what did they believe was important to prevent (surely not free elections and the political organizations which liberty would nurture)? Combining freedom of association with electoral processes for awarding governmental authority, and given the Framers' concern for excessive partisanship, how would their constitutional system work? *Federalist* 10 met this issue head on. But Chase wrongly believes "Madison's prophecy" was thwarted by the development of modern parties. Schattschneider knew better, that it was the modern conception of "responsible parties," and not parties as such, which was thwarted by Mr. Madison's Constitution.

Similarly, Chase excludes the legislative caucus from his conception of party simply by definition. This tautology is imposed upon the analysis of the early Republican party, which is described as a "congressional faction" rather than a "broadly based national party" (p. 11). Whether "broadly based" or narrowly based, the difference between a congressional caucus and a national party is not that only the latter is national. Since each organized alignments for purposes of controlling votes both inside and outside the national legislature, why is each not equally a national party? Because one is not *modern?* The circular argument is resolved by definitional exclusion.

The national nominating system could scarcely begin until a national elective office required it (as Chase does realize, p. 6). Nor would the beginning be a clean start, with the contending factions all choosing the same organizational forms and processes irrespective of how alternatives might produce different outcomes. Thus Jacksonians could foresake the convention mode in favor of the caucus when, as in 1828, political circumstances in Virginia made that advantageous (p. 89). Both legislative caucuses and convention meetings preceded the 1831–32 national conventions on state and local levels.

This is a remarkably thorough historical work. Original data are meticulously assembled, state by state, to document each aspect and stage of the development of the early nominating processes. But, as the author is aware, the data are not self-explanatory. And his explanation is marred, in my opinion, by a faulty definition of party, so that where early development is to be noted and explained, it is rather explained away for failing to satisfy the prevailing modern view of what a "real" political party is.

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Middle East Record, Volume Four, 1968. Edited by Daniel Dishon with members of the Shiloah Center for Middle Eastern and African Studies, Tel Aviv University. (New York: John Wiley and Sons and Jerusalem: Israel Universities Press, 1973. Pp. 889. $55.00.)

This is an exceptionally useful reference work for the student of Middle Eastern politics. It is no mere "Record"—no inventory list. This volume parallels the scholarship, objectivity, and depth of treatment of a Year Book of the Encyclopedia Britannica. The only difference is that this volume of the *Middle East Record* confines itself to one year of political developments (with some attention as well to economic development) in one region of the world. Unlike Encyclopedia yearbooks, the *Record* does not bring us up to date on the state of the art in various fields of scholarship, but 889 pages on one year of politics encourage generous coverage.

Drawing upon official texts and more than two hundred newspapers and journals, often providing extensive excerpts and always citing sources, the *Record* offers articles on the following subjects: Soviet policy toward the Middle East (68 double-column pages) as well as U.S., French, British, and U.N. policies; inter-Arab relations; the Arab-Israeli conflict (with 48 pages devoted to the Arab Palestinian movement); and 385 pages altogether devoted to internal developments in the countries of the Middle East.

All articles seek to provide context for the facts or opinions cited. On controversial issues, great care is taken to cite different spokesmen or authorities within, no less than among, countries. Neither Soviet nor Arab nor Israeli attitudes emerge as monoliths; quite the contrary. Indeed, one of the great services the *Record* renders is to let the reader learn in detail just what the main issues are and how, variously, they are being perceived, argued, and documented.

Many subheadings, an ample index, and above all, careful editing and clear writing make this large volume easily accessible and serviceable.

The first volume of the *Record* dealt with developments in 1960, the second volume with 1961, the third with 1967 and its background; the present fourth volume, as noted, covers 1968; and a single volume dealing with both 1969 and 1970 is said to be under preparation. Future volumes are expected to catch up more quickly with recent years.

There is no other publication of the scope and quality of the *Record* covering the Middle East. It is an impressive achievement.

MANFRED HALPERN

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Foreign Policy and U.S. Presidential Elections, 1940–1948 and 1952–1960 (Two volumes). By Robert A. Divine. (New York: New Viewpoints, 1974. Vol. I: Pp. 353; Vol. II: Pp. 359. $4.95 each.)

Written in the narrative tradition by an historian, these two volumes provide a highly readable, often illuminating, but essentially preanalytic perspective. The author draws no general conclusions, save three largely gratuitous ones in the preface, nor does he venture any summary. Professor Divine's study is principally of interest because it assembles in one place foreign policy components of these six contiguous elections, as represented primarily by what candidates for the presidency said or did in battles for both nomination and election. Although he relies predominantly on primary documents and journalistic accounts from the period, Divine makes judicious and uniformly persuasive use of public opinion poll data. Volume I covers 1940–1948, when there were visible policy differences among presidential candidates. Volume II covers the period of the Cold War consensus in presidential elections, 1952–1960.

Divine states in the preface that he has willingly courted the "danger of distorting the political process by my deliberate neglect of the significant domestic factors that also influence the outcome of presidential elections" (I,x). He justifies this neglect of domestic factors on the highly dubious and unsupported ground that ". . . I intend only to lift international considerations out of the relative obscurity to which historians and political scientists have consigned them in modern American politics."

By his "deliberate neglect" of domestic factors, Divine assumes implicitly that the foreign policy positions taken by presidential candidates would not be differently understood if he had analyzed those positions in conjunction with domestic policy positions and the forces shaping them. If we, however, disagree with this assumption then Divine misleads us in effect when he states:

> . . . I have tried to tell the story dispassionately, letting the actors in the historical drama speak for themselves. I have no strong views I wish to impose on my reader, and I feel deeply that the attempt, especially in the last decade, to channel history along rigid ideological lines has retarded rather than advanced our understanding of the recent past (I,x).

But Divine is in fact imposing strong views on the reader. By excluding *a priori* any possible evidence of the linkages between foreign and domestic policy orientations and forces, he thereby implicitly denies conservative, Marxist, and other interpretations of the domestic sources of foreign policy. Such interpretations find in the linkages of domestic and foreign policy orientations (vis à vis capitalism, socialism, democracy and communism) the key to understanding foreign policy positions offered voters. Divine's implicit interpretation, i.e., that there were no such crucially illuminating linkages, is a serious viewpoint. Nevertheless Divine must be faulted for presuming that his approach, as stated in the above quote, is more *objective* and *less theory laden* than competing interpretations on both the right and the left. But *is* a study more objective that pretends to make an assumption-free selection of facts, yet is structured to leave out any substantial basis for consideration of competing theories? The choice of one's own theory ought to follow the discovery of the greater explanatory power of that theory compared with others. From a social scientific viewpoint Divine's work may be found naive for assuming that any study of such wide scope could proceed without explicit or implicit assumptions, which are themselves controversial, theoretically and politically.

On the positive side, these volumes provide thought-provoking particulars and analyses. In the 1940 campaign, following the fall of France and the Battle of Britain, foreign events and candidates' reactions crucially affected the polls. FDR's chances for a third term would have been far from good but for the war. Divine argues persuasively that both FDR and Willkie were able to eliminate their respective rivals for their parties' nominations largely because of public perception of their foreign policy positions. Divine demonstrates with poll data that FDR's advocacy of strengthening of American military forces and aiding the Allies by all means short of combat involvement, while stating that the U.S. should do all it could to stay out of the war, illustrates his sensitivity to public opinion. Also using poll data, Divine argues that public reaction to his forthright stand in favor of the Allies won nomination for Willkie over isolationist Republicans, and that in August 1940 Willkie would have beaten FDR. But Willkie slipped steadily in the polls as the war worsened for the Allies, for FDR showed a greater willingness to increase aid to the Allies and Willkie, according to Divine, tried to appeal to isolationist and internationalist sentiment simultaneously.

Treating the 1944 election, Divine claims (I, 169) that FDR intended a more conciliatory posture toward the Soviets than Truman chose, but he gives us no inkling of FDR's plans or perspectives. The win/loss record of known isolationist and internationalist congressional candidates in 1944 proved that internationalism had, by then, triumphed. The polls indicated that but for the war, Dewey would have enjoyed a distinct advantage over Roosevelt, who won by the smallest margin since 1916. Divine accents the McCarthyite style of anti-communism used by the Republicans as a campaign strategy in 1944, well before any apparent threat of Soviet expansionism—a strategy that spotlighted a domestic threat of communism (I, 130–137). The Republicans launched a full-scale campaign charging that

Roosevelt was the captive of left-wing labor forces and continually linked him with Earl Browder, the leader of the American Communist Party, whom FDR had released from jail. Near election day, Dewey himself claimed that Browder was directing FDR's campaign (I, 137). These facts alter the impression that anti-Communist and McCarthyite tactics in American presidential elections first sprung from consolidation of Soviet power in Eastern Europe *after* the war. But Divine's account is far less analytically grounded or historically precise on these and postwar developments surrounding the origins of the Cold War and McCarthyism than might be desired, as contrasted, say, with Nathan and Oliver's new treatment in their *The United States and World Order* (Boston: Little Brown, 1976).

In 1948, Divine shows, Truman's remarkable political recovery coincided with successfully projecting the image of standing firm against the Soviets while, via the airlift, avoiding war during the Berlin crisis. Most interesting is Divine's account of the Truman-Clifford attempt to isolate Wallace's third-party movement. In a speech in New York, Truman declared: "I do not want and will not accept the political support of Henry Wallace and his Communists" (I, 183). Divine concludes: "The final step in the Clifford strategy, hanging the Red label on Wallace and thereby enhancing Truman's appeal to all anti-Communists, had been carried out by the President personally." Divine argues that Wallace's position was that ". . . a tough line only increased the Russians' feeling of fear and insecurity that lay behing their bluster. . ." and that the U.S. should adopt ". . . a new policy that included accepting Soviet domination of Eastern Europe." Unfortunately Divine is no more specific than that on what Wallace stood for, or on why his beliefs were heretical then and are more widely accepted now.

Divine notes, "Above all, the election of 1948 marked the institutionalization of the Cold War . . . [which] meant the end of meaningful dissent on foreign policy . . . for nearly two decades" (I, 276). Consequently, the second volume, which begins with the 1952 election and ends with the election of 1960, is less thematic and more narrowly anecdotal, and hence much less illuminating. We learn that Stevenson was an early exponent of the domino theory, and was in some ways more hawkish than Eisenhower, despite the impression lent by Stevenson's emphasis on a nuclear test ban in 1956. Divine's treatment of Taft is much less clear than it could be, and he leaves us to guess whether Taft himself was ambivalent or trying to hold together incompatible constituencies. Divine tells of a secret meeting between Eisenhower and Taft in the spring of 1951. Ike offered to release a draft of a public statement renouncing all intention of running in 1952 if only Taft would agree to support the idea of collective security in Europe. Taft refused, Ike tore up the document and set out to obtain the nomination (II, 13). But Divine gives us no account of why, and over what specifically, the two men disagreed. Unlike Nathan and Oliver, Divine gives an incomplete account of how Eisenhower ended the Korean War and why Truman did not, which was so important in the 1952 election.

As a general criticism, Divine's study is weakest in failing to sharply delineate foreign policy differences among elites, as for example, Seyom Brown did so successfully in *The Faces of Power.* Thus the book's central purpose—to illuminate the effects of foreign policy on U.S. presidential elections from 1940 to 1960—is undercut to the degree that the author fails to make clear the differences in perspective and policy positions among contending forces and candidates.

JERROLD E. SCHNEIDER

University of Delaware

War and Presidential Power: A Chronicle of Congressional Surrender. By Senator Thomas E. Eagleton. (New York: Liveright, 1974. Pp. ix, 240. $8.95.)

Who Makes War: The President versus Congress. By Senator Jacob K. Javits, with Don Kellerman. (New York: William Morrow and Co., 1973. Pp. xx, 300. $8.95.)

No issue gave the framers of the Constitution more concern in their formulating a workable scheme of separation of powers than the matter of control over the war power. These works by two current members of the Senate pursue the same general themes: an analysis of the original constitutional understandings on the point; a survey of practice and precedent over the years tending toward aggrandizement of executive authority in this area at the expense of Congress; and, by way of conclusion, vigorous advocacy of the proposition that in the national interest Congress must adopt measures and practices which will check this trend and restore the original constitutional "balance."

Senator Eagleton's study is divided into two parts of approximately equal length. The first is a review of executive actions, ending with the Vietnam experience, that have involved the United States in armed conflict. In a somewhat polemical and lawyer-like fashion, he has designed his historical survey to expose the presidents concerned, especially those from FDR onward, as more or less deliberate subverters of the original constitutional plan. The second part, which is the book's major scholarly contribution, is a detailed analysis of legislative efforts in Congress during the 1970s, culminating in the passage of the War Powers Act of 1973, designed to recapture for Congress a greater share of responsibility in the war powers area. An original sponsor and prime mover of legislation of this nature, Senator Eagleton at the end found himself in opposition to the measure finally adopted. He not only voted against the 1973 Act in the form in which it came from conference committee, but he also voted to sustain President Nixon's veto of it. Senator Eagleton defends his about-face on the measure on the ground that the language finally incorporated amounts to congressional recognition that the President has the authority to engage American forces in armed conflict through his own initiative in emergency situations and to pursue that course without express congressional assent for a period of sixty days (in certain situations, for ninety days), subject only to the condition that he report his actions to Congress. He terms the statute finally adopted "an historic tragedy"; for in his view it gives to the President and all his successors a "pre-dated 60-day unilateral warmaking authority" (p. 219).

In a more complete and evenhanded fashion, the study by Senator Javits surveys the history of presidential-congressional relations in the exercise of the war powers. Only his concluding chapter is devoted to the matter of efforts by Congress during the 1970s to reassert for itself authority in this sphere. Since the book was presumably published prior to the time the 1973 War Powers Act was given its final form, Javits does not take issue directly in his study with Senator Eagleton's assessment of the Act's ultimate impact. It is clear, however, from Senator Eagleton's account of the legislative actions that eventuated in its enactment that he and Senator Javits were in sharp disagreement over whether it should have become law.

No question has afforded more opportunity for engagement in what Senator Javits terms "one of America's favorite sports—Constitution wrestling" than the subject dealt with by the authors. The pages of congressional proceedings abound with accounts of great and near-great debates on the matter. Journalists and learned commentators have offered their contributions at length and in profusion over the years. As Senator Javits puts it, the historical record demonstrates that the "twilight zone [between congressional and presidential war powers] has become a kind of constitutional DMZ infiltrated by expansionist-minded Presidents" (p. 256). This theme, upon which Senator Eagleton bears down even more heavily, is doubtless justified, but it is by no means novel. The question to be resolved, as the two authors see it, is how Congress should go about trying to restore the sanctity of this constitutional DMZ by legislative fiat. Their differences on the point are over means, not ends.

As historians in their more candid moments are apt to observe, history is an argument without end. Senator Javits appears to be much more ready than his colleague to recognize that a great many factors other than mere lust for power by the executive have been responsible for the accretion of power by presidents at the expense of Congress. The changing role of the United States in the world community, new techniques associated with making war, the mystique of the "imperial presidency" so assiduously cultivated in some quarters in the post–World War II era, and the instinctive aversion of members of Congress toward taking a politically inexpedient stand on issues involving the national security in time of crisis must all be taken into account, he avers, in explaining this trend.

Both studies are as much criticisms of American "imperialism" of the nineteenth and twentieth centuries as they are attempts to portray the presidents concerned as usurpers of powers properly belonging to Congress. In his zeal to show that the president has usurped war-making powers constitutionally allocated to Congress, Senator Eagleton even goes so far as to misquote the Constitution itself at one point. The clause vesting in Congress the power to "grant letters of marque and reprisal" (a clause authorizing the licensing of private shipowners to engage in privateering, a practice long since abandoned in international conflict) becomes at his hand the authority to "grant letters of marque; *Order* reprisals . . . etc." (p. 8, emphasis added)—a far different matter. Nevertheless, he concludes his brief survey of nineteenth-century developments with the comment that "throughout the nineteenth century, with remarkably few exceptions, American Presidents and Congresses sought to maintain the traditional constitutional balance between the power of the executive . . . and . . . the Congress" (p. 42).

Senator Javits is not so generous with some of the earlier presidents, as some of his chapter headings (e.g., "Jefferson's Private War"; "Lincoln: The Great Violator?") suggest. He is careful to acknowledge, moreover, that not all assumptions of initiative by presidents that have resulted in war or the "bullying" of weaker nations have been disavowed by the Congress or the American public later when the facts came to be fully known. While, for example, it is now conceded that the Mexican War was a "presidential war" brought on by the aggressive measures of President Polk and sanctioned only with reluctance by Congress, Javits notes that no one now advocates giving California back to Mexico. Nor, he adds, is there any strong sentiment in the United States for giving up the Panama Canal Zone concessions obtained through some high-handed diplomacy and military maneuvering by President Theodore Roosevelt.

The Vietnam War was the catalyst for these re-examinations of the questions of how and by whom this nation may be committed to a war-like endeavor, how much manpower and treasure may be employed, and for how long. The most significant outcome of that conflict from the constitutional standpoint was Congress's assertion, through exercise of its power over the purse and its authority to raise and support armed forces, that it can limit the arenas of action, wind down our military involvement in an ongoing conflict, and compel eventual disengagement. Passage of the War Powers Act of 1973 was a concluding effort by Congress to place on our statute books an admonition to presidents, "No more Vietnams!"

The long-range impact of that legislation upon the making of policy decisions by presidents involving the use of American military forces abroad remains to be seen. In the one clear test of its efficacy to date—the Mayaguez incident—it proved to be no serious deterrent to the President's capacity to act decisively and effectively in an emergency through use of military force. The Act came into play only to the extent of obliging him to report promptly on the matter to Congress, so as to afford Congress an opportunity to register its disapproval of his actions if it wished. The affair was terminated long before the necessity arose for the President, in accordance with the Act's terms, to obtain a positive resolution of approval by Congress to enable him to continue on his course. As subsequent popular polls seemed to indicate, the President's forceful response to the crisis redounded greatly to his credit.

These works provide illuminating insight into the minds and motivations of two of the most influential members of the Senate with regard to their respective courses of action on a matter of critical concern to the nation. Advocates of the proposition that the national interest requires greater involvement of Congress in the making of policy decisions in the military action sphere will find in these works able and effective support.

JOSEPH E. KALLENBACH

University of Michigan (Emeritus)

Black Power/White Control: The Struggle of the Woodlawn Organization in Chicago. By John Hall Fish. (Princeton, New Jersey: Princeton University Press, 1973. Pp. xii, 356. $12.50.)

This volume is an historical/analytical account of the development of The Woodlawn Organization (TWO) during the 1960s and the early years of the present decade. Woodlawn is a neighborhood immediately to the south of the area occupied by the University of Chicago. The university and the organization have had continuous relationships from the beginning; in fact, according to the author, it was the university's plan to expand to the south that led to the creation of TWO in the first place. TWO was initiated under a grant to the late Saul Alinsky and his Industrial Areas Foundation as similar community organizations were created in other cities across the country. Fish describes in some detail how TWO became a spokesman for the Woodlawn community through the confrontational style used by Alinsky organizations throughout the country (accompanied by minimal "victories" over various "enemies" but not much basic change in social conditions). In the late 1960s, TWO moved beyond the position of spokesman for the residents of Woodlawn to three attempts at community control: (1) a youth project involving the Blackstone Rangers; (2) a school decentralization project; and (3) involvement in the planning of the Model Cities program in the area. In detail, the author shows that TWO failed to win on these three issues because it could not overcome the combined power of the Democratic party organization and entrenched local bureaucracies. The city of Chicago was not ready to give up public authority to a private group whose interests were not often in harmony with their own. Hence, in order to survive, the organization sought coexistence with the powers that be through community development programs. In summary, then, the book provides a description and analysis of the evolution of a community organization from confrontation to coexistence with the dominant institutional forces in Chicago. In addition, the volume tries to deal with the difficult question of "so what?" What difference did the presence of TWO make? In one sense the answer seems to be "not much."

Population was leaving the area while fires literally were burning much of the physical plant. Professor Fish, however, argues differently. He claims that the organization helped retard the physical decay of the area and provided meaning and hope for the residents. And the survival of the organization itself, despite its setbacks, is viewed as a major accomplishment. What does the author conclude about the potentialities of community organizations whatever their locale? A fair inference would be that these organizations can assist residents to gain a sense of identity and can win victories when they carefully select targets and do not take on entrenched powers on issues salient to those powers. And it may be that their very presence causes outside agencies, both public and private, to be more responsive to neighborhood desires. But, more soberly, there is little evidence that they can change the basic conditions of ghetto life.

How did the author reach his conclusions? It was based on "six years of intense involvement with TWO both as a scholar-observer and as an active participant" (p. 6), including attendance at hundreds of meetings, service on numerous committees of the organization itself, and interviews with a large number of the participants in the affairs of the organization. It resulted in a strong commitment to the organization. The effort was monumental, but the results are hard to evaluate. As with most studies of this kind, the validity of the conclusions are suspect. The author, apparently, made no effort to relate his work to the developing methodology of participant-observation or to the substantive works of authors employing this methodology (e.g., Gans, Liebow, Oscar Lewis). And, less important, the writing style is at times so filled with acronyms that all but the most dedicated follower of TWO might be tempted to put the book down.

It is easier to raise these criticisms than to suggest clearcut alternative approaches. The evaluative case study may be needed in areas where few prior research efforts exist. Clearly, to study the effectiveness of an organization like TWO logically implies an experimental or quasi-experimental design (as exemplified by the work of Donald Campbell). And yet the application of these designs to such complex problems as assessing the effectiveness of community organizations has barely begun. As a result, despite the plethora of community organizations like TWO that developed in the 1960s, we have little solid evidence concerning their accomplishments.

Nevertheless, this particular study is valuable for those interested in the politics of race, community organizations, urban politics, and the enduring questions of participatory democracy and the limits of power of the citizenry as they attempt to deal with the problems around them. That answers to these enduring questions are not forthcoming may be less important than the fact that they were raised.

LYMAN A. KELLSTEDT

University of Illinois, Chicago Circle

Race and Authority in Urban Politics: Community Participation and the War on Poverty. By David Greenstone and Paul E. Peterson. (New York: Russell Sage Foundation, 1973. Pp. x, 364. $12.50.)

In a narrow sense, Greenstone and Peterson are concerned with explaining the consequences of those famous three little words, "maximum feasible participation," in the anti-poverty legislation. Their research sites are the nation's five largest cities in 1960—New York, Chicago, Philadelphia, Los Angeles, and Detroit. But in a larger sense, they are really lifting the study of big city politics out of the explanatory *ad hocracy* long characteristic of investigators enthralled by Chicago's machine, New York's pluralism, Los Angeles' fragmentation, Detroit's unionism, and Philadelphia's bluebloods. This alone would make the contribution to the study of city politics an important one, well deserving of the prestigious imprimatur of the Russell Sage Foundation.

It is a commonplace for students of big city politics to be excessively impressed by the idiosyncratic character of particular large cities, attributing to it virtually every political event, change, or behavior. This is the *sui generis* approach to local politics, analogous to the explanations of the stereotypical "area specialist" in comparative politics. New York's notorious hyperpluralism ("It is no accident," say the authors, "that every major work on New York City politics has depended heavily upon the group approach of Bentley, Herring, and Truman" [p. 39]) becomes an explanatory *deus ex machina*, as does Chicago's machine, or Detroit's class conflict. Greenstone and Peterson rank the five cities in terms of their participatory levels (New York first, followed by Detroit, Los Angeles, Philadelphia, and then Chicago), but find that "no single factor identified in the case studies accounts for the variation among the five cities" (p. 46).

What is needed, therefore, is an integrated set of explanatory constructs which will account for intercity variations in the dependent variable of participation. The discipline's now-favorite whipping-boy of pluralism is tried, and fares no better than the idiosyncratic explanations derived from the "political culture" approach. The authors substitute the concept of objective interest, or more elaborately, "latent-manifest role analysis," for the pluralist's essentially subjective treatment of interest. Pluralists depend upon a model of electoral interest and organizational maintenance to predict behavior. But unfortunately for the model, a good deal of the participatory conflict is better explained by ideology than by actors like mayors behaving in their own electoral interests. In the Agger-Goldrich-Swanson terminology, upon which the authors depend heavily, the war over the war on poverty was essentially between the "Liberal Pragmatism" of most big city politicians on the one hand, with blacks and their "Community Conservationist" allies on the other, and "Progressive Conservatives" playing a variable role. It is, incidentally, remarkable–given the presumed impotence of urban mayors–to see how significant was the role of the mayor in imposing a policy settlement. That two recent major works on urban politics (this one and Peter Rossi et al., *The Roots of Urban Discontent* [New York: John Wiley, 1974]) attribute considerable weight to mayoral positions should prompt a reevaluation of the received wisdom about urban mayors.

Insofar as participation in the community action program is taken to be a dependent variable, Greenstone and Peterson offer, if not the last word, at least a very definitive one. There is also, however, the consideration of participation as an independent variable. This question asks what consequences participation has for the allocations of urban policy outputs. Here the analysis is much shallower. Peterson's earlier work on forms of representation in community action is elaborated in the volume, and demonstrates that more democratic modes of selection were negatively related to the "effectiveness" of representation. The measures of the effectiveness and the impact of participation are, however, casual at best. Two of the authors' University of Chicago colleagues (Verba and Nie in their *Participation in America* [New York: Harper and Row, 1972]) formulate a much more explicit model of participation as both a dependent and an independent variable. Had the present writers been as attentive to the impact of participation on changes in urban policy as they are to the analysis of the structure and determinants of the participatory settlement, their efforts would have taken better account of both sides of the participatory ledger.

Because the analysis of the policy consequences of participation is so abbreviated, it is difficult to evaluate the authors' unwavering support of community control in the final chapter, which purports to be a "policy analysis" of participation. No serious policy analyst, though, would begin with such skimpy evidence on policy impact. What would be needed at a minimum is a parallel rank-ordering of the cities in terms of what was gotten or changed as a consequence of the participatory system. But because the overwhelming body of conceptualization and data bear upon participation as a dependent variable, one might be suspicious, on the basis of the evidence presented, of the conclusion that "community control, for all its faults, can facilitate the forward thrust of black power in the American regime" (p. 315). The strength of the analysis does not lie in these shaky conclusions, but rather in the effort (parallel to Rossi et al.) to lift big city politics from the clutches of the case study, and in showing that the assumption of *sui generis* is neither necessary nor viable. The long-term impact of community control is not definitively settled here. But, as the authors forcefully argue, the community action program symbolized, and perhaps even accelerated, a transformation of big city politics from distributive "Liberal Pragmatism" to a potentially redistributive politics of race.

ROBERT L. LINEBERRY

Northwestern University

Postal Monopoly: An Assessment of the Private Express Statutes. By John Haldi. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1974. Pp. 70. $3.00.)

The United States Postal Service. By Gerald Cullinan. (New York: Praeger Publishers, 1973. Pp. 271. $10.00.)

The apparently inexorable deterioration of the mail service ought to make both of these books timely and interesting. In fact Haldi's monograph does address itself to just this question in a lucid and, on the whole, persuasive manner. Cullinan's book, one in a long series Praeger has published on U.S. government agencies, manages to convey more than most readers, including this one, would want to know about the post office without, at the same time, throwing much light upon its effectiveness and efficiency.

Haldi's *Postal Monopoly* is a closely reasoned tract in the American Enterprise Association's Evaluative Studies Series. Reflecting the free-market preference of the authors AEI publishes, Haldi mounts an impressive attack upon the private express statutes which provide the postal service (now postal corporation) with a legal monopoly in the delivery of letters. The poor service we all receive is the direct result of this legal monopoly, he argues, for all the reasons traditional economic theory proclaims, given a situation in which an enterprise is removed from the discipline of the marketplace. Untroubled by competition, the postal service has no incentive to improve its service in order to sell its product; it is thus inefficient, inflexible, and badly in need of modernization. Furthermore, because of its monopoly status it has, at least in the past, been able to engage in very questionnable pricing practices (i.e., the great majority of senders of first-class mail must pay substantially higher rates than the service requires in order to subsidize second and third-class mail and a small volume of very costly, hard to deliver, first-class mail).

Haldi contends that many of the important innovations in mail delivery result from private competitors who, acting on the margins of the law, have introduced service improvement techniques as a means of attracting clients. These techniques have then been adopted by the Post Office as a competitive response—along with legal action to prosecute such companies as violators of the private express statutes. He also points to the success of the United Parcel Service in providing generally cheaper and more efficient service than the post office in the delivery of fourth class parcels, the one area where private express statutes do not apply.

The traditional counterargument utilized by proponents of the postal monopoly is that competitors would enter the market only to "cream skim"; they would compete with the postal service only in the lucrative markets—first-class intra-urban or interurban ones—leaving the less attractive high costs markets to the Post Office. The only means the Post Office could use to compete would be to cut the postal rates and improve service on their first-class mail, but resort to this revolutionary tactic would reduce revenue and make it impossible for the service to continue to subsidize its higher cost routes and second and third class mail out of first class revenue surpluses.

If this were to occur, however, users of first class mail would benefit, and if society decides that it does not wish to charge rural dwellers and second class (magazine and newspaper publishers etc.) the actual cost of delivery, then the proper recourse ought to be an explicit public subsidy out of general revenues rather than a hidden subsidy paid for by first class users.

Haldi's presentation is lucid and concise. Clearly he is arguing a brief rather than presenting an impartial, balanced case, but he argues very persuasively. Cullinan in *The United States Postal Service* touches upon Haldi's argument only in the last chapter when he resorts to the cream-skimming argument and pronounces himself in favor of tightening the statutes creating a postal monopoly. He follows this prescription with the caution that "in a free society a monopoly must be earned. If the postal corporation's monopoly is to be broadened and reinforced, the corporation must demonstrate that it can serve the American people better and more cheaply than any competing enterprise" (p. 257). One might well ask how that can be demonstrated unless other enterprises are, in fact, allowed to compete.

Cullinan's book is a readable, though somewhat superficial, history of the postal service. The bulk of the book is a postmaster-by-postmaster history of the postal service, focusing on the personal characteristics of the postmasters and their accomplishments. The strong views the author holds (the post office should be a public service not a business run by the profit sheet; postal management has been callous in its treatment of its employees) are readily expressed and contribute to a good guy/bad buy view of the postal world. The book should be useful to those who, for whatever reason, might wish a quickly readable and very general history of the postal service. I cannot imagine however, that more than a handful of serious political scientists would fall into that category.

HAROLD WOLMAN

University of Massachusetts, Boston

Beyond the New Deal: Harry S. Truman and American Liberalism. By Alonzo L. Hamby. (New York: Columbia University Press, 1973. Pp. 635. $12.95.)

"Trumania in the '70s" it has been labeled, our recent proclivity—stimulated in no small part by facile comparisons to Gerald Ford—to turn to Harry Truman and the years of his presidency to escape the present, to hunt remedies for the future. Professor Hamby's book then, comes along when the time is ripe for a fresh, careful examination of Truman as president. More particularly, we may be grateful for the author's decision to focus on Truman's interaction with that group which now seems peculiarly amorphous—American liberals, whose last lost champion was George McGovern. I have quarrels with Hamby's presentation, but let it be promptly noted that he does fulfill the historian's task. He chronicles the past with precision, he manages to keep the object of his exploration center stage, and he does not pretend to objectivity—at least not to an objectivity which eschews final evaluation.

Immediately, in his introduction, the author bares a critical assumption: the relationship between Truman and the liberals was *bound* to be difficult in light of its sequential position after the era of the New Deal, and, even more importantly, after the era of Franklin Roosevelt. FDR, Hamby claims, "*was*" liberalism. "For this reason alone, the future of liberalism beyond FDR was uncertain" (p. xvii). The logic of this argument is that there was no place to go but down. The question was, just how far.

Hamby views Truman's achievements with regard to liberal objectives as "substantial" (p. 513). For the benefit of that public which may have missed the point as it is made in the first 503 pages, the author presents a final chapter in which he concludes that the "subtle creative tension" which characterized the relationship between Truman and the liberals was healthy and that, in tandem, "they successfully defended and reinforced liberalism, moving it beyond the New Deal and keeping it in confrontation with the crises of postwar America" (p. 516).

So much for the summation. What about the meat of the argument?

The meat is history; quite literally we are told facts. There is little interpretation. Instead, events unfold chronologically. The reader is led down a straight path (roughly covering 1945–52), urged along none too gently by an impressive array of evidence. The data have been culled from manuscript collections and interviews as well as from the more predictable periodicals, newspapers, primary and secondary sources. If we are denied the excitement of theory, and the pleasure of exploring causality by meandering from the already beaten track, we do gain the satisfaction of having run the mile with such deliberation that the chalk beneath our feet has become familiar.

The book is divided into two parts. The first traces the concept of progressivism as it evolves from an identity with communism—an identity fostered by both the Depression and the war—to an altered persuasion, the Vital Center, which distances itself as much from the totalitarianism of the left as from that of the right. In the second part, the author, having established a reasonably fixed idea of what liberalism implied through at least the years 1948–52, concentrates on the exchange between those who came to symbolize it, and their now more readily accepted leader, the President. Indeed, it is Truman who, throughout, serves as Hamby's focal point. Regrettably, he seems unnaturally forced into that role. The impression persists that the author's spontaneous interest lies in his cast of liberal characters—and in their cause.

And here is the rub. Hamby does a precise job, he succeeds in presenting policies and politics and the intellectual founts from which these flow (note, for example, his provocative reminder that "the split between the progressive as moral critic and the progressive as politician represents one of the oldest themes of American liberalism" [p. 288]). But what we should also get and do not, is a frame of reference for the liberal belief system as it actually evolves within the minds of men. Kirchwey, Lerner, Niebuhr, Schlesinger (author of *The Vital Center*), Stone, Wechsler—any one of these could have provided the base for the neglected personological context. (I deliberately omit Wallace.) Any of their stories—followed through rather than merely commented on at erratic intervals—would have driven home the crucial point that the interplay between Truman and the liberals was more than intellectual or political gamesmanship. It was, for those most deeply involved, an intensely personal drama played out on a public stage.

But Professor Hamby's book is cogently argued. What it lacks in analytic imagination it compensates for in large part by diligent attention to the historical record. And what it lacks in bite might well be supplied by the reader, a reader eager to debate what often seems curiously old history or, better yet, to ponder anew the elusive connection between American political thought and American political behavior.

BARBARA KELLERMAN

Tufts University

International Publications: An Annotated Subject Bibliography, With Library of Congress Catalog Card Numbers, 1973–74, second edition. By International Publications Service. (New York: International Publications Service, 1973. Pp. 274. $7.50, paper.)

Intended as a selection device particularly for libraries and institutions, this work is an extensive, annotated listing of more than 6,500 titles mostly in English selected from worldwide sources. As an additional aid for libraries, the entries are established according to Anglo-American cataloging rules, including all available Library of Congress Card Numbers. The titles together with full bibliographical information are arranged under broad subject headings within two major divisions: Part (1) Serials and Series, and Part (2) Monographs. A semiannual supplement is issued in February of each year. A major limitation to this publication is the exclusion of an index, which makes it difficult to locate specific titles.

ROBERT B. HARMON

San Jose State University

National Party Platforms, 1840–1972, 5th edition. Compiled by Donald Bruce Johnson and Kirk H. Porter. (Urbana: University of Illinois Press, 1973. Pp. 889. $20.00.)

The fifth edition of Johnson and Porter's *National Party Platforms* brings up to date their impressive compilation of major as well as minor American party platforms. The volume's sheer bulk of detail is almost overwhelming; enough anecdotal material can be found to provide illustrations and examples for many lectures. While the Vegetarian Party's platform is conspicuous by its omission, aficionados will be relieved to find that both the Prohibition Reform Platform of 1876 and the Independence Platform of 1908 have been included.

At first glance one may seriously wonder whether such a compilation is anything more than the preservation of historical minutiae. The platform is a frequently maligned document. It is often said that platforms are never read by voters, are quickly forgotten by elected officials and contain little besides vacuous generalizations. Oddly enough, however, platforms are frequently the objects of internal controversy and recrimination at party conventions and are, from time to time, taken seriously enough to be repudiated by candidates.

Though both Pomper and David have found some direct relationships between the parties' platform promises and subsequent legislative action, seldom is either party platform terribly precise or forthright on important policy questions. Major party platforms are rarely intended to provide voters with an intelligible account of how the party plans to behave in office. Rather, the audience to which the platform is addressed is usually the party itself. Platform writing represents an occasion for party leaders and factions to attempt to come to terms with one another on the conditions for cooperation in the coming campaign. While the content of the platform may not be intelligible to voters, the platform's coded phrases are of great interest to party leaders. Behind its apparently empty rhetoric the completed platform is a symbolic contract among party factions—a contract in which each major faction has stated, in coded form, the terms on which it has agreed to cooperate with the national party's campaign efforts.

Analyzed carefully, the platform can be an extremely valuable document to students of American party politics. Platforms offer an amalgamation and distillation of the views and preferences of each party, taken as a whole, over a long period of time. In conjunction with other sources of data, platforms can provide a basis both for characterizing the amorphous national parties and for gauging the relationships between parties and other social and political forces. Contrary to conventional wisdom, party platforms do address themselves, in code, to important public issues. Breaking the code is a difficult but fruitful task. Political scientists such as David, Ginsberg, Pomper, and others who have used party platforms in their research will welcome this updated volume.

BENJAMIN GINSBERG

Cornell University

Clean Air: The Policies and Politics of Pollution Control. By Charles O. Jones. (Pittsburgh: University of Pittsburgh Press, 1975. Pp. xiv, 372. $13.95.)

A major focus of "postbehavioral" political science has been policy analysis. Numerous case studies, representing discrete and particularistic accounts of policy events, have been published. But structured, systematic approaches to such analysis have largely been lacking. A methodology for policy analysis is only beginning to be formulated. In this report, Professor Jones goes considerably beyond the first generation of policy studies, and begins to provide a structure for policy analysis which may permit generalizations and provide guidance for other studies from which ultimately an organized body of knowledge may emerge. It is apparent, incidentally, that this present study has benefitted significantly from the author's earlier essay, *An Introduction to the Study of Public Policy* (Belmont, California: Wadsworth Publishing Co., Inc., 1970).

In his Preface, Professor Jones suggests that the book (p. x) "is about 'smoke and survival,' about the conflicts between what we want and the conditions under which we get it. I have sought [he states] to provide a realistic analysis of the tough choices that have to be made in an affluent society." In the first chapter he states that "it is simply too easy to forget the realities of specific problems in identifiable places when one analyzes policy development and implementation by decision makers, many of whom are far removed from the smells and smoke of industrial processes" (p. 3).

In a broader context, this book is significant for at least three reasons. First, as the title indicates, it deals with an important environmental problem. Second, it provides a meaningful case study of intergovernmental relations in this field of environmental protection. Third, it presents a detailed analysis of the complex decision-making processes in a field where technology and scientific data are intertwined with social and political concerns.

The study begins with a review of the air pollution situation in Clairton, Pennsylvania, southeast of Pittsburgh, the site of the largest coking facility in the world. It details local, state and federal efforts to deal with problems of air pollution in that community over several decades. The account starts with what were primarily local efforts in 1941. It traces state efforts to strengthen and reenforce local controls, and then finally assesses the consequences of federal intervention. This is more than a conventional analysis of intergovernmental relations, however, for the author presents in considerable detail the different kind and character of inputs into the decision process at each governmental level, suggesting that the conflict and struggles we have come to associate with intergovernmental relations are more than simply power struggles, more than simply ways for determining who gets what, when, where, and how.

It is apparent throughout the study that Professor Jones has mastered a good deal of the science and technology of air pollution and its control, especially as related to the Clairton situation. From this knowledge he is able to write what may be the most significant chapter of the book, namely, Chapter 8, entitled "Implementing Policy Beyond Capability."

As public policy decisions come more and more to depend upon scientific and technological inputs (facts) rather than simply on conclusory and declaratory statements of policy makers, our concern for due process, reasonableness, fairness and equity requires that such policy is based on three kinds of knowledge (data, information) and analysis: knowledge which goes to the identification of the problem (it is not enough simply to know that the air is hazy or smells bad); knowledge with respect to possible alternative solutions to the problem; and finally, and most often neglected, knowledge about the societal capability for dealing with the problem including institutional dimensions. Professor Jones, especially in chapter 8, points effectively to the fact that air pollution control policies, and one might add many other environmental programs and policies, have often not been soundly based on these three categories of knowledge. An almost inevitable result then is that goals are not achieved, and perhaps more seriously public confidence in government is further eroded. Failures in correcting environmental problems may in part (as some crusading lawyers would have us believe) be due to the machinations and manipulations of selfish interests and evil men. But they may also reflect our failure and inability to deal with issues of knowledge and particularly with questions of societal capacity. Ardent environmentalists might appropriately take note that at times the political system may be as fragile as the ecosystem. Perhaps both need to be regarded with care and circumspection. This would seem to be the most significant lesson of this policy study by Professor Jones.

Special note should be taken of the two appendices which add significantly to the value of the book. Appendix I is entitled "A Framework for Analyzing Policy Processes"; and Appendix II, "A Layman's Introduction to Air Pollution."

NORMAN WENGERT

Colorado State University

Administrative Feedback: Monitoring Subordinates' Behavior. By Herbert Kaufman, with the collaboration of Michael Couzens. (Washington, D.C.: The Brookings Institution, 1973. Pp. 83. $5.95.)

The book is a slight one: five chapters and about 83 pages of text (at $5.95, this is about 7c a page). The language is well crafted, but the content reminds me of a term paper by a beginning graduate student. There is an expression of seriousness about research design, information gathering, and findings; an occasional insertion of a fact or bit of data in an awkward "fit"; in general a tone of claim beyond performance, a continual assurance of the firm base of having "been in the field" but little evidence of the voyage to the field or that the trip was necessary—these are marks which call to mind the term paper of a novice. But there is a grace which puts the disappointing content in its very best light, brushes lightly over flaws, and makes common sense and familiarity with lore do the work that research and data should do so skillfully that lore almost seems findings and intelligence almost seems research—these are marks not at all of the novice. Perhaps the explanation is in the authorship (Kaufman is a senior fellow in Brookings and Michael Couzens was his research assistant).

In the chapter entitled "Research Objective," the authors strike a note of "science." Some aspects of the discussion seem problematic (and for that matter, hard to separate from the subject of the second chapter, "Research Strategy"). The purpose of the research is stated as trying to establish whether the "leaders of organizations are informed about the activities of their subordinates" or not. The study chooses to focus on "'administrative feedback', defined as all the processes by which the bureau leaders—the whole headquarters—are apprised of subordinate behavior" to the exclusion of "'substantive feedback,' the flow of information advising headquarters how close the activities of the organizations come to the substantive targets set by the leaders" (p. 1). This is a dubious distinction (as anyone who has wondered about communication between Washington and Vietnam knows), but within the rights of researchers to make. Amid the homilies of this chapter (e.g., "Despite efforts to reconcile instructions, many directives come down without regard to those from other sources. When this happens, subordinates may have to decide for themselves what their situations require, picking and choosing among the directives for justification" [p. 21].), there is precious little help in sorting proverbs from hypotheses, findings, or useful distinctions.

In chapter 2 ("Research Strategy") the attempt is made to deal with questions which are usually thought of as "methodological," It is here the reader gets the first hint about what sort of evidence is likely to be reported back from the field:

> So we set ourselves a more modest objective: to examine feedback practices and experience in a small number of organizations without aspiring to exhaustiveness or to methodological rigor. We discovered very early we would have to rely heavily on impressionistic and anecdotal evidence, and that the results would be inconclusive. But we thought impressions based on empirical studies would be more enlightening than the surmise and speculation that now surround the subject (p. 16).

This statement brings forward a host of doubts about the book. That the study can't be "exhaustive" is certainly understandable; but what are we to make of a "study" which disavows *aspiration* to methodological rigor? If the "evidence" is going to be "impressionistic and anecdotal" how much better off will we be than with "surmise and speculation?" The fear arises that we will get anecdotes and impressions as evidence and we will be asked to consider them as based on empirical studies largely on faith and without seeing much of the empirical study—this fear is pretty well substantiated by the rest of the book.

It is a good thing that the authors explain (in the last paragraph of the "Research Strategy" chapter) how the research was carried out, because the reader would be hard pressed to guess from the subsequent discussion that all these sources had been consulted. The reporting of research results is usually not specific enough even to be called anecdotal. *How* the generalizations were arrived at is almost totally without indication. The data base is invisible.

The discussion is riddled with statements which remind us of what the study *doesn't* tell us. For example:

> So the web of personal contacts is fashioned from both formal and informal elements. It therefore carries a great deal of traffic, including abundant cues and signals to bureau leaders about subordinate behavior (p. 37).

This approaches (at the "therefore") the obviousness of redundancy, but (at the "including") becomes a non sequitur. *Does* the web provide "abundant cues and signals" or not?—one would have thought that was one of the things the study might have cleared up! Moreover, while we get hard data on number of phone calls and number of employees (tangentially relevant at best), we get no data on, say, the number of investigations or inspections the various bureaus conducted in the period studied.

The book's conclusions present more unsubstantiated but perhaps wise homilies ("We think net increments of social and individual benefit are possible, often at modest cost, simply by bettering administrative feedback") and closes thus:

> Every researcher ends his report with a plea for further research. These entreaties often arise from the special curiosity of the researchers, rather than from the broader social implications of the additional knowledge. We are impressed with how many assumptions about the functioning of our governmental system rest in the last analysis on untested faith in feedback as one crucial component of that system. Just suppose that faith is misplaced (p. 80).

Right! Maybe someone ought to study administrative feedback.

FRANK MARINI

San Diego State University

Political Strategies in Northern School Desegregation. By David J. Kirby, T. Robert Harris, Robert L. Crain, and Christine H. Rossell. (Lexington, Mass.: D.C. Heath & Co., 1973. Pp. 255. $10.00.)

In the preface the authors state that they have attempted to write "a statistical social history" of the school desegregation issue from 1963 through 1968. In the course of doing so, they seek to answer the question of what actors attempted to influence school desegregation policy and to what effect. As social history the project fails, for its comparative analytic framework is too spare to permit full description of the rich variety of factors which affected desegregation policy in American cities during the period in question. It is more successful in meeting the second objective–explicating the nature of the relationships among the dominant actors involved in this policy area.

Methodologically, the study is innovative. It represents a creative blend of the positional and reputational approaches to the study of community power within an issue-specific comparative framework. The findings are based on data collected from a probability sample of 91 cities located in every region of the United States except the South. In addition to aggregate demographic data, interviews were conducted with informed individuals in each city. Nineteen categories of informants were contacted in each city in order to elicit information based on each type of respondent's unique vantage point.

Two related, but conceptually distinct, indicators of policy response were developed for purposes of evaluating both the qualitative and the quantitative aspects of influence. The first, labeled General Response, measures the range of policies that were adopted in order to improve the quality of education for a city's black students. The second, Action to Desegregate, is a more specific measure designed to reflect the degree to which a city took action that in fact led to desegregation. Although there is a fairly strong tendency (r=.59) for cities which score well on one of these measures also to score well on the other, the causal linkages which produce each type of policy response are shown to differ.

Neither activity on the part of civil rights groups nor assertiveness on the part of the school superintendent was found to have much impact on General Response. A unified, low-key civil rights movement, however, *coupled* with strong leadership by the school superintendent showed a pronounced effect on the desegregation variable. Conversely, widespread support for school integration among whites tended to result in high General Response scores, but it had no effect on Action to Desegregate. Interestingly, the data showed that white opposition to integration had little impact on either policy variable inasmuch as it tended to become manifest only after integrationist policies had been adopted.

Two of the more important findings of the study bear on the respective roles of school boards and civic elites. Contrary to earlier research which suggested that a unified, pro-integrationist school board was the single most important factor in producing a positive response to integration demands (Crain et al., *The Politics of School Desegregation,* 1968), the present study indicates that it was the politicized, conflictual board that was most likely to have a positive impact on both of the school integration variables. Support for integration by a strong community elite likewise was found to contribute to both types of policy response. Paradoxically, however, it was found that the stronger is the elite stratum within a community, the less likely it is that civic leaders will support integration demands.

Demographically, cities that took specific actions to desegregate shared certain characteristics with those that had high scores on the General Response variable. Both tended to be older cities with large populations and relatively high percentages of ethnic, older, and poorly educated residents. As the authors point out, such cities "have a more traditional style of politics–including such factors as blatant interest groups, patronage, and political machines" (p. 64).

In other respects high General Response cities differ from high Action to Desegregate cities. While the former tend to have relatively large black populations, the latter tend to have smaller proportions of black residents. The explanation offered for this finding is "that large cities with large black populations have struggled mightily with the issue of black educational opportunities, but they cannot be desegregated, either because there are not enough white students to go around, or because distances between black and white schools are too great" (p. 64). Moreover, those that did desegregate to some degree tended to be concentrated in states that had adopted more aggressive desegregation policies at the state level.

For students of policy formation processes this study offers valuable insights into the political dynamics of a significant contemporary issue area. For those interested in the study of community politics it serves as an innovative model for comparative research. For proponents of voluntary school desegregation, however, it provides little encouragement. The authors conclude not only that blacks have lost interest in the issue but also that continued progress toward school desegregation in northern cities is likely to be confined to those school systems that have already made efforts in that direction but that also must overcome so many practical problems that only modest success can be expected.

WAYNE PEAK

Colorado State University

The U.S. Intelligence Community: Foreign Policy and Domestic Activities. By Lyman B. Kirkpatrick, Jr. (New York: Hill and Wang, 1973. Pp. xi, 212. $7.95.)

This book was written before the recent uproar about the CIA, and this review is being written in the midst of that uproar. Thus fixing on the facts for purposes of a review is difficult; important information is just now emerging, and our conception of what we want to know is under revision. Professor Kirkpatrick, long involved in his subject as both practitioner and scholar, wishes to "set the record straight" (p. ix) to offset sensational but allegedly inaccurate exposés. The result is an establishment critique; he finds little seriously wrong with the intelligence community and expresses considerable confidence in existing control mechanisms.

Despite its size, the book is relatively rich in details on the history of the intelligence community, provisions of the National Security Act, administrative structures, and coordinating mechanisms. His review of past congressional grumpiness about intelligence and of its jurisdictional squabbles on the exercise of legislative oversight provides a nice backdrop to current efforts at reform. Judicial activities in defining the rights of ex-employees or the limits of wiretapping are discussed. There is a definition of intelligence, a description of sources of information, a review of standard problems of intelligence acquisition and analysis, and a useful annotated bibliography. All is set forth with admirable brevity, in readable prose, and with a felicitous choice of examples.

But there are sins of omission. For estimates of the size and budgets of agencies we must look elsewhere. It is no secret that the CIA houses two separate hosts—analysts and operators—and that cooperation and respect between them is minimal. CIA analysts have long felt that military intelligence mirrors military self-interest to a point bordering on a deliberate deception of the President and his National Security policy advisors. CIA's output was so badly regarded in Kissinger's fiefdom that the White House shattered the organizational structure for preparing national intelligence estimates and scattered the "old-boy" network ensconced therein. None of this really appears in the book. Some of it should have; the rest dates it for today's reader. And Kirkpatrick's careful discussion of improper FBI and Army domestic intelligence activities is also now dated by recent revelations about the CIA.

On covert operations there is virtually nothing. The author's conception of intelligence—"a compilation and distillation of the total knowledge on any given area or subject" (p. 3)—puts him with the analysts, not the operators. References to the Bay of Pigs, Guatemala, or the PHOENIX program are never expanded into a thorough discussion of types of covert operations, their uses and dangers, and their proper place in intelligence. Recent disclosures and the questions now being asked about such activities are not foreshadowed; there is no consideration of the frequently voiced suggestion that analysis be made institutionally separate from covert operations.

Professor Kirkpatrick emphasizes intra-executive and congressional control mechanisms as the only ones compatible with the secrecy necessary for intelligence effectiveness. In this connection, he gives short shrift to the charge that intelligence agencies are out of control. Instead he correctly detects the heart of our problem: a loss of confidence in the character or integrity of those highest officials responsible for supervising the community. His only solution is "recurrent, exhaustive scrutiny by respected critics, followed by the purging of policies and practices found to be repugnant to American ideals. This must be done by the President and Congress, if necessary, under the pressure of public opinion" (p. 191). Presumably he would approve of the recent examination of the intelligence community that has been taking place along precisely these lines.

The book presents contrasting case studies on the impact of intelligence on policy. The Cuban Missile Crisis is "almost a model for the meshing of intelligence and policy. . ." (p. 86). In Vietnam, reasonably accurate (and thus pessimistic) intelligence assessments were persistently ignored. What determines which will occur? The book offers some ideas, stressing the role of the President, but gets into this important subject in no very penetrating way. It seems the impact of intelligence varies with time, the issue, and personalities—not much to chew on intellectually.

PATRICK M. MORGAN

Washington State University

Old Wars Remain Unfinished: The Veteran Benefits System. By Sar Levitan and Karen A. Cleary. (Baltimore, Md.: The Johns University Press, 1973. Pp. 190. $10.00.)

This volume provides a useful brief description of a special welfare system—the veterans welfare system. Veterans, of course, may take exception to describing the program of the Veterans Administration as welfare. And citizens opposed to "welfare" may not question aid to veterans. But a rose by another name. . . . As the authors remark in their preface, "Over the years, programs in aid of veterans have evolved into a comprehensive welfare system" (p. ix).

Veterans' welfare is interesting for several reasons, but two or three stand out. First, income support for veterans is apparently acceptable, relatively liberal, and the means tests are not mean. Veterans' welfare, as the authors suggest, provides something of a model for other assistance programs. Second, and more important, the implications for veterans' programs (and the Veterans Administration) of any major expansion of more general social welfare programs deserve careful thought. Does the VA have a future?

The authors begin their volume with a description of the veteran population and an overview of the benefits available to veterans. We are reminded of the substantial cost of these programs ($11.5 billion in 1972) and learn that "most of the costs of support of World War II veterans is still ahead of us" (p. 1). (Obviously, interesting games can be played with the appearance of the budget by choosing to label these costs as "human resource costs" rather than "defense related costs".) A summary description of the administration of veterans benefits is followed by a discussion of veterans organizations and their relations both with the Congress and the Veterans Administration. Not surprisingly, it is not uncommon for leaders in the American Legion to become administrators in the VA.

Chapters 2 and 3 focus respectively on income support and medical care. The income support discussion distinguishes among several programs, including service-connected disability compensation and need-based pensions, and the operations of the programs are described. The authors emphasize that pensions available to indigent veterans and their survivors are more generous than other public assistance programs and the application process is much less demeaning. "All a veteran has to do to maintain eligibility for a pension is to submit an annual income statement indicating earned income of the past year and anticipated income in the upcoming year. If the veteran is 72 years old and has been on pension rolls for two consecutive years, he is exempted from filing income reports" (p. 49). Later we are told, "The VA does not scrutinize reports and respondents are assumed to be truthful" (p. 51). Obviously there are varying standards for welfare; some poor people are clearly more deserving than others. Yet it may be noted that the GAO has criticized VA laxity and recommended tighter procedures.

Chapter 3, Medical Care, is essentially an economic analysis. Readers are told in some detail about facilities, services, costs, and patient loads. But the attentive reader may be troubled, as indeed he may have been in chapter 2. The difficulty is the apparent failure to dip below the surface and see how things really are, as opposed to how the VA says things are. For example, the authors write, "The steady expansion of VA medical responsibility has resulted in a national hospital system which apparently can be organized and run to deliver above-average care to high numbers at a reasonable cost" (p. 101). Missing is any acknowledgement that VA medical care may be (and indeed has been) subjected to criticism directed at its response to the problems and numbers of Vietnam veterans.

The last chapters of the book (4 and 5) examine the variety of services available to returning veterans and consider the future of veterans' programs. Much of the discussion in Chapter 4 focuses on various education, training, and employment activities. The authors argue that these have been weak: "Job creation and employment services for veterans have relied mostly on rhetoric" (p. 160). The authors note that there are no follow-up data on GI Bill students or training program graduates; as a result, little is known about the effectiveness of such programs. And in discussing the GI bill the authors raise a question that should be of interest to every educator: Should the government induce "veterans to pursue programs of higher education while both society and the individual may benefit from an expansion of the supply of craftsmen and individuals with skilled trades?" (p. 161).

At the end the authors question the long-run necessity (and equity) of the VA and its programs. The pension program, they suggest, might serve as a standard or floor with citizens in general being compensated at levels now reserved for veterans. "Similarly, the adoption of a national health program would obviate the need for a separate veterans hospital system" (p. 173). In short, the VA might in some sense serve as a model and might in any case be absorbed into generally expanded social welfare programs. Whether the American Legion would be comfortable with this is, of course, questionable.

On the whole this is a useful and informative book and belongs on any well-stocked social policy shelf. Yet it is limited by its sources. Levitan and Cleary relied heavily on official sources and have reported, or so it seems to me, a formal official picture of the VA and its programs. It remains to go out and ask veterans what programs they know about, what they are getting and have gotten, what they have been denied, and how satisfied they are. In other words, what is needed is a study of the VA by political scientists or sociologists to supplement this one by economists.

JAMES W. DAVIS, JR.

Washington University

The New Left and the Origins of the Cold War. By Robert James Maddox. (Princeton, N.J.: Princeton University Press, 1973. Pp. 169. $2.45, paper.)

An effort to evaluate Mr. Maddox's book within the constraints of *The Review* makes one jealous of pensmen who inscribe the Gettysburg Address on a dime.

Mr. Maddox's thesis is that "a growing body of academic historians consistently misuses the fundamental sources of a crucial era in America's history" (p. 163). In elaboration he examines seven historians. Responsible evaluation of his argument would entail: (1) presentation of the positions of the seven on each issue Maddox analyzes; (2) comprehensive citations of the sources on which each bases his arguments; (3) determination, through systematic comparison of (1) and (2), when Maddox's indictments are justified. This space-consuming enterprise would necessitate a book longer than Maddox's. Yet short of it a "review" may add more to polemics than to understanding. We seek a compromise between brevity and responsibility through application of the three-stage approach to a small portion of Maddox's book.

The task of stringent selection is made easier by Maddox's distinction between "soft" revisionists (e.g., D. F. Fleming) who emphasize individual behavior, and "hard" revisionists (e.g., W. A. Williams or Gabriel Kolko) who see the Cold War as "the inevitable result of the American system" (p. 4). I agree that the latter "raise more fundamental issues" (p. 4), for their use of sources is subordinate to assumptions about American policy profoundly divergent from traditional perspectives. As between Williams and Kolko, I choose Williams for the primacy of his influence (e.g., in a 1971 survey, history professors were asked to name books which had the greatest effect on students "sharply critical of contemporary U.S. foreign policy"; Williams was mentioned almost twice as often as others [p. 163]).

For detailed examination I choose Williams's analysis of reparations, which includes this account of Potsdam:

> "This Council," Stalin remarked in explaining the Soviet view of the conference at its first general session, "will deal with reparations and will give an indication of the day when the Peace Conference should meet." The primary political issue, he continued, was that of dealing with Germany and its former allies. That was "high policy. The purpose of such a policy was to separate these countries from Germany as a great force." Recurring often to the "many difficulties and sacrifices" brought upon Russia by those Axis partners, Stalin argued that the proper strategy was "to detach them once and for all from Germany." As for reparations, Russia would if necessary "compel" such deliveries.
>
> The American response on reparations was crucial to the outcome of the Potsdam Conference, and also, very probably, to the whole course of subsequent events. "Reparations," Byrnes told Molotov on July 20, "do not seem to the United States to be an immediate problem" (W. A. Williams, *The Tragedy of American Diplomacy*, New York, 1962; pp. 246, 247).

Because his government opposed a loan to Russia and developed a "new vision of omnipotence" with the atomic bomb, Byrnes proposed each country take reparations from its zone.

> *Now the fascinating thing is that the Russians fought that proposal for one whole week—from July 23 to July 31—before Stalin finally agreed to it. Even then, he remarked very sharply that it was "the opposite of liberal" (Ibid.,* p. 250; emphasis in original).

Williams explains the American action:

> . . . the United States—confident in its vast economic and military superiority over Russia—made the reparations agreement to avoid any direct financing of Soviet recovery. American leaders were certain that the bomb, and Russia's great recovery needs, provided them with the leverage to re-establish the Open Door, and pro-Western governments, in eastern Europe (*Ibid.*, p. 253).

In short, the United States did not give up anything since, through subsequent exercise of superior power, she intended to regain what she had pretended to concede.

We sense the magnitude of Williams's distortions when we set his account against the documents. As instances: (1) Stalin never made the "speech" "cited." Williams pasted together snippets from remarks on different days and contexts. The result has the "accuracy" of a composite photo in yellow journalism. (2) Stalin's threat to "compel" reparations deliveries wasn't made at the first session concerning Germany but at the fourth concerning reparations due *Yugoslavia* and *Greece* from *Bulgaria (Foreign Relations of the United States, Diplomatic Papers, The Potsdam Conference,* Vol. II, pp. 172, 173). (3) For Williams, Byrnes's words, "reparations do not seem to be an immediate problem," seem an expression of American insensitivity to Russian need. The actual context, as recorded in the official minutes:

> Mr. Molotov stated that somebody must consider the question of Italian reparations. . . .
>
> Mr. Byrnes pointed out that the United States Government has already advanced $200,000,000 to Italy and would probably have to advance $400,000,000 or $500,000,000 more. Therefore reparations do not seem to the United States to be an immediate problem (*Ibid.*, p. 148).

(4) The most revealing illustration of the ingenuity with which Williams's conclusions lead him to his facts is usage of Stalin's phrase, "the opposite of liberal." This was never directed against Byrnes but against Bevin over Russia's percentage share of frozen German assets. The exchange:

> Mr. Bevin said with regard to percentages, we thought we had met you yesterday in the meeting of Foreign Ministers. I agreed to 12½ percent and thought that we had treated your claim liberally.
>
> Mr. Stalin said that it was the opposite of liberal (*Ibid.*, p. 516).

In each instance cited the record clearly sustains Maddox. True, not all of Maddox's charges are comparable in gravity nor in finality of resolution. Occasionally he seems to strain at a gnat; occasionally appeal to the sources leaves the issue moot. But often the distortions are more serious than those cited, and the evidence more conclusively in Maddox's favor. As mentioned, responsible evaluation is possible only if a three-stage procedure is followed in each instance.

It is Mr. Maddox's major contribution to put scholars on notice that serious distortions do occur in revisionist writings and to remind them that those who use these materials uncritically imperil the professions of scholarship. *Caveat Professor.*

WHITTLE JOHNSTON

University of Virginia

Conglomerate Enterprise and Public Policy. By Jesse W. Markham. (Boston: Division of Research, Harvard Business School, 1973. Pp. xviii, 218. $9.00.)

Conglomerates Unlimited: The Failure of Regulation. By John F. Winslow. (Bloomington and London: Indiana University Press, 1973. Pp. xxi, 296. $10.00.)

During the 'sixties there occurred a remarkable surge of "conglomerate" mergers. In earlier decades industrial mergers had typically been "horizontal" (between firms in the same industry) or "vertical" (between firms engaged in different stages of the production process leading to a given product.) Conglomerate firms, on the other hand, result from mergers involving companies engaged in wholly dissimilar lines of output. In the words of the jacket of the Winslow book, "it is possible for a carpeting company, an airline, a meatpacker, and a sporting goods company to be under common management." As the wave of conglomerate mergers became apparent, serious concern came to be expressed in many quarters regarding the public policy implications of the conglomerates. In particular, questions have been raised about the possible need for controls limiting conglomerate expansion and about the adequacy of existing anti-trust legislation to serve as the basis for such regulation. Both these books, additions to the large literature on conglomerate enterprise, have resulted from research inspired by these concerns. But beyond this shared point of departure, the two books have little in common.

Markham, an authority in the economics of industrial organization, has written a careful report, addressed primarily to economists, on a research program conducted at the Harvard Business School; the program was designed to test empirically certain hypotheses with significant public policy implications—hypotheses about how conglomerate firms are managed. The research reported, following a thorough canvass of the existing theoretical and empirical literature, tends consistently to question the validity of many of the arguments advanced by the critics of conglomerate acquisitions.

Winslow served as counsel to the Antitrust Subcommittee of the House Committee on the Judiciary which investigated conglomerate corporations during 1969–70. He has written a book to alert—indeed to alarm—the lay public about what its jacket describes as the "financial and political machinations" involved in conglomerate operations. In the words of the publishers' publicity release, Winslow "reports hair-raising findings . . . names and quotes from company documents."

Certainly Winslow has written an exciting and highly readable book (built skillfully round the materials revealed in a long series of congressional hearings). Whether the quality of its assumptions concerning the social harmfulness of conglomerate mergers is correspondingly high must, however, be seriously questioned.

Winslow persistently ignores fundamental economic insights. This can be perhaps typified by his sarcastic dismissal of a profoundly important observation which he quotes (p. 32), from the so-called Stigler Report. In that report the possibility of reciprocal trading by conglomerates—one of the anticompetitive practices often ascribed to conglomerate management—was considered not to be dangerous. If reciprocity "leads to efficiency," Winslow quotes with disdain from the Stigler Report, "there is no reason to stop it; if it leads to inefficiency there is no reason why the conglomerate should adopt it (since it would reduce its overall profits)." In fact, careful consideration of the conclusions implied by consistent profit-maximizing firm behavior leads Markham early in his book to the tentative suggestion (not subsequently overthrown by the book's empirical findings) of conglomerate "neutrality"—that is, that "conglomerateness" is irrelevant to the issue of possibly intolerable market power, as conventionally defined. Winslow's uncritical assumptions to the contrary appear not unrelated to his apparent lack of interest in economic reasoning.

In fairness to Winslow it should be pointed out that—despite a contrary impression created by his second chapter—the possible "anticompetitiveness" of conglomerates is not (as it is for Markham) his principal source of concern. Winslow is alarmed, rather, by the "financial machination which aids and abets the present trend of industrial concentration" (p. 183); he is alarmed by the harm which such methods of acquisition may work for the acquired companies and their stockholders; and he is outraged by what he sees as the blatant failure of the appropriate governmental bodies (ICC, SEC, IRS, and the antitrust division of the Department of Justice) to enforce the existing laws in respect to the recent wave of conglomerate acquisitions. One can be impressed by much of what Winslow has to say on these matters without subscribing to his apparent general presumption that conglomerate acquisitions must be socially deleterious.

If Winslow's book must be criticized for taking the harmfulness of conglomerate firms entirely for granted, Markham too, appears to have taken the possibility of such harmfulness rather too seriously. He finds it necessary to establish, by elaborate and sometimes ingenious empirical demonstration, the case defending conglomerates from anticompetitive charges. He has been concerned to discover from the facts the extent to which such "anticompetitive" practices as reciprocity and cross-product subsidization are to be found in conglomerate enterprise, and to what extent such enterprise has increased industrial concentration. It is only because his findings suggest no serious tendencies in these regards, that Markham concludes that modification or extension of antitrust laws are not called for by the phenomenon of conglomerate mergers.

What both Winslow and Markham do not seem to take into account is the insight that social efficiency in the allocation of resources is not jeopardized by bigness, or by high concentration ratios, or even by so-called anticompetitive actions; it can be threatened only by artificial blockage to industrial, commercial, and financial entry. Effective competition in the marketplace does not require smallness, nor large numbers, nor the inability of individual market participants to exercise a degree of clout. The powerful and socially beneficial influence of competition requires only that entrepreneurs with novel ideas be free to try them out without fear or favor. From this perspective the concern must surely be not to extend antitrust laws, not to expand the scope of governmental regulation of business—after all, as Winslow recognizes, the surge of conglomerate expansion may itself well have been the result of existing antitrust policies—but to curb their interference with the process of entrepreneurial competition.

The point is *not* that conglomerate enterprise is completely free of potential for harm. The point is that—where such enterprise does indeed spell such harm—market participants be at liberty to seek alternative forms of industrial, commercial, and financial organization. And the point is also that insofar as conglomerate enterprise may just possibly hold some promise of superior efficiency, it not be throttled by well-meaning governmental regulation. From this perspective both these books, each in its own way possessing unquestionable merit, do appear, in some degree, to have somehow lost their way.

ISRAEL M. KIRZNER

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The Urbanization of the Suburbs. Volume 7, Urban Affairs Annual Reviews. Edited by Louis H. Masotti and Jeffrey K. Hadden. (Beverly Hills, Calif.: Sage Publications, 1973. Pp. 600. $20.00.)

During the 1950s social analysts writing for the popular press reacted to the postwar boom in single-family housing construction and land subdivision on the rim of America's central cities by decrying the emergence of a new suburban culture characterized by homogeneity, conventionalism, and mediocrity. In these analyses, the stereotypic suburb was depicted as middle-class, WASP, familistic, and Republican.

The early 'sixties saw the publication of a number of studies which questioned the accuracy of the stereotype. Case studies such as Bennett Berger's *Working Class Suburb* (Berkeley: University of California Press, 1960) and Herbert Gans's *The Levittowners* (New York: Pantheon, 1959) indicated that there were at least some exceptions to the popular image. Demographic analyses showed there exist a variety of community types within most metropolitan areas and that the *way* central cities and suburban communities differ varies among metropolitan areas. Voting analyses explained suburban Republicanism as a result of "selective migration" rather than a consequence of the suburbanite's conformance to the prevailing climate of political opinion. By the end of the decade, social scientists had agreed among themselves that the "myth of suburbia" was itself an interesting phenomenon which told us more about contemporary culture than about suburban development.

Having discovered what the suburban phenomenon was *not* about, urban sociologists and political scientists set out to describe and explain suburban communities, government, and politics. The perhaps surprising conclusion of these rather intensive research activities is indicated by the title of Volume 7 of the *Urban Affairs Annual Reviews: The Urbanization of the Suburbs.* Suburbia, on closer analysis, looks very much like that older phenomenon, the central city. The title of this volume suggests (like the work of many contemporary journalists) that suburbia has now entered a wholly new phase of development in which suburbs have been engulfed by "urban" problems. The contents of this reader, however, belie the title. For a careful reading of this collection of original research on suburban history, society, government and economics indicates that definitional and analytic problems of past research in this area may have obscured similarities among cities and suburbs which outweigh the differences that that research emphasized. Although suburbs may be coping now with many new developmental problems, this research suggests that communities on the rim of the city have always been more "urban" than the earlier literature indicated.

The primary contribution of this volume of reading is not the information on suburban development which is presented—although there is much that is of interest in the discussion of life styles, racial and ethnic movement, policy issues and economics—but rather in the attention given to the selection and definition of critical variables for analysis of metropolitan problems. Generally, *location*—central city or suburb—does not appear as a significant variable. Instead, population density, proportion of housing-units in one-unit structures, age composition, social class homogeneity, and frequency and type of commuting are cited in several articles as critical variables which may affect community life-style. As Harvey Marshall concludes (p. 143) in his review of the literature on suburban life-styles:

> It may be that we are asking the wrong questions—that the question of urban-suburban variations in life styles is irrelevant. Given our interest in determinants of particular life styles, perhaps we ought to be more concerned with specifying and evaluating the types of variables which affect them, rather than with the simple comparison of "suburban" and "urban" behavior—a comparison which inevitably confounds a variety of possible causes.

While location may not be a relevant variable for some types of metropolitan analyses, it may be a control variable in explaining the impact of other critical variables. For example, Thomas Scott's discussion of suburban governmental structures states that four critical variables affect local governmental structure in all types of communities: "state constitutional and statutory requirements and provisions; regional and local custom and tradition; certain relevant and unique local idiosyncracies; and the social and economic structures of the particular community" (p. 227). According to Scott, however, the fourth factor may have a greater effect in suburbs than in central cities because suburbs are generally "newer" and more homogeneous, and their residents are more likely to "vote with their feet" (p. 232). It should be noted, however, that even when as in this analysis, suburban designation is cited as a control variable, one might argue that the critical variables for analysis are date of establishment of the community, homogeneity, and mobility rate. Indeed, Scott concludes: "It should be clear from this entire discussion that it makes very little sense to treat suburban governmental structures apart from all other elements of the local political system" (p. 238).

One interesting example of what can happen when attention is turned from suburban designation to other variables is presented in an analysis of suburban police services by Elinor Ostrom and Roger B. Parks. Starting with the proposition of the Presidential Task Force on Suburban Problems that, in the area of crime, the "distinctive suburban problem . . . as in so many areas, is fragmentation" (p. 367), the authors proceed to formulate four working hypotheses on the relationship between *size of jurisdiction* and *number of police jurisdictions* *within a metropolitan area* and various measures of police service. Their analysis shows that "suburban fragmentation" operationally defined in such a fashion may be positively associated with better police service. Perhaps more important, the analysis indicates the necessity of specifying the analytic variables at issue.

The articles cited above illustrate a theme which recurs throughout this massive volume: Suburban research *per se* may not be a valuable field of research endeavor. Suburban development is an important aspect of metropolitan change. Thus this collection will prove a valuable contribution to courses in urban and metropolitan politics. But the attempt to study suburbs as separate entities or in contrast to central cities has led us to overlook more important variables than suburban designation and has not contributed significantly to our understanding of local communities and politics. It is time we turn our attention to these other variables.

DEBORAH R. HENSLER

The Rand Corporation

Expectations and Inflation: Nixon, Politics, and Economics. By Rodney J. Morrison. (Lexington, Mass.: D.C. Heath, 1973. Pp. 307, $11.50.)

In August 1971 Nixon suspended dollar convertibility, placed a surtax on imports, expanded fiscal policy, and froze prices and wages. The New Economic Policy (NEP), as these policies were then labeled, instituted controls in a period not dominated by a world war—a far-reaching change moving the allocation of resources away from markets and prices to bureaucracy and controls.

The events leading up to and following the August 1971 NEP are, as Dr. Morrison points out, perplexing. Under a conservative Republican administration the U.S. economy has been subject to two economic freezes, export controls and embargos, IRS profits sweeps and audits, the monitoring of price and wage decisions, the spotchecking of private transactions, and a Secretary of the Treasury telling the American public, "People who don't comply are going to be clobbered." And yet the business community, on the whole, applauded Nixon's NEP with many business leaders thinking it was a good and forceful move at a critical time.

That the NEP and its descendants represent a failure of substantial proportions is, as Dr. Morrison suggests, beyond discussion. The U.S. is now completing a decade of inflation. The succession of inflation, recession, and high unemployment, of capacity shortages, and idle plant and equipment, and of inventory accumulation and liquidation is sapping the strength of the American economy—a deterioration that is manifest in the decline of the dollar in the past decade.

Dr. Morrison seeks to assess in this book whether Nixon's economic policy failures reveal the inability of an enterprise economy to cope with inflation, or, alternatively, whether they reflect errors of the Nixon officials responsible for the economic policy.

Dr. Morrison's thesis is that the Nixon administration's original plan of "gradualism" to cool inflation with an orderly slowdown in aggregate demand was a correct and sensible plan. It would have worked if given a chance. Unfortunately, the Nixon economic officials, presumably under White House orders, began to waffle at the first signs of rising unemployment that accompany a successful disinflation process. Official pronouncements to "explain" every increase in unemployment and inflation in 1969 and 1970 were typically contradicted by later facts. As a consequence, the public concluded that the administration was not firm in its commitment to disinflation, that its anti-inflationary posture was more rhetoric than substance, and that it was not to be taken too seriously. The administration and its economic game plan lost their credibility.

As Morrison points out, the Nixon administration's approach to taxes, expenditures, and budget deficits helps explain this loss in credibility. Although he campaigned as a fiscal hawk determined to end the massive Johnson deficits, Nixon soon was running even larger deficits. The Nixon administration statements in 1969 concerning the extension of the surtax, the investment tax credit, the anti-inflation tax package, and tax reform were contradictory and confusing. Even more revealing is the fiscal 1972 deficit. According to Morrison, instead of acknowledging and explaining this substantial departure from Nixon pre-election speeches and Republican doctrine, the Nixon budget officials shifted from the unified to the high employment budget and described a large deficit as if it were a balanced budget. Rightly or wrongly, the public interpreted this as conceptual juggling designed, consciously or not, to cover up a discrepancy between Republican doctrine and administration policy and began to discount official statements, especially those concerning economic policy.

The public's confidence in the Nixon administration's commitment to the disinflation process was further undermined, in Morrison's assessment, by the special treatment advanced for Lockheed, milk prices, meat and textile quotas, steel imports, the oil industry, the ITT mergers, and by its policies concerning Penn Central, the SST, and the ABM. The lack of principle and the opportunism could have confirmed the public's fear that inflation would continue and may have thereby played a major role in the generation of inflationary expectations.

Morrison reviews the administration's pronouncements on economics prior to and after the 1970 election and suggests that they were characterized by a kind of fear and possibly of panic. High officials appeared to be trying to hide, gloss over, or cover up unfavorable, though predictable, economic developments and discrepancies between Republican doctrines and Nixon actions. These efforts to get around unpleasant facts may have further reinforced the public's growing lack of confidence in the administration's anti-inflationary policies. With the benefit of hindsight one may detect in the administration's 1970 behavior the beginnings, or the rough outline, of the syndrome that emerged later in the Watergate scandal.

Dr. Morrison ably supports his thesis that the Nixon administration's shifting actions, policy, and rhetoric undermined its credibility—confirming and reinforcing fears by the public that Nixon would not persevere in the anti-inflationary battle. Even a "gradualist" cooling of inflation would result in a substantial rise in unemployment. Yet very few people believed, as they pondered the administration's changing rhetoric, that Nixon would risk political defeat by continuing with disinflation; they concluded instead that "gradualism" would be dumped and that it would give way to political considerations. The political element was to play an ever growing role in economic policy starting in August 1971, and the successive changes in Phases II through IV may have been designed for short-run political gains, but they can hardly be viewed as flowing from carefully thought-through stabilization policy. Unfortunately, the NEP and the subsequent collection of *ad-hoccery* disrupted the economic system, eroded economic liberties, dislocated industries and firms, generated shortages and miscalculations, and paved the way for double-digit inflation and a severe recession.

Dr. Morrison makes a strong and compelling case for his thesis. I would differ with him only to the extent that I view the economic disaster not only as a Nixon failure but also as a failure of the Republican party and the business community which accepted, and even applauded, the Nixon zigzags. The Republican party failed to realize that bold and dramatic actions without an underlying philosophy are bound to be opportunistic and empty. The Republican party must also be held responsible for Nixon's willingness and ability to institute dramatic and frequent changes in economic policies without a clear conception of what these shifts implied for the viability of the private enterprise system. Nixon's difficulties in economics may magnify and perhaps exaggerate the Republican party's inability to develop a credible economic policy relating theory and practice, but these economic failures were nevertheless apt to occur in the absence of an articulated Republican economic policy.

DAVID I. FAND

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War, Presidents, and Public Opinion. By John E. Mueller. (New York: John Wiley & Sons, 1973. Pp. xxiv, 300. $8.75, cloth; $5.95, paper.)

Few books convey as effectively as this one the intellectual excitement and joy of discovery that can accompany scholarly inquiry. John Mueller communicates these feelings while investigating with poll data the patterns of popular support for the Korean and Vietnam wars and for presidents Truman through Johnson. Many of Mueller's findings force careful reexamination of common misconceptions about these wars and presidents. Although some of Mueller's analyses suffer from serious flaws, others will doubtless serve as the new conventional wisdom for some time to come.

One genuinely provocative finding is a striking similarity in the pattern of popular support for the Korean and Vietnam wars, this despite the demonstrably stronger vocal opposition to the more recent conflict. Mueller bases this conclusion partly on a regression of the poll percentages showing the degree of popular support on the logarithm of the number of American casualties that had been suffered at the time of each poll. The parameters indicate a decrease of about 15 points in the polls for each tenfold increase in American casualties (e.g., from 100 to 1000 or from 1000 to 10,000). Mueller explains this logarithmic pattern by arguing that people react with greater sensitivity to a rise in casualties at the outset of a war, becoming increasingly callous to the human losses as time goes on. Hence it requires ever larger increments of casualties to produce a given drop in public support in the later stages of the wars.

Serious theoretical and statistical questions go unaddressed in this analysis, however. Mueller himself mentions a theoretical difficulty:

four surveys he examined revealed no clear connection between a person's estimate of the number of casualties and his or her attitude toward the war (pp. 62–3). Yet Mueller's explanation of the aggregate pattern he found would seem to rest crucially on such an individual-level relationship.

Another difficulty concerns Mueller's statistical operation. Using casualty figures in their cumulative raw form may mean that the observed relationship between casualties and opinion is simply a statistical artifact, for, in an ongoing war, casualties *do* tend to rise, and empirically, popular support *does* tend to decline. Only if these two phenomena co-vary *after* their respective trend components are removed would a conclusion claiming their causal relationship be warranted.

A number of other findings about similarities between the two wars seem equally noteworthy and are better supported by the data: (1) *college-educated* persons were the *most* supportive of both wars, grade-school-educated persons the least; (2) *younger* persons were the *most* supportive, older persons the least; (3) men were more supportive than women and whites more supportive than blacks; (4) Democrats tended to be more supportive of the wars than Republicans *while a Democrat was president;* many party identifiers changed their attitudes toward the war, however, when the presidency changed parties.

These patterns emerge from Mueller's effort to go beyond the simplistic "hawk-dove" distinction so widely used to summarize attitudes toward the Vietnam war. Here he classifies people according to three "mentalities": follower, believer, and partisan. The generalizations concerning education and age given above exemplify Mueller's notion of a "follower" mentality, that is, "people who take as cues for their own opinion the issue position of prominent opinion leaders," especially the President (p. 122). The patterns concerning sex and race represent "believers," those who focus on the issue itself instead of some external actor. Finally, the "partisan" mentality describes those who adopt the policy stance of the leaders of their party. We are not told, however, either how to tell which party leaders a partisan will respond to, especially when the White House is occupied by the other party, or, more generally, how to disentangle the contrary effects when two or more of these "mentalities" pull in different directions.

A similar confusion attends the application of the "follower" mentality, for Mueller tries to stretch it to cover too many different situations. Early in the book, the author says that followers are cued by various opinion leaders, of whom "the most important by far is the president" (p. 69). Later, however, he explains part of an increased opposition to the Vietnam war by some young people as a "follower" phenomenon, but only by considering as the cue-giving mechanism the belief that the antiwar protests were "special movements of and for the young" (p. 138): Some young people came to believe that their appropriate position was to join their peers in opposing the war. The "follower" notion is similarly broadened both to account for the short-term tendency of people under 30 to criticize the 1970 Cambodian invasion (because other youth were critical), and to suggest the possibility that the widespread hostility toward Vietnam protestors may have reinforced popular support for the war, the protestors serving for many as a "negative reference group." That is, since the protestors were *anti*-war, and they were negatively perceived, many citizens would choose to be *pro*-war. While these several contentions sound plausible, Mueller cannot have it both ways: he cannot have the President act as a cue-giver sometimes, and then explain deviations after the fact by assigning the cue-giving role to some other person or group. A well-developed "reference group" theory would have to specify *a priori* the conditions under which certain persons or groups would cue on a given actor, rather than permit *post hoc* explanations of deviance. Indeed, given the relatively crude poll data Mueller has available, confining the "follower" notion exclusively to presidential cues would seem the wisest strategy.

Turning in the second part of the book from war to presidents, Mueller seeks to explain the change in popular approval of a president during his term of office. To do so he employs four variables: (1) coalition-of-minorities (a president's support should fall off steadily as he unintentionally antagonizes subgroups by his actions); (2) "rally-round-the-flag" (a president's support should climb sharply, then fall off steadily, when he is involved in a dramatic international event); (3) economic slump (a president should lose adherents as unemployment rises); and (4) war (a president should decline in popularity when he presides over a war).

Among the interesting findings and speculations in this section: all postwar presidents suffered from the coalition-of-minorities phenomenon, except Eisenhower (in part, Mueller conjectures, because he didn't *do* anything!); a president loses about three points in the polls for each percentage point rise in unemployment; and although the Korean conflict cost Truman 18 percentage points in popularity, Vietnam had no *independent* effect on Johnson's decline in popularity. This last finding puzzles Mueller, as well it might. But Douglas Hibbs has reanalyzed Mueller's data controlling for the distortion introduced by serial correlation (whose presence Mueller acknowledges at pp. 232 and 238) and found *no such difference* in the effects of the two wars on the popularity enjoyed by Truman and Johnson. Several other patterns reported by Mueller were also altered when the serial correlation was removed. (See pp. 284–89 of Hibbs's chapter in *Sociological Methodology 1973–1974,* ed. H. L. Costner. [San Francisco: Jossey-Bass, 1972].)

The various problems described here serve as caveats in accepting a number of Mueller's conclusions. Yet the study stands as a significant and valuable pioneering effort at time-series analysis of poll data, one which will surely stimulate other studies of the dynamics of American public opinion. Moreover, the book should serve extremely well as a classroom text. Its teaching value rests on its imaginative puzzle-solving approach to important political questions, its lucid and lively writing style, and its skepticism and caution about survey data. Shown repeatedly, for example, are the effects of question-wording on response patterns. For teacher or student, this study will invariably entice and challenge: Read it.

WARREN LEE KOSTROSKI

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The Decline of American Gentility. By Stow Persons. (New York: Columbia University Press, 1973. Pp. 336. $12.50.)

The decline studied in this book—both in terms of the substantive reality and in terms of theorizing about it and perceptions of it—takes the reader from the late colonial period to, roughly, the First World War. By that time, says Professor Persons, the gentility was gone, and "... it served no useful purpose to make the older distinction between gentlemen and common men" (p. v). To be sure, even with the gentlemen removed or replaced, social theorists could conjecture about their demise. Thus Persons's second to last chapter treats "The Heritage of Tocqueville," with at least brief discussions of such modern writers as Hannah Arendt and Philip Selznick. The chapter bearing Tocqueville's name is not the only place in which it becomes apparent that the historical fascination with our renowned French visitor persists. (Cushing Strout's recent book, *The New Heavens and the New Earth: Political Religion in America* [New York: Harper & Row, 1974], has twenty chapters. In nine of those twenty the first word is Tocqueville; in four others that name appears later in the first sentence; and in only two chapters does that name not appear in the opening two paragraphs.) Stow Persons's opening chapter, "Democracy and Gentility," which establishes the major themes, definitions and tensions, derives in very large part from Alexis de Tocqueville.

The subsequent two chapters—"An American Theory of Manners" and "The Natural Gentleman"—offer elaborations of gentry conduct, including wholesome avoidance of artifice and reverence for simplicity, if not rusticity. "The Gentry and American Society" analyzes some of the inherent problems in maintaining the gentle way—problems such as the tenuousness of the nuclear family and the transition of the gentlewoman into lady writer. The long and well-written chapter on "Gentry Politics" centers upon the Liberal Republican persuasion and movement, and it is perhaps the most familiar part of the book. "Ancients and Moderns" moves on to the late nineteenth-century controversy regarding education, with Charles W. Eliot and the Harvard curricular changes pitted, at least symbolically, against Noah Porter and the efforts to perpetuate the old ways at Yale. And as theorists reflected, conjectured, and quarreled, more concrete alterations eroded the gentry class and marked its passing, perhaps to be replaced in the twentieth century by the "alienated intellectual" (p. vii). The New Rich, a seemingly endemic nervousness in the now genteel ranks, and fundamental changes in ethnic patterns came as unanswerable tokens of the demise of the gentry and with it, the demise of liberalism. "The life span of gentility in the mass society virtually coincided with the rise and fall of liberal individualism" (p. 301).

Two aspects of the book aroused my misgivings. Inevitably, problems of definition arise in a work treating such amorphous entities as "the gentry." And perhaps inevitably, the selection of illustrative thinkers will dissatisfy some—why, I wondered, was there no Santayana and "The Genteel Tradition" or Albert Jay Nock and *The Memoirs of a Superfluous Man.* But the strengths of the book are much greater than these almost inevitable drawbacks. *The Decline of American Gentility* is a rigorously thought out and finely written synthesis, a particularly strong feature of which is the lucid analyses of such key thinkers on the subject as Edwin Lawrence Godkin, William Dean Howells, William Graham Sumner, Henry Adams, and Charles Eliot Norton.

LEWIS O. SAUM

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Administrative Alternatives in Development Assistance. By Gary S. Posz, Jong S. Jun and William B. Storm. (Cambridge, Mass.: Ballinger Publishing Co., 1973. Pp. 125. $15.00.)

"There's something about an Aqua Velva man," say the TV commercials, with a clear implication that the "something" automatically makes the Aqua Velva man a prize catch, far superior to ordinary mortals. Similarly, there is something about the professional man in field missions of the Agency for International Development (AID) that, according to the authors of this study, makes him a prize catch and requires as a condition for his effectiveness (or as a concession to his idiosyncrasies?) a radical restructuring of the agency in which he works.

The professional in question is the technical assistance specialist in agriculture, education, engineering, and the social sciences. A total of 169 AID professionals in these categories was interviewed at length and in depth during the mid-1960s. The evidence is said to show "clearly" that

> in the minds of thousands of professional men and women who served in technical assistance advisory roles overseas the lack of organizational accommodation to them in their professional role was the most damaging aspect of their experience. The highly bureaucratic context within which they worked simply prevented most of them from achieving their normal level of effective professional performance (p. 3).

As an alternative to "the traditional Weberian-Scientific Management model of organization as it is reflected in present development assistance organizations," the authors recommend Larry Kirkhart's "consociated ideal type of organization" (p. 101). The consociated model has a structure

> founded on the project team as the basic work unit. Each team has financial authority and is interdependent with other project teams. Authority in the system is multivalent, characterized by temporary hierarchy, situational leadership and diverse patterns among project teams. Time imperatives govern the total organization; each project team dissolves upon completing its project. . . (pp. 103–5).

The authors—a foreign service officer with a Ph.D. in public administration, a professor of public administration, and an associate professor thereof—are serious scholars sufficiently steeped in the jargon of their trade to ensure that no public official in a position to do anything about their findings will read them. This may be unfortunate, but I am not sure that it is.

The AID of the early 1960s was a surprisingly innovative agency. It was also a cumbersome bureaucracy. It needed streamlining for a variety of compelling reasons, only one of which was to give key professionals at the far end of the line greater freedom to "do their thing." Development—forgive the hackneyed adage—is too important to be left to the professionals. As one can discover from its annual reports to the Congress, though not from this study, the AID at the time of the interviews employed directly more than 15,000 persons, of whom about 10 per cent were technicians in the four fields from which the interviewees were selected and another 10 per cent were performing similar work in other disciplines. There is no evidence in this study to indicate that the technical assistance professionals in AID were any more frustrated by the constraints imposed upon them than were the nonprofessionals or the several categories of professionals—lawyers, economists, area specialists, etc.—not employed as technical assistants. In all probability there was widespread frustration among committed employees at all levels, but the authors' data do not seem to validate sweeping generalizations. Of the 196 respondents, 108 believed they had been allowed sufficient discretion in planning and carrying out their work. Only 41 saw "excessive bureaucracy" as a chief problem of the U.S. Government.

In any event, time and circumstance have transformed and slenderized the old AID almost beyond recognition. The authors frankly concede that their study would already be obsolete if it were viewed simply as an effort to improve the functioning of large U.S. aid missions. They hope it will be viewed more broadly as a contribution to the problem of building organizations in which the knowledge and creativity of the professional person can be applied to the development of the low-income world without interference and frustration from bureaucrats within the organization. Though this identification of the professionals as the good guys and the bureaucrats as the bad guys strikes me as simplistic, for certain kinds of undertakings there is real merit in the team scheme which the authors solemnly bless as "more congruent with contemporary organization theory as well as with the needs of professional employees" (p. ix).

ROBERT E. ASHER

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Governing Metropolitan Toronto: A Social and Political Analysis, 1953–1971. By Albert Rose. (Berkeley: University of California Press, 1973. Pp. 224. $8.95.)

The Toronto system of metropolitan area

government, instituted in 1953 by a Conservative provincial premier who controlled the parliamentary channels of power, has intrigued North American students of metropolitan politics during the twenty plus years of its operation. A federation of local authorities with substantial fiscal and planning powers, it came into existence at a time when population growth and urban sprawl were threatening to get out of hand. This most recent study of the Toronto experience is the work of an advantageously placed participant observer. He directed research for alternatives before the official plan was promulgated, has headed a community planning association, and been chairman of the official metropolitan housing authority. In appropriate recognition of social science research requirements, he warns the reader of his perceived biases and his philosophical premises. The book is neither an apologia for an official scheme, nor the diatribe of a disappointed partisan. It is a thoroughly researched, briskly written account of the events and issues which occurred in the development of the government of this major Canadian metropolis.

It sketches briefly the background of conditions that led to the creation of "Metro" in 1953, then proceeds to analyze its operation during three periods. Two are associated with the dominant figure of Frederick M. Gardiner, the first chairman of the Metropolitan Council. The first was one of institution building and physical construction; in the second, mounting tensions between the representatives of the city and the suburbs produced new efforts to achieve political reorganization. The third covers the administration of William R. Allen, who succeeded Gardiner. The author's interest in planning and housing gives the book a broader scope than the politics of metropolitan governmental structure. Two chapters discuss city and regional planning, and one treats slum clearance, public housing, and urban renewal. The final chapter is a comprehensive survey of major issues that press for decision in the 1970s: the role of the Metro chairman, the geographic scope of the planning area, the amalgamation of the City with its suburbs versus continuance of the two-tier metropolitan structure, governmental interaction with newly emerging neighborhood groups, and the probabilities of local political parties becoming meaningful entities in metropolitan decision making.

The author was initially a believer in the federal metropolitan system as a utilitarian program for delivery of services to the population of a rapidly expanding metropolis. As he examines the operation of that system he becomes critical, although not to the point of advocating its abandonment. He makes a persuasive case that the Metropolitan Council has chosen to emphasize physical development and the financing of area-wide activities, and has neglected the social and human concerns of a considerable portion of the population. Some of the most interesting reactions to Metro have come from middle-class neighborhood groups which have been politicized in their fight against the official bulldozer and the builder of highways and high-rises. The now familiar story of the elderly and impoverished being crowded out of affordable residential space is told soberly. A more unique theme is that the central city is being neglected by the metropolitan government, despite the encouragement of commercial and governmental rebuilding. The central city is not suffering physical deterioration, but the quality of life, particularly for its less affluent citizens, is being threatened.

The author has done a very capable job of organizing his data and keeping several analytical threads going throughout the book. Themes are clearly stated, and subheadings enable the reader who is not familiar with Toronto affairs and personalities to understand a complex story. Professor Rose also manages to keep a good balance in his treatment of institutions, processes, groups, and leadership figures. If there seems to be some deficiency in the discussion of the common man's reaction to metropolitan government, the lack may result from the fact that the Toronto political system has not developed a means to articulate this reaction rather than from any fault in the author's method or perception. Protest and criticism come mainly from middle-class groups. Since the book is largely a case study, little effort has been made to compare Toronto's experiences with those of other urban centers, yet the book is far from parochial. The one shortcoming is that the book does little to explain the realities of Ontario Province's parliamentary and bureaucratic politics which seem to impinge on the Toronto political system at every turn. Within its framework, however, this is an excellent book.

WINSTON W. CROUCH

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Educational Policy-Making and the State Legislature: The New York Experience. By Mike M. Milstein and Robert E. Jennings. (New York: Praeger, 1973. Pp. xiii, 156. $15.00.)

In its narrative sections, this is a useful addition to a growing list of books and articles that address themselves to the politics of education. Focusing on the educational policy process at the state level in New York, the authors describe in brief compass the various actors, structures, and processes that determined educational outcomes in the late 1960s. After some general remarks about educational policy-making and about the special setting of New York State, the monograph takes up seriatum the State legislature, the Governor's office, the Regents, and educational interest groups. Two case studies comprise chapter seven. Chapter eight is an attempted summary and synthesis built around systems theory.

In the descriptive sections of the book, the richest chapter is the one on interest groups. The poorest one is on the legislature. Survey techniques still suffer from the perversities and sensitivities of respondents. Putting it bluntly, on the basis of six years of direct and intimate observation, I simply do not believe the legislative responses to the authors' questions. Self-delusion on their part? Perhaps. Misperceptions on my part? Perhaps. Induced answers by authors' questions that lead the witness? Probably!

There are a few factual errors. The Regents themselves had no dealings with the governor's staff during the period covered by the book. Members of the "input sector" do not by and large review the policy products—at least in education—in any systematic way.

The most disturbing aspect of the book, however, is the authors' constant and maddening hedges: the legislature is the focal point in the policy process, but the governor is the focal point in the legislative process; the Regents are the educational leaders in the State, but they are low men on the political totem pole; interest groups are powerful, but they are not as powerful as they once were, except that the new United Teachers may become powerful enough to change the nature of the policy struggle; systems analysis is a great methodological tool which can be used to explain phenomena and some day may help us predict policy outcomes, except that it has not yet proven itself able to guide the collection of data, and the exact mix of interdependence among actor groups is "impossible to forecast." Etc., etc., etc.

Like all attempts to systemize the fleeting contingencies of public affairs, this study helps by naming the important actors and suggesting some samples of interaction. But like the systems theory it both espouses and cautions against, it is light-years away from catching the chemistries, spontaneities, procedural accidents, shifting intensities, and secular drifts that condition the real policy process.

STEPHEN K. BAILEY

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Military Force and American Society. Edited by Bruce M. Russett and Alfred Stepan. (New York: Harper and Row, 1973. Pp. 371. $18.00.)

This is a book that betrays the passage of time. In the nearly twenty years following the publication of Samuel Huntington's *The Soldier and the State,* studies of the professional American military treated their subject as a body of autonomous and responsible public servants. This approach was shattered by the Vietnam War. Now we have this collection seeking to start again where Huntington left off, namely, to examine the nature of the relationship between the soldier and society.

There are five essays: two ideological, i.e., attempts to define what the soldier or the society should be; three substantive, i.e., descriptions of how in the context of soldier-society relationships, particular decisions are made. In addition, there is an exhaustive bibliography.

The ideological essays are the most interesting. The authors stand poles apart; one, Captain James S. Dickey is a military intellectual; the other, Douglas H. Rosenberg, a peace activist. Their views about the social utility of organized violence differ accordingly, but both share the romanticist concept of the military calling. The honorable soldier has the alternative of either being a member of a classless society or of creating a society-free class. Both agree that it is the armed forces that are affected most directly by the crimes of society.

Dickey adheres closely to the Huntingtonian model. For him, professionalism is a total vocation. The career officer should be so committed to his role as to have no incentive to involve himself in nonmilitary matters. The interests of other groups should be a matter of indifference to him.

But the question arises, how does the officer's role create a personal ethic? For Dickey, the crux of the problem lies in the dialectic of "honor" and "duty." Honor resides in the sense of being bound by one's oath; duty in the obligation to carry out orders. Honor may oblige an officer to refuse orders that appear to him unethical.

This places a heavy burden on the conscience of the officer. Ethical considerations have rarely played a decisive role in military behavior. Historically, honor and duty form an odd couple. The traditional impulse for a military vocation arose from the union of the concepts "station" and "duty." That is to say, a member of the elite is obliged to risk his life for his sovereign. Honor is an elite attribute. But it got in the way of duty. Out of honor, an officer might refuse to take orders from officers of inferior social status.

Dickey accepts the fragile nature of his honor-duty complex. He believes that it will flourish to the degree that military responsibilities are divorced from their social and political context. The army he envisages is one functioning along the lines of Janowitz's constabulary model, with a strong middle class enlisted contingent and an officer corps dominated by West Point graduates. He seems to hope that the middle class ethic will take on the role formerly given to the aristocratic code. He wants the army to become a trade rather than a service.

Rosenberg's approach is utopian. In a class-dominated society, he asserts, the armed forces are bound to be the instrument of an expansionist ideology. His solution is to redirect social energies along Reichian–Charles, not Wilhelm–goals; away from consumerism and corporate competitiveness. It is Rosenberg's contention that the behavior patterns of the United States in the post–World War II era have been determined by an ideology that ties the general welfare to the American role as the bastion of the "free" world. Under this interpretation the American armed forces are not so much wicked as the slaves of a master whose goals have no limits.

Arguments of this kind have a built-in persuasiveness, created in part by their evident highmindedness and in part by the reader's awareness of how much has gone wrong. But the nature of Rosenberg's terminology creates doubts. His use of the concepts "ideology" and "myth" seems to me to be simple-minded. They cover any notion of common interest and any set of widely held beliefs. His schema allows considerations of righteousness and greed, but gives no role to stupidity and incompetence. It credits more intelligence to our recent leaders than a detached examination of their actions would perhaps sustain. He attributes to the American ideology and myth a degree of coherence and incantatory power strikingly lacking in his actual description.

Evidence to this effect is provided by the three analytic essays. Abercrombie and Alcalá examine professionalism through a fifty-year content analysis of the service journals. They reveal a steady increase in politically oriented articles, with the greatest increases during peace intervals, but with a progressively higher floor being maintained during successive periods of overt hostilities. They conclude that the competition for peacetime allocations leads the army to emphasize its political role. They also concede that the emphasis on nonmilitary factors may run counter to the cause of objective professionalism.

Jung Ryool Lee in an analysis of U.S. Budgets in the period 1945–71 found that the objectives of social welfare and economic growth were inversely related. This dichotomy has been hidden by the favorable international position of the American economy. But as that advantage disappears the choice between the "welfare state" and the "warfare state" will be forced upon us.

Wayne Moyer's analysis of House voting on defense in the 90th and 91st Congresses uncovered a pattern strongly linked to ideological habits. The greatest shifts towards limiting defense expenditures came from congressmen with strong civil rights voting records. The most persistent support of high military budgets came from congressmen first elected during the McCarthy era. The pattern of voting followed the lines of a liberal-conservative division.

The overall impression gained from this book is that the motives in the various segments of military behavior have been expediential. Officers followed the line of politicization when it enhanced their career prospects. When it began to pose a threat to their status in the system, they came to see the advantages of the other extreme. Heavy defense expenditures were accepted as long as the pie seemed big enough for everyone. When the limit of the number of slices became evident, opposition to defense spending arose. If there was anything like an American ideology it could be described as a belief in a limitless future. Now that the prospects are less rosy, a more coherent system may emerge.

M. D. FELD

Harvard University

The American Conscience: The Drama of the Lincoln-Douglas Debates. By Saul Sigelschiffer. (New York: Horizon Press, 1973. Pp. 488. $12.95.)

Sigelschiffer writes that "the clash between Abraham Lincoln and Stephen A. Douglas is an epic of American history, illustrative of the struggle of the national conscience over slavery" (p. vii). Sigelschiffer's purpose is to recreate, insofar as possible, the forceful drama of the Lincoln-Douglas debates in a way that demonstrates the vitality of the issues involved.

With respect to the main issue of expansion of slavery into the territories, the two antagonists squared off in the following manner:

> Douglas maintained that the founding fathers had envisioned a republic composed permanently of free and slave states. Each newly acquired territory, therefore, should be permitted to choose which status it preferred. This he called the theory of "popular sovereignty." Lincoln, on the other hand, believed that the founders anticipated the future abolition of slavery, which could never take place unless territories were kept free until subsequently admitted as free states (p. 16).

In Lincoln's estimation, northern acceptance of Douglas's doctrine of popular sovereignty would have been the greatest disaster that could have befallen the American people, an even *greater* disaster than civil war. Lincoln often said that the Founders had put the public mind at rest in the belief that slavery was "in the course of ultimate extinction," that where it was tolerated it was done so only as a necessary evil. But the Kansas-Nebraska Act, whose chief architect was Douglas, had declared "inoperative and void" the provisions of the Missouri Compromise of 1820 which prohibited slavery in the Louisiana Territory above 36'30". It was this act, Sigelschiffer rightly notes, that brought Lincoln "who had been out of politics since 1849 because of his unpopular stand against the Mexican War while in Congress . . . back on the political scene" (p. 40). Douglas expected his "popular sovereignty doctrine" to replace the Federal proscription of slavery contained in the Missouri Compromise Act. That is, the people of the territories, when they deemed it in their interest, could, by local majorities, "vote slavery up or vote it down." This would, as Lincoln pointed out, treat human freedom as if it had no greater moral basis than local self-interest.

Sigelschiffer cites a newspaper reporter's description of one of the many colorful banners that gave a festive atmosphere to the fifth Lincoln-Douglas debate. "It was," the reporter wrote, "a representation of a two donkey act, or Douglas attempting to ride Popular Sovereignty and Dred Scott. His straddle is remarkable but not equal to the task as both animals kicking up their heels send him sprawling" (p. 305). And, it was precisely Douglas's attempt, Sigelschiffer indicates, to make his two-donkey act appear a one-donkey act that was exposed by Lincoln during the course of the debates. Lincoln's challenge effectively prevented Douglas from assuming the leadership of the Republican party at a time when he was at the peak of his political powers, and created a split in the Democratic party which paved the way for his own presidential candidacy in 1860.

Lincoln saw in the Kansas-Nebraska Act a "conspiracy" to nationalize slavery. That act had declared that states *and* territories were subject to the Constitution; and, what the Constitution required with respect to slavery in the territories was given an authoritative interpretation by the Supreme Court in the Dred Scott decision (pp. 52–54). Lincoln maintained, however, that Douglas's squatter sovereignty had been "squatted out of existence" by the Dred Scott decision. It was no longer a sacred right of popular sovereignty to "vote slavery down" since the Supreme Court had declared that neither the Federal Government nor a territorial government had the power, under the Constitution, to exclude slavery. Lincoln maintained that the *nationalization* of slavery awaited only one more decision, a decision based on the reasoning of Dred Scott declaring it unconstitutional for a state to proscribe chattel slavery. Lincoln declared in the "House Divided" Speech that "We shall lie down pleasantly dreaming that the people of Missouri are on the verge of making their State free; and we shall awake to the reality, instead, that the Supreme Court has made Illinois a slave State." Lincoln called upon the people to return to the "ancient" faith of the Declaration and reaffirm their commitment to equality. Only a passionate attachment to the reasonableness of the equality doctrine of the Declaration, he believed, could atone for the transgressions of the Kansas-Nebraska Act and the Dred Scott decision.

In 1858, the American people took partisanship seriously. More than 15–20,000 people, some coming from considerable distances, would assemble to hear a political debate lasting upwards of 5–6 hours. To us today, whose political vitality has been sapped by the uniform monotony of the mass media, these people must seem like a race of political giants. Sigelschiffer recaptures some of this excitement that made politics the number one spectator sport in the nineteenth century as he presents a lively and readable account of perhaps the most important election that has occurred in American history.

EDWARD J. ERLER

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Public Employee Unionism: Structure, Growth, Policy. By Jack Stieber. (Washington, D.C.: The Brookings Institution, 1973. Pp. 256. $7.95.)

Growth in public employment and public union organization during the past decade has sparked expanded research and publications. To contribute to an understanding both of public union and employees' organizations and collective bargaining, Brookings sponsored this volume as one in its series of Studies of Unionism in Government.

The title is broader than the contents. The book does not examine *all* types of unions and employee associations at *all* levels of government, nor does it show how the structure, growth patterns and policies of *all* public unions shape collective bargaining. It covers public employee organizations in state and local governments, excluding school instructional personnel (a substantial percentage of state and local employees), transit, and federal employees.

Reflecting the excellent features expected of a Brookings study, this volume is based upon approximately 300 interviews with union and association representatives and government officials in 55 cities and 23 states and data collected for other studies in the series. Most useful are the Appendices; Appendix A summarizes a 74 per cent response from questionnaires mailed to 2,072 cities with 10,000 or more population covering local employee representation, employee representation and union membership, employee representation and state legislation, and public employee organization. Appendix B has six statistical tables covering membership of state employee associations for selected years and various aspects of representation elections among local government employees.

The volume is informed and informing, factual, tightly structured, and nondoctrinaire. It has no undergirding conceptual framework and extrapolates essentially descriptive inferences and conclusions from the data. Its author, Dr. Jack Stieber, Director of the School of Industrial Relations and Professor, Department of Economics, Michigan State University, has rich and varied experience as researcher, writer, teacher, and arbitrator in private and public sector unionism.

The opening chapter categorizes public unions and employee organizations: all-public unions, mixed unions (majority of members from private industry), uniformed protective services (police and fire fighters), state and local employee associations, and professional associations (NEA, AAUP, and American Nurses' Association, ANA).

For industrial unions and associations, uniformed services, and ANA the text discusses structure, minority-group participation, leadership and finances; using the above typology, separate chapters consider conflict and cooperation, collective bargaining, bargaining unit and representation election, the strike issue and political action.

The bulk of the research and writing in public employee bargaining is being done by economists. In light of the close relationships among government, politics, and public sector bargaining, and since the core concepts of political science (power, constitutionalism, sovereignty, pluralism and interest groups) should facilitate research, one would expect political scientists to be involved in such studies. But this is not the case. New York's Taylor Act relied upon a report prepared by four labor economists and a lawyer.

This point is illustrated by the chapters on "The Strike Issue" and "Political Action." In the former, there are at least two political issues: whether a strike by local employees distorts the political process, and whether a legislature or school board should resolve finally disputes when earlier procedures are unsuccessful. Our understanding of public-sector bargaining would be improved if political scientists applied their concepts and knowledge to this area. Although Dr. Stieber has discussed them elsewhere, neither the "Strike" chapter nor the "Emerging Patterns" chapter deals with them.

Similarly, the chapter on "Political Action," which notes accurately that public unions supplement their bargaining with political activities, fails to mine the rich ore of public union politics. Where there are no explicit statutes insuring bargaining, and a mixed union represents employees in a party-dominated, patronage municipality, union political behavior differs significantly from that of all-public unions in a council, city manager, merit-structured government. Also, the intensive and extensive reliance upon lobbying and multilateral bargaining deserves study and analysis by political scientists.

None of the above is intended to fault the author but emphasizes that understanding of public bargaining would benefit from an infusion of political science and political scientists.

Within the scope of its organization and goals, this volume is a constructive contribution to the growing literature. The appendices should be helpful. All of us can be grateful to Brookings and Dr. Stieber for focusing our attention upon the forms, development and policies of public unions and employee organizations.

BERNARD L. SAMOFF

University of Pennsylvania

The Dynamics of the Party System: Alignment and Realignment of Political Parties in the United States. By James L. Sundquist. (Washington, D.C.: The Brookings Institution, 1973. Pp. 388. $8.95.)

James L. Sundquist set for himself the enormous task of examining the history of American political party conflicts in order to discover the characteristics of party alignments and realignments associated with the critical elections of 1860, 1896, and 1932. Where possible he relied on good existing research for his historical material. Following his analysis of the three major cases of realignments and some lesser ones he sets forth sixteen generalizations plus some sub-generalizations all of which "are intended to describe not the cases as such but realignment as a phenomenon—in other words to apply to *any* (his italics) major realignment of the American party system, whether past, current, or prospective." After stating his generalizations, Sundquist adds three more interesting chapters. In the first of these he discusses the issues that have cut across existing major party cleavages since the last major political realignment—communism, race, the Vietnam war, and law and order and the "social issue" first publicized by Richard M. Scammon and Ben Wattenberg. He then examines the current status of the party system and finally he offers a forecast of political things to come.

The author suggests that the three major alignments illustrate possible different outcomes of the process. In one "a third party, Republican, supplanted an existing party. . . . The second was marked by the absorption of an important third party, the Populists, by one of the existing parties. . . . The third realigned the two existing major parties. . . ." While the process was different in each case all of them took two or three decades to complete. The first two involved long years of buildup before the advent of the "critical" election while the third came very quickly with the onset of the great depression but then required three decades "to work its way through the decentralized American party system." Two fascinating historical chapters trace the "second stage" of the last realignment. The first describes the development of competitive major parties in the previously predominantly Republican states of the upper midwest and west. This development involved the weakening of the liberal wing of the Republican party and the rise of "programmatic liberals" within the Democratic party. Liberal Democratic senators from those regions today are, the author suggests, the ideological heirs of the Progressive Republicans of the earlier era. The second chapter traces the corresponding change over the last quarter century in the South as it has shifted from almost complete Democratic party domination to a region which voted solidly for Richard M. Nixon in 1972. Presidential Republican voting rose first in the "rim south" and in the metropolitan centers and later spread to the Dixiecrat strongholds of the deep south where dissatisfaction with the national Democratic party had already manifested itself in the Dixiecrat revolt of 1948. These political changes in both the North and the South have created parties more clearly divided along economic or class lines.

While there are many variables which help to determine the form and scale of realignments, Sundquist suggests that five stand out—"the breadth and depth of the underlying grievance, the capacity of the proposed remedy to provoke resistance, the motivation and capacity of party leadership, the division of the polar forces between the parties and the strength of the ties that bind voters to existing parties."

Any book as wide-ranging and as detailed as this one is almost certain to interpret some events or institutional developments in a manner with which other students will disagree. Some may even object to the effort to generalize for all time from only three case studies, but surely most political scientists will welcome this willingness to generalize even if future re-alignments force revisions in some judgments.

It is my own view that the author may not have recognized or at least not stated fully the differences between third parties in the nineteenth century and those which have arisen during the past half century and therefore still suggests for them a role which in fact is very unlikely. The third parties of the nineteenth century fit almost everyone's definition of a party or an incipient party. Most of them created local organizations and fielded candidates for local, state and congressional offices in order to build a base from which to compete at the presidential level with the existing major parties. Quite by contrast third parties in more recent years are not "parties" at all but merely independent candidates for the presidency who discourage candidates at the local level and never leave the safety of their chosen major party at home where they are not considered extremists but actually lead the mainstream of that party. There they expect to continue to run for public office whenever the opportunity presents itself. Institutionally the direct primary has obviated the need for third party alternatives to the major parties at all levels except that of the presidency. (New York state is an exception because constitutional and legal provisions virtually assure the development of a third and fourth party.) If my judgment is correct then Wallace voters did not, as Sundquist suggests, "return" to the Democratic party after 1968 because they had never left it—as indeed neither had Wallace whose electors appeared on the Alabama ballot under the symbol of the Democratic rooster. Even if voters did split their ticket to vote for Nixon in 1972, most of them saw no pressing need to switch parties at other levels because the Democratic primary elections allowed them to nominate candidates whose views on divisive issues matched their own. The direct primary has made possible the satisfaction of voter wishes within the local sections of the major parties without resort to the third party. It may have been an important reason that third parties played no role in the major 20th century realignment and a reason to expect that later realignment will be accomplished inside the Republican and Democratic parties.

Other minor questions can be raised about some of the author's statements, but none will greatly detract from the value and importance of the book. It makes a major contribution to the understanding of the political process in America.

HOWARD R. PENNIMAN

Georgetown University

The Politics of Massive Resistance. By Francis M. Wilhoit. (New York: George Braziller, Inc., 1973. Pp. 320. $8.95, cloth; $3.95, paper.)

This book is intended primarily for the general reader. And here it is at its best. For it offers a readable, informative, and balanced account of Southern resistance to desegregation following the Supreme Court's 1954 decision in *Brown* v. *Board of Education.* Although Professor Wilhoit is particularly concerned with the resistance movement itself, his treatment is so comprehensive that the book is a useful introduction to the more general study of desegregation politics in the South.

Much of the book is taken up with discussion of the origins, organizational components, leadership, ideology, "iconography and demonology," and strategy and tactics of the southern resistance movement. Following separate treatment of each of these topics, there are case studies about school desegregation in Little Rock and New Orleans and about the admission of black students at the University of Georgia, Ole Miss, and the University of Alabama. Information is also included about action in the courts and in Congress—the latter incorporating summary descriptions of civil rights legislation enacted since 1957—and about the position taken by post—World War II presidents.

Professor Wilhoit attributes the rise of the Massive Resistance movement in large part to steps taken by the South's political leaders. The failure of President Eisenhower in the period following the *Brown* decision to support it publicly is also stressed. Wilhoit implies at several points in the book that the mass of white southerners would have accepted desegregation without overt resistance—albeit reluctantly—had it not been for the character of their political leadership. On the other hand, he gives considerable attention to cultural, ideological, and institutional factors which, it would seem to me, predisposed the South to some form of resistance. In any case, the material about southern ideology, myths, religious beliefs, and symbols, and about the importance of *machismo* in southern politics constitutes the major contribution of the book.

This review of the contents makes clear the range of subject matter which is encompassed in Professor Wilhoit's volume and thus may indicate its weakness as a systematic study of resistance politics. Despite his understanding, the various topics which Wilhoit treats are not held together by a coherent thesis. The volume is replete with interpretations and generalizations, most of which seem to rest neither on empirical findings nor on deductive reasoning. Furthermore, Dr. Wilhoit does not really accomplish the tasks he sets for himself at the beginning of the book: to describe the development and evolution of the Massive Resistance movement and to analyze the "interrelationships of mythic ideas and political action" in each of the movement's stages (p. 9). One searches in vain for analyses of the interrelationships between ideas and action in each of the movement's stages. In fact, despite such general characterizations as "first reactions," "rise of Southern resistance," "critical confrontations," and "abatement of massive resistance," there is no meaningful identification of "stages" of the movement.

Thus, it is evident that from the standpoint of political analysis, the book has a number of deficiencies. According to the title, the book is about the "politics" of massive resistance. Yet, it is not clear what the author's concept of politics is. In Chapter 11, the term is used in its more limited, popular sense (i.e., the politics of parties and elections). But the variety of topics covered by the book as a whole suggests that Wilhoit actually has a broader conception in mind. It is also disappointing that he has not done any original field research of his own. In his preface he suggests that by its nature his book did not require elite interviewing. And this, of course, returns us to the point made at the beginning of the review. The book is intended primarily for the general reader. Political scientists will find it valuable principally as a source of generalizations which may be subjected to more rigorous examination and systematic testing.

JOHN L. FLETCHER, JR.

Boston University

Memoir of a Revolutionary. By Milovan Djilas. Translated by Drenka Willen. (New York: Harcourt Brace Jovanovich, Inc., 1973. Pp. 402. $12.00.)

Memoir of a Revolutionary, originally written in manuscript during the late 1950s but published in English only in 1973 (a fact of historical interest which might have been mentioned by the publisher), is the second in a series of autobiographical works by Milovan Djilas which began with his *Land Without Justice* (New York: Harcourt, Brace, 1958). The present work opens with the young Djilas's departure from the "heroic reality" and legends of his rugged childhood home of Montenegro, culminating in his final transformation into a "professional revolutionary" who has discovered another reality, equally heroic, with which he totally identifies. The author's narrative skill and beauty of style, his ability to portray individual drama and to convey to the reader a sense of revolutionary passion and conviction—all unsurpassed in works of this genre—unquestionably place this book firmly in the great tradition of the telling of history by its participants.

Djilas's most important accomplishment is to offer us a portrait not so much of an individual revolutionary but of the soul of one of the most vital national Communist movements of the twentieth century. The members of that movement, he observes, were originally motivated to join it not out of intellectual conviction or hope of material gain, but because for them the act of revolution seemed to represent the most adequate response to individual emotional crises generated by an elemental repulsion toward the world in which they lived. In most cases, only after an individual "became psychologically oriented within the movement" (p. 93) did theoretical Marxism play a significant role, and the essence of this role was to "strengthen" and "purify" rather than to convince. Indeed, the nature of this relationship between revolutionary theory and practice helps to account for the dogmatic and exclusive tenor which the former almost always assumes in such a context. The intense "emotional" bonds within the movement—"a warm camaraderie and love, of a kind customary in traditionalist families and religious communities" (p. 320), intensified "to the point of self-obliteration"—"played a more important role in the creation of the Communist movement and party than the revolutionary Marxist ideology, with all its pretensions of a universal understanding of society, the world, and man. In fact, revolutionary Marxist ideology adjusted itself to the emotional needs of revolutionaries, helping them mature rapidly" (p. 95). The attraction of Stalinist dogma lay precisely in its insistent self-sufficiency, its claims of universal explanatory power, and its exclusiveness. In these ways it was perfectly suited to the need of a revolutionary movement for a comprehensive and durable worldview that would complement and reinforce its own spontaneously generated, rigid code of conduct.

The heart of Djilas's narrative is his account of prison life and the importance of the prison experience for the entire structure and outlook of the Yugoslav Communist movement. Prison emerges as a microcosm of the larger sociopolitical system, replete with class struggle, party politics and factionalism, conflict and alliance among diverse political currents, an educational system, and even crude arrangements (with the assistance of the sewage system) for the distribution of food and other scarce resources. A hunger strike could assume global significance, being judged in the light of the international position of the USSR and the general crisis of world capitalism. "In this crowded space, in a community made up exclusively of Communists, we were building up our—closed Communist—world, in a far more intensive and complete form than the party organizations outside" (p. 228). The daily hardships of prison life drew this community of revolutionaries together as could no other force and imposed on it an internal, collective moral discipline so strong that it endured long after prison sentences had ended. Indeed, it does not seem too much to say that the prison experience provided the primary impulse for a revolution in party norms, without which the subsequent "Bolshevization" of the Yugoslav Communist Party would have been much more difficult and painful than it actually was. Unlike the pathetic victims of Solzhenitsyn's world, the Yugoslav Communists—who often suffered brutalities no less grotesque than the citizens of Gulag—emerged from prison stronger, more united, more committed, and fully prepared to destroy the old world which had been the cause of their misery and degradation.

Throughout, the historical account is generously supplemented with moving character sketches and vignettes of extraordinary sensitivity through which Djilas succeeds in capturing the diverse character of the movement. There are cowards, men and women of great courage, schemers, individualist misfits, intellectual renegades (in campaigns against whom Djilas figured prominently), and those who selflessly gave their lives to the cause. Tito, whom Djilas spares intense examination, emerges through fragmentary accounts as "a strong man, but not a careful listener" (p. 259), a willful personality who would not tolerate lengthy debate even when he was misinformed or simply in the wrong, but nevertheless a great leader and something of a "father" even to his closest associates.

But who is Milovan Djilas? Into the personality of Djilas himself this autobiography offers disappointingly little insight. What is the character of a man who is capable of a tender and enduring love for a woman, but who also leaves the reader with a sense of disgust at his lack of feeling for a fellow Communist who ended his promiscuous but unhappy history of sexual liaisons by giving his life to the party? What is the character of a man who looks upon his rigidly dogmatic attacks on dissident prewar communist intellectuals with a certain satisfaction, while demanding, fifteen years later, his own right to speak out against the party? These important questions are left unanswered by Djilas's life of Djilas (although some insights are offered in Djilas's subsequent reflections in his *The Unperfect Society: Beyond the New Class* [New York: Harcourt, Brace and World, 1969]), and perhaps it cannot be otherwise. Yet it is doubtful, considering the legends, both reverent and malicious, that have grown up around Djilas's name, that his contemporaries will ever succeed in offering as compelling a portrait of Djilas as Djilas presents of them.

GERSON S. SHER

Reston, Virginia

Instability and Political Order: Politics and Crisis in Nigeria. By B. J. Dudley. (Ibadan: Ibadan University Press, 1973. Pp. xii, 265. Naira 6.50, cloth; Naira 2.50, paper [approx. $6.00 and $2.25, respectively].)

By the time I had got through the first fifty pages of this book I was sorry that I had agreed to review it. Reeling under the strain of understanding Nigerian politics since 1960 in such terms as "corruption coefficient" expressed as RR_1/RR_2 (p. 46) and "extent of alienation" as WW_2/W_1W_2 (p. 47), I had realized that this is not my kind of political science. And I was embarrassed (and still am) by the fear that this reaction might be interpreted by Professor Dudley and others as professional jealousy, in that a few months before this book had appeared I had published a study which attempted to cover much of the same ground. Obviously there is room for more than one assessment of the same data, but the question we must ask of any such venture is, does it really contribute to our understanding? In honesty all I can answer is that the way in which Professor Dudley has chosen to explain events very largely vitiates the contribution he might have made.

He sets out to explain why, only a little more than five years after independence, the political system of one of Africa's most important states had almost reached a point of collapse, a military coup took place, then another, and a civil war broke out which ended in the defeat of the attempted Biafran secession. The book falls into two very distinct parts, dividing at the first coup of January 1966. In the first part the author attempts to use game theory to explain how the maneuvering among Nigeria's three major parties and their satellites led to a dangerous situation of "bipolarization," which "would tend to rule out the chances of effecting change through the accepted regulative rules of the system and hence change, and by implication a halt to polarity, would be brought about only through the intervention of exogenic forces" (p. 80). Game theory, however, almost entirely disappears in the second part, though it is not clear to me why factions among army officers—the exogenic force—could not be analyzed in the same way as political parties. As it is, the second part of this book provides a much more readable account of the first and second coups, the events leading up to secession and civil war and the course of the war itself. There is much more narrative here, and some new data.

Why, then, should I have preferred the second part to the first and found it of rather more value? There are, I think, more general problems involved than a personal blindness to formulae of the sort quoted above, which I find impossible to relate to political events. First, behind the symbols in these formulae there are hidden concepts, which, in being reduced for operational purposes in this way, are all too often left unexplored and unexplained. Thus, Professor Dudley's general orientation seems to be toward the conceptual apparatus of Easton and Almond and Verba, but we are left not only with any doubts we may already have about those gentlemen, but also with the present author's failure adequately to answer such questions as, where do his "rules of the game" come from, what exactly are "resources" and how are they generated, and where does change (viewed here essentially as change of rules) come from?

The second general problem is the replacement of explanation in terms of social relations by pseudo-explanations in terms of combinations of symbols. The use of game theory forces explanation to be kept on the level of the often unedifying twists and turns of political parties, which can more often be expressed as parts of formulae than the exploitations and passions of human beings. Professor Dudley prefers to talk of elites quantitatively possessing "more than a proportionate share" of wealth, status, and power (p. 31), rather than of classes and their qualitatively different social relations. (In any case, on p. 32 he seems to fuse the concepts of class and elite.) The particular result of this dehumanization and elite bias is the almost complete disappearance of the mass of the Nigerian population: workers, peasants, petty traders and the rest, as voters, strikers, rioters and rebels, feature very little. It is significant of the author's kind of analysis that, once he has established a condition of "polarity" at the party level, the actual events of 1963–66–i.e., the crisis of the system–can be summarized in a page and a half (pp. 81–2); thus what men and women did becomes subordinated to theory. Similarly, game theory (and for that matter Almond and Verba) cannot deal with the complex phenomena loosely labeled "tribalism" in the literature, and without an understanding of tribalism as ideology and in relation to political action Nigerian events cannot be adequately explained. To reduce "tribalism" to "an attribute of elite behaviour" (p. 43) and to come to the conclusion that the Ibo were massacred in September 1966 just because they "happened to be the most vulnerable" (p. 168) is a gross oversimplification.

In conclusion, then, I find this book fatally flawed by its author's chosen method of analysis, and I remain sorry to have to say this about one of Nigeria's best political scientists.

KEN POST

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Anarchists and Communists in Brazil, 1900–1935. By John W. F. Dulles. (Austin: University of Texas Press, 1973. Pp. 603. $12.50.)

Like his earlier two books on Brazil, Professor Dulles's latest one is a welcome and useful contribution. Covering the activities of the Brazilian Left during the Old Republic and the early years of the Vargas regime, Dulles focuses on the formation, evolution, and activities of anarchists and Communists as they relate to an emerging labor movement and to the national scene. As a detailed history of leftist currents in Brazil during the early third of the present century, the book presents a descriptive narrative of events, a mosaic of detail and fact written in clear style and in an interesting manner.

Twelve sections compose this work. The first examines the background which shaped the evolution of anarchist and socialist movements in Brazil. Dulles looks at the Italian, Portuguese, and Spanish immigrants who came to work in Brazil; the workers' organizations which were established; and the early strikes which mobilized the proletariat under the leadership of anarchists and socialists. A second section focuses on the dominant anarchist activities just after the Russian Revolution. A third traces the decline of anarchism to 1921, while a fourth and fifth delve into the ideological struggles between anarchism and the incipient Communist movement.

The remaining sections of the book are concerned primarily with the activities of the Partido Comunista do Brasil (PCB), founded during late March 1922. Dulles focuses on internal squabbles and ideological conflict which undermined party cohesion during the 'twenties. He portrays such major Communist personalities as Astrogildo Pereira, the party's first secretary general; Evarardo Dias, a leading socialist; and Octávio Brandão, an anarchist. Government repression of these and other opposition leaders is detailed.

During the early PCB period, Brandão emerged as theoretician, and his ideas dominated the theses of the party's second congress during 1925. Thereafter, the party prepared to participate in elections by establishing a worker's front organization for its clandestine activities. But from January to August 1927 the PCB was permitted to function openly. Leôncio Basbaum took over the national leadership of the youth movement, while Astrogildo established contact with Luíz Carlos Prestes, the rebel army lieutenant and "cavalier of hope" who had marched a column of soldiers thousands of miles through the Brazilian backlands. This initial contact with Prestes led later to his conversion to the party and to his emergence as party leader.

From this upsurge of party activity, Dulles describes the decline through the years from 1929 to 1932. The decline was characterized by party factionalism, in part caused by ties with Prestes, in part caused by policy changes dictated by Moscow which affected relations with the worker's movement. A resurgence of party strength corresponded with Prestes' return to Brazil from Moscow and in the formation of a popular front, the Aliança Nacional Libertadora (ANL). The account ends with the abortive uprisings in Rio, Natal, and Recife during 1935.

This book is nicely illustrated with cartoons, drawings, and photographs of the period. There is a useful glossary and an excellent comprehensive index. An appendix on prices, wages, and strikes is included although there is no accompanying interpretation and analysis.

Overall the book is limited for the social scientist seeking in-depth analysis of the events. There is little effort to note patterns of party behavior and activity or to draw conclusions and findings from this broad sweep of Brazilian history. What the political scientist and historian will welcome, however, is an interesting account written in an appealing style which allows the reader to share in the experience of left-wing opposition politics in Brazil.

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Dependence and Opportunity: Political Change in Ahafo. By John Dunn and A. F. Robertson. (New York and London: Cambridge University Press, 1973. Pp. 400. $19.50.)

Since the writing of M. Owusu's *Uses and Abuses of Political Power: A Case Study of Continuity and Change in the Politics of Ghana* (Chicago: University of Chicago Press, 1970), a number of local political studies on various small and medium-sized African towns has appeared. The principal intellectual inspiration for many of the growing crop of grass-roots political studies of which Dunn and Robertson's book is a recent addition, has come from Apter, Austin, and Zolberg among others.

The principal objectives of these studies are to (1) gain intimate knowledge and understanding of political change in the non-Western World, and (2) to test the validity of the claims of the macro-studies and macro-theories of political scientists in order to advance comparative political theory.

Unfortunately, Dunn and Robertson's carefully researched work is not directly concerned with comparative theory (see chapter 1, introduction, pp. 8–9), but with the "*frenetic* pursuit of meanings" (italics mine, p. 8)!

This is a study by two Cambridge scholars, a political scientist and an anthropologist, of the historical impact of British colonial control and decolonization on local socioeconomic and political structures in the Ahafo region of Ghana. Ahafo is not presented as typical of Ghana or Africa but the authors note that many aspects of Ahafo politics, for example, its extreme localism, do resemble politics in other Ghanaian districts. An explicit comparison of Dunn and Robertson's observations with Owusu's conclusions based on an earlier, similar study of Swedru which the authors conveniently ignore, would show that though localism is indeed an important feature of Ghanaian political life, many equally, if not more significant, noncommunal considerations influence Ghanaian political behavior. In chapters 3 and 4 Dunn skillfully traces the development of Ahafo, a process which opened up a wide range of *opportunities* (primarily socioeconomic) for the Ahafo people even as it led to their *dependence* on the outside world.

Robertson writes chapters 2, 5, 6, 7, in which he attempts an interpretation of Ahafo politics (which both authors describe misleadingly as essentially localist-communal and factional), in terms of the *semantic range* and *symbolic manipulation* of the concept *krom* (i.e., "community" or "town") in Ahafo. For Robertson, Ahafo political rivalries are all expressions or strategies of "communal aggrandizement" (p. 209).

Robertson's (and Dunn's) self-confessed incompetence in Twi leads to serious factual distortions. It is incorrect to limit the *objective* usage of the word *kro* (kuro) to state capitals, or to large populous towns, and to interpret Ahafo Politics as expressions of a "continuum of settlement growth" (pp. 17–18).

There are numerous mistranslations of Twi sentences, phrases, and words throughout Robertson's chapters (see pp. 17, 191, 211, for glaring examples). Nevertheless, Robertson's discussion of the politics of chieftancy in Ahafo and his account of the emergence and growth of village/town development committees are informative, despite conclusions (see pp. 297–301; 244–253 particularly), which are contradicted by much of the internal evidence provided by the authors. Surely, individual self-interest, class, and other non-local: regional, national, and even Pan-African considerations, frequently unrelated to communal interests, played a tremendous part in the dynamics of Ahafo politics.

The weakest section of the book concerns the analysis of party politics in Ahafo in the '50s and '60s (chap. 8). Dunn's arguments here are often ambiguous and hardly persuasive On the one hand, like Robertson, he sees Ahafo politics as the ". . . continuation of communal war by other means" (p. 72; see also p. 334). On the other hand, Dunn suggests that party politics in Ahafo is more than Ahafo articulation of Ahafo communal interests. He indicates that "among a small proportion of the local population—perhaps 10 percent at the outside—who concerned themselves over *national* political disputes" (italics mine, p. 333), *class* or status interests (see p. 46) have very probably influenced their electoral choices.

Only a systematic, comparative analysis within a global context, of the complex interests at stake, can provide a meaningful synthesis of the intricate dynamics of Ahafo and African politics in general.

MAXWELL OWUSU

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Encyclopedia of Soviet Law (2 vols.) Edited by F. J. Feldbrugge. (Dobbs Ferry, N.Y.: Oceana Publications, 1973. Pp. 774. $95.00.)

This work is a compilation of four-hundred fifty short articles covering major aspects of the legal system of the Soviet Union. It is the product of five years' collaboration by forty-three Western specialists in Soviet law.

The *Encyclopedia* includes articles on the various branches of law (e.g., administrative law, criminal law), on legal concepts (e.g., illegitimacy, legal persons, parasites), and on nonlegal topics that affect the Soviet legal system (e.g., dictatorship of the proletariat). Entry headings have been taken from the *Index to Foreign Legal Periodicals* and also from Soviet dictionaries of Soviet law. Since both Western and Soviet topic headings have been used, the reader can begin either from a concept used in Western law (e.g., bail), or from a Soviet concept or practice (e.g., collective farm). Extensive cross-referencing facilitates access to related entries.

The forty-three authors include many leading Western experts in the field of Soviet law, each writing in her or his field of special interest. Their work was coordinated by F. J. Feldbrugge of the Documentation Office for East European Law at the University of Leiden, who selected entry topics and solicited contributions.

The *Encyclopedia* is aimed at students of Soviet society, who frequently encounter legal terms in their work, as well as at Western lawyers who want to find out how a given legal concept is handled in the U.S.S.R. It succeeds in meeting the needs of both groups.

The entries are, for the most part, factual and definitional, with little value judgment. Most entries aim at explaining a legal concept or institution, not at describing social factors underlying the concept. The entries do not purport to provide a comprehensive analysis of the subjects described. But for a reader unfamiliar with Soviet legal concepts, the entries generally do provide the basic information that one encountering a concept or term for the first time would desire.

In a work of this type, clarity and accuracy are of the essence. Entries in the *Encyclopedia* achieve a high degree of accuracy. Exposition of concepts is generally clear, though a few entries, particularly those written by persons whose native language is other than English, lack precision and clarity.

A fold-out chart included inside the back cover of the second volume groups the entries into the appropriate branches of law. The editors also include as an appendix a list of selected Soviet statutes covering the period 1917-1973, with references to their sources.

The *Encyclopedia* fulfills a need for readily accessible factual information about Soviet institutions of law and government. Western scholars writing about the U.S.S.R. typically focus on power relationships and frequently fail to provide the reader with sufficient basic data on how the Soviet system functions. For example, a scholar discussing Soviet paralegal institutions like the "comrades' courts" may give an opinion as to whether the comrades' courts fulfill real social needs or are, on the contrary, a tool for repression, but may not describe how they actually function. The *Encyclopedia*'s entry on "comrades' courts" provides a concise explanation of the origin and function of the comrades' courts, analyzing their competence, jurisdiction, and powers of disposition. This entry thus provides background explanation for a reader who has encountered a discussion of comrades' courts in another source and puts the reader in a better position to make an independent judgment about them.

At $95.00, the *Encyclopedia* will not be a bedside favorite. But it is an extremely useful and well-conceived reference work providing ready access to a wealth of information about Soviet legal institutions.

JOHN QUIGLEY

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The Australian Loan Council in Federal Fiscal Adjustments, 1890–1965. By R. S. Gilbert. (Canberra: Australian National University Press, 1973. Pp. xi, 337. $24.75.)

It is not academic verbiage to say that this is a much needed book. The Australian Loan Council not only has been a major part of Australian federalism for many years, but has also been a major case of cooperative federalism in action.

Here for the first time is a full study of the steps that led to the creation of the Council and of its operation.

The whole problem of state debts was much considered at the Australian Constitutional Convention, but it was not solved there. The framers of the constitution looked forward to an equal basis of state taxation, so that Commonwealth government aid could simply be made by per capita payments, but this never happened. Furthermore the framers took a simplistic view of financial matters, and failed to realize how important the Commonwealth would become in economic affairs. Likewise, they did not realize the extent of Commonwealth need for revenue, when they assumed that the states could expect extensive Commonwealth financial aid.

The first decade of federal history was, says Gilbert, "littered with proposals and counterproposals on the problem./In these the states wanted the Commonwealth to provide funds and the Commonwealth wanted a "coordinate" system of revenue so that it could finance its own politics. In this sense, coordinate means a system in which the Commonwealth and state governments would be independently responsible for their own revenues. Yet the states wanted a continuation of the guarantee of three-fourths of the customs revenue which had been provided for in the Constitution but which was to end in 1910. The Commonwealth partly got around this by putting its surplus revenues into trust funds so they would not have to be paid to the states.

All of the jockeying eventually led to the creation of a voluntary, that is, nonstatutory Loan Council in 1923. Under this council, which functioned with remarkable success, representatives of the Commonwealth and state governments met and agreed on loan programs by consensus or unanimous agreement. The Council achieved its two main goals—the reduction of public borrowing and the lowering of the bond rate, although the latter took two years to accomplish. More important—and Gilbert emphasizes this—it provided an experience which made it easier to secure the later adoption of a permanent statutory Loan Council.

Basically the new Council, adopted by an agreement which was written into the Constitution by referendum, provides for one vote for each state plus two votes and a casting vote for the Commonwealth, and with a majority vote to govern. It controls both Commonwealth and state borrowing programs, including the determination of interest rates. Gilbert emphasizes that it is a supraparliamentary body with the power to make binding decisions on the balance of payments, monetary policy, state fiscal policy, and development policy. He thus concludes that there are really three Constitutional policy making entities in Australia—the national and state governments and the Loan Council.

In the author's detailed analysis of the operation of the Loan Council, one conclusion is particularly worth noting. The Commonwealth, which has little power in many fields, has been able through its domination of the Loan Council to "achieve indirectly in the Loan Council what it is not directly empowered to do." Basically this book is not just an analysis of the Loan Council, but rather is a fascinating study of the division of both political and economic power between the national and state governments.

RUSSELL H. BARRETT

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Through Russian Eyes: President Kennedy's 1036 Days. By Anatolii A. Gromyko. (Washington, D.C.: International Library, Inc., 1973. Pp. xviii, 239. $9.95.)

"The historical mind," asserts Arthur M. Schlesinger, Jr., in *A Thousand Days, John F. Kennedy in the White House,* "is rarely ideological—and, when it becomes so, it is at the expense of history." Unfortunately, the official Soviet historical mind is highly ideological, and Gromyko's is no exception. Consequently his study suffers from a surfeit of ideology.

The author's credentials are on their face excellent: son of the present Soviet Foreign Minister, a doctorate in history, head of the Foreign Policy Section of the USA Institute in the USSR Academy of Sciences. If, however, the quality of Soviet scholarship regarding the USA is to be judged by Gromyko's book, it must be poor indeed. For his years in this country and his special research missions seem to have contributed little to his understanding of our politics or the Kennedy Administration. His story of the Kennedy presidency reads as if he had patched it together in the most sloppy, shallow manner from a number of other studies and superimposed upon it a heavy-handed dogmatism.

A lengthy chapter which concentrates upon Kennedy's campaign to win the presidency relies on Schlesinger's *A Thousand Days . . .* and upon Theodore H. White's *The Making of the President, 1960,* even, at times, to the very style White employed. A ponderous theoretical chapter, which attempts to demonstrate that our government and politics are to be understood solely in terms of the ruling monopolistic circles, is followed by the two principal chapters on the foreign policies of President Kennedy's administration. The book ignores totally his domestic policies. Two brief chapters indicate that in Gromyko's eyes Kennedy eventually became a "realist," favoring the abandonment of policies of "enmity and prejudice" toward the Soviet Union in favor of peaceful coexistence and cooperation. Having thereby engendered the hostility of the "ultras" (the extremely reactionary forces) together with the "overwhelming part of the American monopolist bourgeoisie and especially the military-industrial complex" (p. 223), John F. Kennedy was assassinated. Published in Moscow in 1972, the American edition includes a critical "epilogue" by Hans J. Morgenthau.

Gromyko's scholarship may be dismissed out of hand. "It is evident," "it is known," and "it is common knowledge" are all too frequently the author's sole authority for his conclusions. Whatever the Soviet Union does is peaceful, correct, realistic; the U.S. is invariably imperialistic, warmongerish, unrealistic. Since present Soviet policy ignores Nikita S. Khrushchev as an "unperson," the Soviet reader is not permitted the knowledge that he was Kennedy's counterpart in U.S.-Soviet relations; his name is never mentioned.

What can the Russian reader learn from Gromyko's book about the U.S. and President Kennedy? Essentially it is that the U.S. government is controlled by a monopolistic ruling class, elements of which compete ruthlessly among themselves. The monopolists are divided according to region—the northeast monopolists, based upon Wall Street and dominated by the Rockefellers, Morgans, Mellons, and Fords(!); the midwest monopolists, the second most powerful group; the Texas monopolists; and other regional groups—each exerting influence upon foreign policy.

According to the author, Kennedy's 1036 days can be realistically understood only in terms of the power of this reactionary class of monopolists. They have the final word in every critical decision. Presidents, senators, and bureaucrats are all at the service of these monopolists, the overwhelming majority of whom hate and fear the socialist countries. Gromyko's treatment of Kennedy's foreign policy, whether it related to Cuba, Berlin, Vienna or Vietnam, follows the same script. Kennedy was a reckless cold-war advocate, an adherent of the imperialistic and anti-socialist views of his master class. His vaunted flexibility in foreign policy reflected no essential change from that of Eisenhower or Dulles, only a rejection of outworn tactics. Although in a formal sense the author of American foreign policy, Kennedy played essentially the role that was assigned to him by the military-industrial complex. His saving grace was that he did not unleash an immediate nuclear war or completely terminate all negotiations with the Soviet Union.

Toward the end of 1962 the president began to speak, however, in a new and realistic voice and to become more independent of the Cold War monopolists, Gromyko tells us. As a result, the international climate improved and positive developments occurred between the Soviet Union and the U.S., especially in terms of a nuclear test-ban treaty. At this point the author has the leader of the Cold War, a figure in the governmental apparatus that is controlled by monopolists, responding positively and on his own authority to the friendly overtures of the Soviet Union! Gromyko escapes dealing with this contradiction by diverting the reader's attention to the president's assassination.

Why was Kennedy assassinated? Gromyko's answer points to a complex of forces and motives: jealousy and fear on the part of the monopolists who were rivals of Kennedy's Boston financial group; anger and discontent aroused by the ruthless competitive struggle which "engulfs everyone and everything in capitalistic America" (p. 6); the bitter attacks by the "ultras" and the press upon the president's new policies toward the Soviet Union; and the glorification of violence in American life. Indeed, for any U.S. leader to oppose a warlike foreign policy is to court death. Explaining the subsequent assassination of Senator Robert Kennedy, Gromyko admonishes his readers: "Thus, it is evident that in the United States, those who do not totally agree with the foreign policy course of the American 'hawks,' or who oppose rampant racism, have their brains blown out" (p. 7). Such is the picture of the Kennedy Administration and American society that is presented to Russian eyes.

ABRAHAM HOLTZMAN

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The People's Liberation Army: Communist China's Armed Forces. By Angus M. Fraser. (New York: Crane, Russak & Co., 1973. Pp. 59. $4.95, cloth; $2.45, paper.)

Angus Fraser's *The People's Liberation Army* is an extremely brief but useful account of the military characteristics and capabilities of China's armed forces; the factors such as deployment, training, costs, resources, production, and technology that condition these forces; the capabilities and limitations of the PLA; Peking's view of the threats to its security; and the implications of the foregoing for strategic developments in Asia.

Fraser takes as his baseline in measuring Peking's military capabilities the Chinese experience in Korea, ruling out subsequent combat experience in the Taiwan Strait and India as too limited in scope and duration. Yet a wealth of material exists in the *Kung-tso T'ung-Hsun (Bulletin of Activities)* of the General Political Department of the PLA, the so-called "secret military papers," covering the period of January 1–August 26, 1961, that casts important light on aspects of Chinese military operations and doctrine and thus affords another landmark in time for a consideration of developments in China's armed forces.

For PLA capabilities and developments in China's nuclear-missile program, the author relies primarily on *The Military Balance* series of the International Institute for Strategic Studies in London, statements by Department of Defense officials, and studies by defense analysts. On this basis, he concludes that the Chinese nuclear program appears to embody the development of a usable range of warheads, mainly for aircraft and missile delivery, and that there is a steady but prudent movement toward greater ranges. Writing in 1973, Fraser suggests that the PRC may have as few as ten or as many as a hundred MRBMs, as well as a small number of IRBMs and he projects some 10-25 ICBMs by 1975. *The Military Balance (1975–76),* published in 1975, gives China about 50 MRBMs and 20–30 IRBMs. According to former Secretary of Defense, James R. Schlesinger, in his February 5, 1975 report on the FY 1976 defense program and budget, deployment of a limited range ICBM has been considerably slower than expected while a number of the 7,000 nm ICBMs may possibly become ready for deployment by mid-1980.

Despite Fraser's inflated estimate of China's ICBM capability, a key conclusion on his part is the defensive nature of China's military forces, with a number of factors arguing against sizable and extended PRC operations beyond its borders.

Another key conclusion is China's preoccupation with the massive Soviet military presence along its border. The author notes, in contrast to Peking's traditional doctrine of defense in depth, the increase of military capabilities in the area, including missile capabilities, and raises the interesting question of whether Chinese behavior is due entirely to a changed estimate of the threat or whether its shifting posture represents a new level of ability to field more and better weapons. He also suggests that Peking's emphasis on a regional missile capability would reinforce the belief that defensive deterrence against the Soviet Union has high priority.

In his conclusion on strategic developments in Asia, Fraser notes that Peking is best prepared to fight the type of war that focuses on defense against the Soviet Army—a type of war that is least likely to be engaged in by the United States. China, he points out, has increasingly raised the cost of an attack on its territory; while any major attack on China would involve tremendous damage to China, the attacker would also have to think seriously about the costs and level of damage that he or his allies would suffer in the process. Since this seems a slightly exaggerated view of China's capabilities, I can only wish that the author had been able to give more time to the intriguing problem of the extent to which China is developing a posture of deterrence, and against whom.

ALICE LANGLEY HSIEH

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The Politics of Labor Legislation in Japan: National-International Interaction. By Ehud Harari. (Berkeley: University of California Press, 1973. Pp. xii, 221. $10.00.)

This study, originally prepared as a doctoral dissertation, is a thorough analysis of a basic labor-relations policy issue which emerged front and center on the stage of Japanese politics during the late 1950s and much of the 1960s. Its complexities serve to illustrate the widely differing interests and positions among the "progressive" and "conservative" forces which make up the Left-Right political scene in present-day Japan. The book provides a much-needed detailed case study, with historical depth, of the dynamic interplay of parties and actions within and outside the Japanese Diet. As such, it offers a refreshing perspective in contrast to the usual static depictions of Japan's political structure and processes. The nature of the issue allows Professor Harari to portray the prominent role of international influences in Japanese politics.

In summary, as the author ably details, the issue was whether, and with what accompanying conditions, the government of Japan would ratify the 1948 ILO Convention No. 87, which provides for Freedom of Association and Protection of the Right to Organize. Despite the far-reaching labor reforms in Japan under the Allied Occupation, and despite Japan's subsequent adoption of most ILO conventions (including No. 98 of 1949 on the Right to Organize and Collective Bargaining), the Japanese Government failed to ratify Convention No. 87 although indicating its intention to do so. The snag arose from amendments to the very liberal labor relations laws, which were adopted from 1948 to 1951 following the threatened (but aborted) general strike of February 1, 1947. The amendments placed restrictions and, in some cases, prohibitions, upon public employee unions regarding their rights to organize unions of their own choosing, engage in collective bargaining, and conduct disputes or strikes. Ratification of Convention No. 87 would have required liberalizing, if not eliminating, a number of these restrictions and prohibitions.

For various reasons, some having to do with ideological and organizational problems of Japanese unionism itself, the ratification question at first simmered in the background. It boiled over, however, in 1957, when the Japanese labor representative first formally complained to the ILO that Japan was in violation of the Convention. The complaint followed a prolonged confrontation between the national railway unions and the government, in which under the existing laws for employees of publicly owned enterprise, strikers were penalized with warnings, pay reductions, and dismissals, including discharge of top union officials. In the eyes of organized labor, the last particularly interfered with internal union affairs, or the right of association, especially when the government refused to engage in collective bargaining with the railway unions as long as they retained as leaders individuals terminated from public employment. Domestic efforts to conciliate the dispute brought no final resolution. By this time, organized labor in Japan had become increasingly prepared to confront the government on the issue; meanwhile the ILO itself, under pressure from international labor bodies such as the ICFTU and the International Transport Workers Federation, became receptive to act upon the appeal.

What followed was hardly simple. For the next ten years, the dispute over ratification and amending the relevant laws "became the longest continuous issue in the Japanese Diet; it involved a variety of individuals, groups and institutions in several styles of conflict" (p. 4)—at both the domestic and international levels of politics. A climax to these events—a series of escalations, confrontations, stalemates, near bargains, and hoped for compromises—came in an on-the-spot investigation in 1965 by an ILO Fact Finding and Conciliation Commission (known as the Dreyer Commission), the first of its kind in ILO history. This set the stage for Diet ratification and some initial amendments, although a number of the aspects of the dispute, such as the right to strike of public employees, remain unresolved even today. (A series of officially appointed tripartite advisory councils has attempted to achieve compromises for Diet adoption without notable success.)

A major contribution of the study is the author's review in chapters 2–4 of the history of labor relations policy and the labor movement in Japan, including Japan's entry into the ILO when it was founded in 1919 and subsequent participation until withdrawal in 1938, and the development and revision of the labor reforms under the Occupation and in the period immediately following the San Francisco Peace Treaty. The difficulties over Convention No. 87, which are taken up in detail in chapters 5–7, may be traced, Harari finds, to the long process of bringing Japan to accept in practice, rather than mere form, international norms of labor participation in pluralistic industrialized societies. To do so required the "linkage" of international and domestic politics. This the author demonstrates very amply in terms of the political maneuvering both abroad and within Japan itself.

The study is highly instructive for those who specialize in the study of Japanese politics and for those who are interested in the formation of government labor policy, especially in the thorny area of labor relations involving public employees. The author's avoidance of generalizations forces the reader to draw his or her own conclusions about how representative the case is of the Japanese political process, what parallels it affords with parliamentary policy making in other democracies, and what it signifies for international-national political linkages. With the wealth of data it provides, however, the study points in those directions as logical steps for further research.

SOLOMON B. LEVINE

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Social Change and Politics in Turkey; A Structural-Historical Analysis. By Kemal H. Karpat and Contributors. (Leiden: E.J. Brill, 1973. Pp. 373. Guilders 88. Approximately $24.00.)

The editor and major contributor, Professor Kemal H. Karpat, introduces *Social Changes and Politics in Turkey* by noting that the authors are concerned "with the correlation between economic and social development and politics, and the role of social groups in this process." (p. 1). He views structural change, defined as shifts in occupation, power, roles and status among social groups caused by technological change and industrialization as the basic cause of political change and modernization. He further maintains that modernization "occurs in successive stages which are qualitatively and quantitatively different from each other," each being "marked by the leadership of a specific social group" (p. 1).

Testing the preceding proposition within the Ottoman-Turkish context is treated at three different levels: Historical-structural foundations of Turkish politics, social groups and their changing roles, and ideologies of social groups.

Karpat, in the beginning chapter, first elaborates this approach by both criticizing and drawing inspiration from such other students of sociopolitical change as Barrington Moore, Daniel Lerner, and Seymour Martin Lipset. While his basic approach is sound, it lacks precision. Development, change and modernization are loosely employed as synonyms, to cite one example. While the reader is initially led to believe that political change results from structural change preceded by a change in technology, he or she is told later, "We believe that the basic difference between developed countries and the third world stemmed from the early organization of the West in a system of modern statehood and its *subsequent* adoption of technology and industrialization." (p. 18). Similarly, while stages of development are referred to as universally applicable, those of the Ottoman-Turkish case are examined without explaining if and how they could be utilized to construct a more comprehensive "stages of development model."

Despite these reservations, Professor Karpat presents an excellent and concise analysis of structural change and its political consequences in Turkey. His efforts to substantiate argument with quantitative data whenever they are available makes the chapter a valuable reference for students of Turkish politics. He observes that the efforts of the founders of the Republic to industrialize after consolidating their power gave rise to new social groups which undermined the near monopoly of power enjoyed by a bureaucratic-intellectual elite. Recent Turkish politics may be understood as a struggle for power between the old and this new industrial-commercially based political elite.

Dankwart Rustow in his chapter on the modernization of Turkey, analyzes Turkish political change in the more familiar stimulus-response framework where the stimulus often has external origins like military defeat by European powers. This is a good summary of previous analyses offered by many Western scholars among whom Rustow himself occupies a prominent place.

The part on "Social Groups and Changes in Roles" is the weakest link in the book. Nezih Neyzi traces the development of middle classes in Turkey by defining class in terms of income—through a period of rapid inflation—and concludes that 20 per cent of the population belongs to the middle class. Lacking an occupational dimension, his analysis does not illuminate Turkish politics. The chapter on "Labor in Turkey" by Bülent Ecevit, tends to equate labor with the National Confederation of Labor, Türk-Is, through most of his analysis. Although limited in scope, his views are interesting if for no other reason than his currently being the leader of one of the two major parties in Turkey. John Kolars has adopted a spatial center-periphery analysis with the center extending to the periphery, i.e., villagers, through administrative, political, economic and social penetration. Probably innovative when it was written (1965), it has lost some currency by the date of publication (1973). In the final chapter of this part, Ilhan Basgöz examines the free boarding schools of the Republic as an instrument of social mobility. His interesting study is too narrow in scope to have been included in a study of structural change.

The final part deals with ideologies of social groups. Summarizing the post-1960 developments comprehensively, Karpat concludes that Turkish politics has evolved into interest groups politics. Even those groups rejecting pluralistic politics act as interest groups when their benefits are involved, he claims.

Emphasizing two case studies in the provinces of Aydin and Adana, Frank Tachau examines professional and educational attributes of provincial party leaders to reach some tentative generalizations in this previously little explored area.

The book comprises papers presented at a conference in 1965, nine years before the publication of the book. Karpat's efforts to update papers is a modest effort of recording recent events. The lapse of almost a decade is unfortunate in two respects. First, these were turbulent years in Turkish politics, deserving closer examination. For example, by now, it has become increasingly difficult to talk about an easily discernible bureaucratic-intellectual elite as an autonomous political group. Second, this has been a period of extremely productive social research in Turkey from which the authors could have benefited. Of particular relevance would be Serif Mardin's and Ahmet Yücekök's works on religion, State Planning Organization's study of modernization trends in villages, Tuncer Bulutay's study of income distribution, Metin Heper's research on bureaucracy, Dogan Avcioglu's interpretation of Ottoman-Turkish history, as well as many others.

The book, nevertheless, provides many insights and clues to a better understanding of

Turkish politics and its historical roots. It may be of interest to students of political change as well as to students of Turkish politics.

ILTER TURAN

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The Soviet Cage: Anti-Semitism in Russia. By William Korey. (New York: The Viking Press, 1973. Pp. 369. $12.50.)

The theme is important and of evident topical interest. It raises many questions. What are the historical roots of anti-Semitism in Eastern Europe in general, Russia in particular? What effects did the revolution have, in freeing the Jews from discrimination and in stimulating the anti-Semitic feelings that arose from the substantial role played by Jews in the first years of Bolshevik rule? When did things start to change, and why? Is there a specifically *Soviet* anti-Semitism, or are we witnessing symptoms of the old disease?

William Korey has made an honest effort to understand. He avoids exaggeration, he reasonably explains unpleasant events, and what he has written is interesting enough as far as it goes. It does enlighten the reader about the recent past and the present situation. One is conscious, however, of lack of depth, a lack of historical sense, insufficient knowledge of Russia, perhaps. The book seldom rises above the level of good journalism. His title sets up two conflicting expectations: the reference to a "cage" suggests a propaganda pamphlet, which this book emphatically is not; the second half of the title implies a study of Russian anti-Semitism, which this book tackles only at a superficial level: the chapter on 'The origins and persistence of Soviet Anti-Semitism" devotes less than three pages to the period 1917–1939. The chapter does contain some acute observations, but they are not followed through.

Much more would have had to be said about the role of Jews in the Revolution and its undoubted effect on the attitudes of ordinary citizens. Even such patently liberal and friendly Russian dissidents as Maksimov and Sinyavsky have commented upon the notable role of Jews in the first twenty years of the Soviet regime, and Solzhenitsyn's *Gulag Archipelago* abounds in the names of Jewish policemen. Stalin and his gang eliminated most of them in due course, benefiting from, as well as stimulating, latent anti-Semitic (and anti-intellectual) attitudes among medium-grade party functionaries. Korey is right in pointing out that this antedated the foundation of the state of Israel.

There is indeed ample evidence for the survival of primitive anti-Semitism among the people, and this finds reflection in the attitudes of officials, for they too are people. Official policy has suffered from a fatal contradiction: on the one hand, officials identify Jews as a nationality and make (secret) discriminatory rules concerning persons of this nationality; on the other, they believe in assimilation as the means of causing the problem to go away. Korey quotes some very revealing statements of both positions. As one official put it, "the feeling of belonging to a distinctive Jewish entity . . . is just what we are trying to prevent" (p. 45). This helps to explain the stifling restrictions on all organized Jewish activities. Yet in the context of Russian (and non-Russian) nationalism, these restrictions, and also job discrimination, reinforce Jewish national feelings.

As Korey implies, a considerable number of party leaders regard Jews as a talented but inherently alien element, incorrigible individualists with a tendency toward having relatives abroad. Other causes include pressure from nationalities for privileges in their own republics, which takes the form of pro-Ukrainian discrimination in Kiev, pro-Uzbek discrimination in Tashkent, which handicaps local Jewish residents. There is also prejudice arising out of the overwhelming "white-collar" composition of the Jewish population, which has caused jealousy and resentment; even today an astonishingly high proportion of Jewish young people succeed, in the face of daunting obstacles, in entering higher educational institutions, though often not those of their choice.

The net effect of all these circumstances, in the absence of Stalinist mass terror, has been the emergence of a mass demand for emigration. Its extent must have astonished and perplexed the authorities. According to Korey, the Soviet reaction has been mixed. It has included trials, harassment and petty (and not so petty) persecutions, which are amply documented in Korey's book. It includes also the granting of exit visas in numbers which, a few short years ago, would have seemed unthinkable.

The extensive emigration has not been given publicity in the Soviet press, but it is plainly very well known. Its existence must reinforce popular mistrust and hostility among Russians. There is no doubt at all that job discrimination has become worse, as officials become more doubtful of appointing applicants of Jewish nationality in case they apply to leave. A disagreeable situation thus faces the two million or so Jews who are likely to remain in Russia. What is their future to be? Have they in fact a future in Russia?

One wishes that the author tackled this

human problem adequately. Nevertheless, the data on recent Soviet policies, on trials, on the attitudes of would-be emigrants and of the authorities, are well presented and well documented.

ALEC NOVE

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Before the Revolution: The Vietnamese Peasants Under the French. By Ngo Vinh Long. (Cambridge, Mass.: The MIT Press, 1973. Pp. 292. $12.50.)

This book is an indictment of French colonialism in Vietnam, a compilation and translation into English of valuable commentary by Vietnamese on their own condition, and a deeply felt personal statement by a Vietnamese scholar harboring an undisguised resentment of what foreigners have done to his country.

Some persons with an interest in Vietnamese affairs may still hold a view of the French colonial period as a time when the majority of Vietnamese accepted, or at very least acquiesced in, the French occupation. As scholarship, both Vietnamese and non-Vietnamese, has increasingly revealed, such a view is clearly wrong. Sections of the Vietnamese population found their interests linked with the French and in some cases gave vocal and prominent testimony to this fact. Even Vietnamese revolutionaries, including Ho Chi Minh, recognized that certain aspects of French thought and culture—as these existed in Metropolitan France—were appealing in themselves. But the record overall is unambiguous. Whether able to translate their resentment into active opposition or not, most Vietnamese under French colonial domination resented their position.

That a contrary view should once have been held was the result of many factors. Among the most important has been the presence of vast amounts of self-congratulatory French literature describing the colonial experience and the inability of other than a limited number of scholars to consult the Vietnamese language material that tells the other side of the story.

It is in this respect that Ngo Vinh Long's book plays such a valuable part. Mr. Long, a graduate student at Harvard when he wrote this book, has selected and translated Vietnamese materials that give a new insight into the life of the peasantry under French colonial rule. Few of the sources used will be well known, even to those who can read Vietnamese, yet all contribute to our understanding of the nature of colonial life. The translations are preceded by Mr. Long's detailed commentaries on aspects of the peasant experience under the French, again from the Vietnamese perspective. Under the general heading of "Analysis," he provides a review of such issues as "Land Expropriation and Land Concession," "Taxes and Tax Collection," "Usury and Agrarian Credit," and "Hunger and Starvation."

I am struck by the following emphases in the book: First, the commentary in the "Analysis" shows the extent to which French colonial writing on the Vietnamese peasantry is, in the perspective of history, remarkably self-condemnatory. To take one example, the human geographer Pierre Gourou, a scholar of great achievement, is nonetheless revealed as a man locked into a colonial frame of reference. As such he reports on the condition of the Vietnamese peasantry in terms that accept that the lot of the rural population is grossly unsatisfactory. Yet his colonial viewpoint allows him to contemplate this phenomenon as essentially immutable. Just because it contains the essence of colonial thinking—the acceptance that one set of standards applied to the colonial and another to the colonized—most French writing on Vietnam can be usefully analyzed to reveal what it was that caused such intense resentment on the part of the Vietnamese. That such insights should be quite contrary to the French authors' intentions does not matter.

The next striking point is the extent to which Mr. Long's translations of Vietnamese materials from the 1920s and 1930s dwell upon the role of the Vietnamese who exploited their countrymen under the protection, if not always the authority, of the colonial power. Here, for Vietnam at least, one perceives colonialism in one of its worst aspects. That Frenchmen should have behaved as they did was one thing: they were, after all, the colonials, the men who so often believed that they had the inherent right to dominate others. The wrongness of their beliefs created what in some ways seems a greater tragedy: a class of Vietnamese tax collectors, corrupt officials, and unscrupulous landlords, who took gross advantage of their own countrymen.

Finally, and far from exhaustively, I was struck by the intensely revealing nature of the Vietnamese literature that Mr. Long has translated. Whether one is reading of the inequities of tax collection in Nguyen Cong Hoan's *Dead End (Buoc Duong Cung)*, or the horrors of famine in *Who Committed this Crime? (Ai Gay Nen Toi?)*, by Tran Van Mai, there is a wealth of detail, feeling, and sociopolitical commentary that shows the vital historical significance of these largely untapped sources. What an extraordinary contrast this material provides to the pseudo-philosophic writings of Pham Quynh, a supporter of the French who was active at the same time.

One reservation might be made about this very useful book. Mr. Long risks giving the impression that all French policies were *planned* to be oppressive. An alternative point of view is that the colonialists' perception of their own role was much more complex and that some of their faults were those of omission rather than commission. Such a view does not excuse the fundamental errors of colonialism. The suggestion is instead that Frenchmen sometimes behaved evilly but at least as often, and even more disturbingly, they thought they were behaving in an admirable fashion.

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A History of the Czechoslovak Republic, 1918–1948. Edited by Victor S. Mamatey and Radomír Luža. (Princeton, N.J.: Princeton University Press, 1973. Pp. 534. $22.50, cloth; $9.75, paper.)

This succinct and informative volume represents a multilevel search for an explanation of the repeated failures of the Czechs and Slovaks to establish a socially progressive political system "with a human face," a goal which the majority of their elites have constantly strived for and occasionally succeeded in maintaining for limited periods of time. Despite the great variety in the fourteen authors' approaches, writing skills, and scholarly or political experience (including personal participation in the events described), they all seem to agree on one basic proposition: geopolitically speaking, a small polyethnic society, fixed by fate (or the devil?) at the sensitive crossroads of great powers' conflicting interests—a hell of a place to be—finds its destinies shaped by international politics more often than by its volition. "An internationally penetrated system" à la James N. Rosenau *par excellence*. Or, perhaps more colloquially: "Too far from God and too close to Berlin and Moscow."

Implicitly or explicitly the concept of a penetrated system underlies the three parts into which the volume is divided. Part One, consisting of seven essays, deals with the birth and evolution of a multinational, though Czech-dominated republic from its external baptism at Washington and Paris to its external funeral at Munich. Part Two analyzes the years of World War II, which splintered Czechoslovakia politically, ideologically, and territorially.

The concluding portion of the book focuses on Czechoslovakia's three postwar years (1945–1948), characterized by the wishful thought on the part of the liberal-democratic leaders that democracy and communism, and the West and the East, might converge and peacefully cross-fertilize each other on the Czechoslovak soil. Cold War or not, there probably has never been such a possibility, given the security interests and the triumphant might of the suspicious Soviet Union in the heart of Europe, the weakened positions of France and England, and the all-too-distant power of the United States. In contrast to Taiwan, Korea, and Vietnam, and despite the genuine sympathies for, and rhetoric about, Czechoslovak democracy, Washington kept on viewing Czechoslovakia as off bounds not only in 1938 (Munich), 1948 (the February coup), and in 1968 (the Brezhnev invasion)—the years ending in "8" seem to be particularly ominous for Prague—but also during the war, in 1944 and 1945, when the American armies and flying fortresses were on the move. The U.S. political and military distance is somewhat emotionally described by Anna Josko in chapter 13 on the Slovak National Uprising (1944)—an uprising that was crushed by the Nazis and abandoned by not only the Soviet but also the American High Command. General Eisenhower's agreement with Moscow and General Bradley's subsequent order to Patton *not* to liberate Prague (1945) but instead to leave it to the Red Army complete the picture of the Czechoslovak captivity in the Soviet zone of decisive influence.

The last segment of the book is Victor S. Mamatey's summary, a mere 13-page but masterful capsule of Czechoslovakia's modern history. The summary is followed by Radomír Luža's comprehensive 40-page annotated bibliography (including communist sources) which by itself makes the book worth examining by scholars interested in comparative politics in the Danubian area.

Since in this book one author follows another in a chronological sequence, the symposium lacks in clashes of conflicting evaluations of the same phenomena which are often a saving grace of many a symposium in political science. When, however, a few chapters at least partly overlap, some refreshing confrontations do occur. In Chapter 12 on the Czech resistance during World War II, for example, Radomír Luža glorifies the extent of the Czech political challenge which, during the war, forced "Adolf Hitler himself to take note of the continuous threat to the security of the Reich" (p. 361). According to Luža the Czech partisan warfare in 1945 was "a real threat to the German communications system" (p. 338). On the other hand, Professor Gotthold Rhode of the German University at Mainz notes that no external impulse, however electrifying (such as the creation of a new Czechoslovak government under President Beneš at Soviet-liberated

Košice in spring of 1945) could move the cautious Czechs to create serious disturbances in "the only territory at the command of the Reich in which [in 1945] economy and communications functioned normally" (p. 317). This sober estimate is later qualified by a strange thesis (based on demographic data that, unfortunately, are not graced by comparison with other Nazi-occupied areas) according to which an astoundingly diligent production of children constituted a special form of Czech resistance: "The Czechs met the foreign subjugation and the threatened loss of national identity by increasing the number of nationally conscious compatriots" (p. 319). Even Ernest Renan would probably consider such a claim made on behalf of patriotic babies as perhaps possessing a tongue-in-cheek quality.

Given the circumstances of the origin and development of the book, especially their authors' lack of direct access to the archives and original documents now in Prague and Bratislava and their understandable emotional involvement (the majority of the authors are Czech and Slovak exiled scholars), *A History of the Czechoslovak Republic 1918–1948* represents a remarkably well-balanced treatment of a very complex and controversial subject. While the review of the book has been appropriately placed in the "Comparative Government and Politics" section, it also belongs to the field of "International Politics," for the turbulent three decades of Czechoslovakia mirrored the Czech and Slovak political cultures and national goals less than they did the external shifts of power and the transformation of the Versailles international system into the Nazi New Order first and then into the Stalin-Brezhnev "socialist commonwealth."

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Fjerndemokratiet. Sosial ulikhet, politiske ressurser og politisk medvirkning i Norge. By Willy Martinussen. (Oslo: Gyldendal Norsk Forlag, 1973. Pp. 299. No price indicated.)

Willy Martinussen, a member of the Institute for Social Research in Oslo and a long-time collaborator of the Norwegian Program of Electoral Research, presents a very important study of political participation in Norway. All aspects of participation are analyzed: types of participation, usage, causes, and consequences. Data come from the 1969 national election survey conducted by Norsk Gallup Institutt A/S. The investigation is divided into three sections: (1) Resources for Political Activity; (2) Political Alienation; and (3) Political Poverty. (Note: *Fjerndemokratiet* refers to the relationships of the citizen to the central government in Oslo; the term is contrasted with *naerdemokrati* or grass roots politics.)

In Section I, Martinussen outlines available channels for citizen participation in Norway; he establishes the percentage of the population that obtains politically relevant information, discusses politics, participates in parties and interest organizations, votes, holds office, and engages in direct political action. He finds that the majority of the citizenry do not employ political means for the realization of individual goals; they are substantially passive, politically weak, and removed from the national political process. Martinussen then investigates causes for the pattern of participation he outlines. Determinants of participation include both barriers (ecological and sociostructural) and resources (such as wealth, psychological well-being, comprehension of the political process, personal competence, and desire to participate). Coefficients are found for all relationships using Multiple Classification Analysis. Having established participation patterns (and causes for differential participation) for the population as a whole, Martinussen proceeds in Section II to determine which are the active subgroups of the society and the basis for different subgroup behavior. He discusses political alienation in Norway and its causes, as well as assessing political apathy and sense of political ineffectiveness. In Section III, Martinussen enlarges on the findings of Sections I and II, focusing on the consequences of politically impoverished groups for a society and differential influence of societal subgroups.

Fjerndemokratiet will naturally be compared with Sidney Verba and Norman H. Nie's *Participation in America*. Martinussen's work is not as broad in scope or objectives as that of Verba and Nie. It does make important parallels, however, and these lend support to the conclusions of both works. For comparative purposes, the juxtaposition of the patterns of participation found by Martinussen and Verba and Nie are extremely interesting; they also demonstrate some significant differences between the two studies. One such area involves citizen participation and membership in organizations. From the two studies, Norwegians are shown to participate more in interest groups than Americans do; Norwegian organizations (particularly unions) incorporate much more of the lower classes. Accordingly, interest groups become increasingly significant as mobilizers of the society. Martinussen's analysis designates membership in organizations as the most important resource leading to citizen participation in national politics (p. 89). He finds that 46 per cent of the sample consider interest groups the most important societal agency for the protection of the individual's interests (elected representatives receive 27 per cent; parties get 11 per cent). Martinussen, unlike Verba and Nie, establishes that membership in different types of organizations have differential consequences for political participation.

Martinussen's analysis focuses on short-term forces (psychological and sociological) to a more substantial degree than Verba and Nie. Accordingly, Martinussen's analysis of long-term forces such as class, juxtaposed with short-term variables, is viewed in a broader and more interesting perspective. There are many such contrasts to be found in these two exceedingly important studies, and our understanding of citizen participation in the political process will be much richer as comparisons are made and the significance of the differences determined. *Fjerndemokratiet* is being translated into English at this time.

ROBERT B. KVAVIK

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The Liberal Imperialists: The Ideas and Politics of a Post-Gladstonian Elite. By H. C. G. Matthew. (New York: Oxford University Press, 1973. Pp. xvi, 331. $17.75.)

Numerous studies of *fin-de-siècle* Liberalism have recently poured off the presses, and the stream shows no sign of slackening. However, Dr. Matthew (lecturer in Gladstone Studies at Oxford University) offers a refreshing contribution to the current debate, since he has adroitly tackled a complex, important, yet hitherto strangely neglected aspect of the subject.

The outlines of this haunting episode in British party history are well known. Between 1886 and 1905 there emerged an able and articulate group of young Liberal politicians, who sought to haul a party demoralized by constant electoral rebuffs out of the Gladstonian ruts into which it had apparently sunk. The principal members were Asquith, Haldane, and Grey, patiently awaiting instructions from Rosebery—a perplexing leader who himself awaited the imperious call of national duty. Despite his undoubted talents, the latter was temperamentally incapable of acting within the confines of the party system. After 1896 he was doomed never to return to active politics, because his occasional and exciting initiatives were invariably followed by prolonged bouts of melancholy and inertia. Few were surprised, therefore, when Rosebery's faithful but intensely ambitious associates swallowed their objections to the somewhat pedestrian Campbell-Bannerman in 1905, and took their rightful places in a powerful and reunited Liberal ministry.

Matthew treats these weighty matters in a thoroughly scholarly manner. He has consulted an array of documentary sources, including the almost unknown papers of a rebellious Scottish whip, R. Munro-Ferguson. Moreover, he maintains an exemplary standard of accuracy throughout.

Yet the finished product is unsatisfactory in several particulars. Firstly, the book's chronological limits (as the subtitle suggests) are too severely restricted to the period between Gladstone's death in 1898 and the formation of the 1905 ministry. The Gladstone-Rosebery governments of 1892–5 are not analyzed, even though many Roseberians were afforded a unique opportunity for experimentation. Secondly, Matthew frequently constructs a distinctive Liberal Imperialist theory of events and philosophy of action without reference to similar views expressed by rival Liberals. For example, on the burning question of achieving temperance reform through local veto a Roseberian pragmatist might well have written (*vide* p. 239) that "a reform does not become an advantage until it is carried. . . . I am weary of doing nothing in order to attain perfection." In fact, this endorsement of the "clean state" came from Campbell-Bannerman in 1899. Finally, a rigid and artificial structural division between political action and ideas inhibits the systematic development of fruitful themes, particularly the failure to apply scientific principles to education in 1902.

Nevertheless, Matthew has written a provocative and absorbing volume. He traces the evolution of a "step-by-step" Irish policy (which Haldane conceived in 1896), and offers a convincing explanation for Rosebery's eventual ostracism. For whereas Rosebery increasingly promulgated Unionist axioms, the "Asquith committee" steadfastly declined to abandon Home Rule altogether. A desire to conceal this fundamental difference made the commoners "appear more extreme on Ireland than they really were" (p. 278). Thus, the débacle which followed Rosebery's maladroit Bodmin speech of November 1905 was unavoidable, and incidentally validates two recurring themes in Matthew's book. Firstly, Asquith's ingrained sense of party loyalty constantly checked the impetuosity of schismatic colleagues, keeping Grey and Haldane "in communion" with the orthodox Liberal church. Secondly, the Roseberian "babies in intrigue" were so averse to sectional organizations that—notwithstanding the notorious Liberal League—they lacked the institutional leverage to pry Campbell-Bannerman out of the leadership.

The outcome might have been different if the Liberal Imperialists had not engaged in fratricidal strife during the Boer War. Matthew exposes in trenchant language their ignorance of the true South African situation. A credulous band of Milner-worshippers nearly seceded from the party because they "placed their faith in a man whom they did not really know and, through their pragmatism, in a policy which they did not understand" (p. 187). Moreover, the apostles of "efficiency" could not offer compensation in other fields, since they had failed to work out detailed proposals on any aspect of domestic policy—with the single exception of trade disputes. The promise of the 1880s remained unfulfilled, a conclusion which raises the question of whether faddist bores and pro-Boers utilized their lesser talents to better effect.

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Soldiers, Guerrillas and Politics in Colombia. By Richard Maullin. (Lexington, Mass.: D.C. Heath, 1973. Pp. 192. $10.00.)

Considering the high incidence and changing character of domestic political violence during its recent political history, Colombia represents an ideal case for examining the impact of violence upon military behavior. Richard Maullin draws upon this rich historical experience to analyze the role of military professionalism as it relates to internal political violence in Colombia. His basic hypothesis is that "under conditions of internal war, the tension between military professionalism and partisan political involvement tends to break down; the discipline and skills inherent in professionalism promote the involvement of the armed forces in political affairs" (p. 4).

Turning first to the nature of internal conflict, Dr. Maullin (now associated with the California Education Department) presents in chapter 2 an overview of the *violencia* of the 1940s and 1950s, some salient features of Colombian politics, and the big-power conflict as related to Colombian guerrillas. A description of the three principal guerrilla concentrations of the 1960s constitutes the core of chapter 3, with emphasis upon their international ties with the Soviet Union, Cuba, and China. Next, in chapter 4, the author reviews the principal rationales followed by the military in subduing the guerrillas. The operational version of the armed forces' counterinsurgency doctrine, labeled "Plan Lazo," was launched in 1962 to erradicate the "independent republics" controlled by bandits and guerrillas. "Plan Lazo" was predicated upon the belief that mere physical repression of guerrillas would not eliminate violence unless the underlying political, economic and social causes also were curtailed (p. 76). The author questions this assumption, though not very convincingly.

In chapter 5, which focuses upon the military as an institution, the author presents three principal conclusions. First, increases in the size of the Colombian military during periods of acute domestic violence were not matched by corresponding budgetary increases in relation to total governmental expenditures. Second, an analysis of aggregate data suggests that ". . . the size of Colombia's military establishment and its costs are unexceptional when compared with other Latin American states" (p. 90). Third, the congruence of goals between United States planners and the Colombian military—internal security—produced a situation in which "The United States can say that it helped Colombians avoid having to sacrifice important development investments in order to repress guerrillas" (p. 104).

This book suffers from four problems: (1) theoretical shallowness, (2) conceptual ambiguity, (3) weak data and erroneous conclusions sometimes drawn, and (4) frequent misunderstandings of the Colombian political system. First, the study lacks a theoretical framework and fails to place the principal hypothesis within the context of the literature. The author seems unaware that Lasswell, Huntington, and Lieuwen, among others, have treated the relationship between prolonged conflict and military power. Second, and more serious, is the analytical confusion stemming from the basic concepts employed. Maullin notes that the military's participation has not only been political, in the broadest sense of the term, but also partisan (p. 112). Yet "partisan" is equated with support for one of the traditional parties (p. 2) and, at the same time, with support of National Front governments (pp. 115, 117). Whichever is the appropriate conceptualization, the basic argument is undermined by the author's definition of professionalism which includes the rejection of partisan interests (p. 2). Consequently, if the Colombian armed forces behave in a partisan manner, as the author correctly holds, they are not professional; heightened domestic conflict should not, therefore, augment the military's political activity. Maullin discards an alternative to increased military participation in politics, namely the continuation, perhaps even reinforcement, of civilian supremacy, resulting from deeply embedded political traditions or ideological and policy preferences shared by politicians and military officers.

Third, the data presented are based largely upon secondary sources, often without much selective skill by the author. For instance, discussion of the Colombian economy rests upon material collected in 1953 (pp. 16–17). Moreover, invalid inferences are drawn from tables. To take only one example, "Table 3-3 shows that for the four congressional elections taking place when Colombia's insurgent bands were most active, antiestablishment political parties and groups were more successful in *municipios* that had suffered from insurgent political violence" (p. 49). Such is the case only in 1962; the relationship is not significant in 1964 and disappears entirely in the 1966 and 1968 elections. Moreover, portions of the data are based upon "miscellaneous unpublished estimates by informed observers" (p. 82). This is not startling, considering that the Rand Corporation sponsored the study, but I am disturbed by the quality of the data presented and strongly suspect that the author had access to materials not published in the book.

Fourth, the author fails to demonstrate a firm grasp of Colombian politics. He overemphasizes the jargon of Colombian *políticos* at the expense of analyzing what actually occurs. Colombian politics are permeated with what has been called the politics of *mamagallismo* (pretentious kidding), which bears little or no relation to political realities. Thus Maullin maintains that Colombian politicians are dedicated to the purposeful redistribution of wealth (p. 21)—certainly the case if one reads their speeches—yet such has not taken place. An International Labor Organization report suggests that ". . . concentration of income has not lessened during the last fifteen years; it might even have increased" (*Hacia el Pleno Empleo,* Banco Popular, Bogotá, 1970, pp. 433–434). This book also contains inconsistencies and factual errors. When Maullin discusses Colombian guerrilla movements, he describes them on page 52 as constituting the major opposition to National Front governments but later in the study (pp. 115–116) he assigns a similar role to ANAPO. The author terminates the National Front electoral arrangements in 1970 (pp. 114, 116, 126), when actually this was not the case at the national level until 1974. Finally, Rafael Reyes promulgated his military reform in 1907, not in 1914 (p. 2). Such errors do not lend much credibility to the book.

I was disillusioned after reading this sketchy, frequently simplistic, review of the impact of violence upon the Colombian military, particularly when one reflects upon the possibilities inherent in the subject. Perhaps the most significant conclusion to emerge from the study is that, contrary to the author's principal thesis, military professionalism, at least as conceptualized here, does *not* satisfactorily explain military involvement in politics, certainly not in Colombia and probably not elsewhere either.

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Priorities in Melanesian Development. Edited by Ronald J. May. (Canberra: Australian National University Press, 1973. Pp. 470. $9.00.)

Growing realization of the importance of the Pacific community of nations and the global search by social scientists for new areas of inquiry combine to make *Priorities in Melanesian Development* of much more than regional interest.

The book is a collection of the sixty-two papers presented at the Sixth Waigani Seminar held in Papua New Guinea in 1972. The general topic of the seminar, priorities in Melanesian development, is timely. As the colonial powers withdraw, it becomes increasingly clear that all wants cannot be satisfied. What options are open to people of limited resources? How will they make critical choices? Which needs should receive primary attention? Seldom have leaders of so many segments of a colonial society had the opportunity to consider alternatives before independence is achieved.

By and large the seminar participants were regional, but their backgrounds are highly varied—native, expatriate, politician, administrator, professor, student. Their printed speeches are arranged into five substantive categories: political development, welfare, education, economic development, and modernization and national identity. These headings indicate the broad scope of interests of the speakers. The prospective reader should be forewarned that the articles presume a wide acquaintance with Melanesian history, including knowledge of the very different development and political status of these island territories. For example, nowhere, not even in the editor's introductory remarks, is the geographical area which comprises Melanesia delineated, nor are the governmental units of the region listed.

It may be helpful to add a note regarding the area's political situation here. Melanesia is a term developed in the last century to indicate a group of islands in the southwest Pacific. Although they shared common prehistoric ancestors, the Melanesians developed into hundreds of societies. Even today, for instance, over seven hundred different languages exist in Papua New Guinea. The recent common bond of these island inhabitants has been their colonial status. How firm this bond will remain as the various units move into political independence is a matter of conjecture and concern. The present variety of political relationships held by Melanesian territories with metropole powers is amazing. Tiny Fiji became an independent nation and a member of the Commonwealth in 1971. Papua New Guinea is rapidly gaining increasing self-government from Australia. The British Solomon Islands, a territory of the Western Pacific High Commission, now has internal self-government. Yet New Caledonia remains an overseas territory of the French Republic and West Irian (formerly West New Guinea) is now a part of Indonesia. And the New Hebrides has the most unusual authority–joint colonial rule by Great Britain and France.

Returning to the book, one is not surprised to discover that the speakers could not agree on any set of priorities. Indeed it would be unrealistic to expect agreement, considering the spectrum of interests and backgrounds represented. Yet this documented lack of agreement has inherent value in demonstrating the magnitude of problems facing emerging microstates. If there is any thread of unanimity running through the speeches in the five problem areas it is the concern that Melanesians realize they have options for development and a warning that neither the Western model of politics, economics, education, social services, or modernization nor the paths taken by new nations of the Third World are necessarily appropriate for Melanesia. For instance, the use of the Westminster system of government as a model was the subject of a great deal of argument among the men (where else in the present-day world would a seminar of sixty-two invited speakers fail to include even one woman participant) who talked on political development. A majority thought that such a system of Government and Opposition parties would be harmful to the development of nationhood, believing it would stress disunity.

The medley of problems and issues explored in *Priorities in Melanesian Development* make this book useful as supplementary reading to political scientists and other social scientists whose interests touch on neo-colonialism, nationalism, constitution-making, public administration, or especially, the emergence of small, independent territories in international society.

L. PATTON DAVIS

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Popery and Politics in England, 1660–1688. By John Miller. (London: Cambridge University Press, 1973. Pp. 263. $14.50.)

Although not central to the concerns of political scientists, this book has a number of aspects which might be of interest to students of minorities, persecution, popular prejudice, propaganda, toleration and the history of the machinery of government. Not much of the book is entirely new material, but it is to be recommended, first, as a clear and thorough summary for those who want one; and more important, as being informed by a consistent theme, that of the difference between the way non-Catholic Englishmen perceived Catholics and Catholicism, and the reality of English Catholic life and of the politics of the papacy.

Dr. Miller steers clear of any speculative flights about political myth and selective perception, and contents himself with an account of the establishment of the Protestant image of Popery, and the manner in which it suited the purposes of some, and the worldview of others, to perpetuate it once established. Dr. Miller's abstemiousness in this respect is perhaps to be welcomed: a coherent narrative, informed by an awareness of the questions to be asked, may be all that *can* be provided. But he does offer remarks like: "It is of course a common trait of collective psychology to explain disasters or adverse circumstances in terms of conspiracies and machinations" (p. 89). Why then did the enforcement of the ferocious laws against Recusancy for the most part continue as lax and dilatory as ever, why did the average Justice of the Peace, or Sheriff, remain as capable of distinguishing between Popery and his Catholic neighbors as before, and why was very little mob violence directed at individual Catholics? On the evidence provided by Dr. Miller himself, there seems to be ample reason for suspecting that anti-Catholicism occupied some crucial place in the political self-understanding of Englishmen, a place which had nothing to do with any genuine Catholic challenge to Protestantism until the accession of James II.

And again: to the question why the dominant interpretation of Catholicism was impervious to correction by the experience of the actual conduct of actual Catholics and actual popes, Dr. Miller answers (p. 67) that it was because of (a) the Protestant view of Catholics and Catholicism that had become established, and (b) the way that view affected Protestant interpretations of current events. As it stands, this is circular, though not viciously so, because Dr. Miller fleshes out these answers with concrete detail. But here, as in the previous instance, one feels that a little more explicit theorizing might have improved the book.

On the other hand, the insights into the workings of seventeenth-century politics and the popular mind which this book permits, may have some value to political scientists. Thus, for example: we are shown how the rigor of the anti-recusancy laws and the occasional attempts of central authority to get them enforced were nullified by the unwillingness of rather independently minded "agents" of the Crown in the localities to do very much against their neighbors and social equals and those they shielded. One of the benefits of *not* centralizing. And again, toleration is once again shown to have been a *fact*, long before it became a desideratum; indeed the most notable theorists of toleration still specifically exempted Catholics from entitlement to the toleration which they had for the most part long enjoyed *de facto*, on the grounds that Catholicism was itself intolerant, and what is more, politically unreliable. And so forth. And the fact that Dr. Miller has not predigested his account through some theory, allows one a glance at this kind of political *detail*, which is the best corrective to the penchant for facile historical generalization, the besetting vice of at least some areas of political science.

H. M. HÖPFL

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The Consolidation of the South China Frontier. By George V. H. Moseley III. (Berkeley: University of California Press, 1973. Pp. 208. $10.00.)

The appearance of this pioneering study of Chinese Communist policy toward the minority peoples of the southwestern provinces of Yunnan and Kwangsi in the 1949–1960 period is the first monograph to emerge from the resurgent interest in two long neglected subfields of Chinese studies. I refer to Chinese relations with the minorities and the southwest China frontier. Professor Moseley's aim is to "trace the interaction between policy and people" (preface, p. x) which he has done with clarity and as much depth as his sources allow.

The theme of the author is that Communist policy toward China's minorities, which make up only 6 per cent of her population but are strategically located in her vast frontier regions where few Chinese live, has under the guise of granting political and cultural autonomy been fundamentally assimilationist. While striving to transform the age-old Chinese contempt for the minorities and the discrimination it bred into an attitude of benevolent paternalism (which they prefer to call socialist brotherhood), the Communists have also aimed at socializing minority societies.

Professor Moseley traces this process from the earliest efforts to build support among the southwestern minorities immediately after the Communist victory, which formed part of the strategy against Kuomintang guerrillas still operating on both sides of the frontier with the backing of some of the Chinese population. Ostensibly autonomous administrative units for the minorities were created in two stages. In the first stage autonomous districts and prefectures were established along the frontier up to 1954. In the second the pattern was repeated in the Yunnan interior while the entire province of Kwangsi became one of the nation's autonomous regions, despite its Chinese majority.

The author emphasizes that power in the newly created autonomous minority areas continued to rest with Chinese, who were represented at all levels of the new government structure and provided a sizable proportion of cadres sent to work with minorities. Further, the Chinese Communist Party (CCP) established its autonomous administrative centers in the Chinese-populated towns (pp. 64–65, 78–79, 110–113).

Professor Moseley goes on to discuss the problems and successes of the CCP in its drive to socialize and Sinicize southwestern minority cultures. His discussion demonstrates that CCP policy is an amalgam of new and old. He does not press the point, but then that is not his aim. For the historian, however, the compulsion to place the analysis in historical perspective is irresistible. The Communist effort to substitute class struggle for ethnic conflict, the drive to collectivize agriculture and the extension of economic, educational and medical facilities to the minorities seem to me to contrast sharply with the persistence of more traditional strategies of Sinicization still employed by the CCP. These include the use of old minority elites in positions of responsibility but not power, large-scale Chinese immigration into minority areas and sporadic attempts to suppress minority cultures (pp. 113–122). These two faces of minority policy suggest that although Chinese Communism has evolved some new methods for integrating the minorities into a new culture, the ancient Chinese goal of assimilation along with much of its strategy remains a feature of policy as well. Perhaps the major change in Chinese minority policy under Maoism is the replacement of its exploitative character by a genuine humanitarian concern for minority uplift.

Finally, the author deals with the role of the southwestern frontier minorities in Chinese foreign policy. This appears again to be a case of reworking an old pattern common in imperial days, known as "using the barbarians to control the barbarians" (i-i chih-i). The minorities are encouraged to maintain close relations with their compatriots on the other side of the frontier under the twin banners of freedom for minority cultures under socialism and the united anti-imperialist front. Professor Moseley breaks new ground by comprehensively reviewing minority policy in a specific region for the first time. This contribution in a hitherto uncharted field constitutes a service to China scholars.

H. MICHAEL METZGAR

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Religion and Political Culture in Kano. By John N. Paden. (Berkeley: University of California Press, 1973. Pp. 484. $15.00.)

How do new political forms gain legitimacy in the political culture of a society which is undergoing rapid economic development, urban growth, and shifts in its external political relationships arising out of the colonial experience and decolonization?

The answer developed in this informed and illuminating study of Kano, Nigeria's major northern city, is that the emergence of new religious organizations and of reformist religious thought developed and transmitted by the *mallam* class of religious leaders has facilitated the process by which the traditional religious foundations of political legitimacy are adapted to support revised concepts of community and authority. Professor Paden develops this thesis by focusing on two critical elements of political culture—authority and community—and by presenting parallel studies of the changing patterns of both religious authority and community and political authority and community in Kano from their pre- and early nineteenth-century roots to the present.

His study of religious authority and community could stand by itself as an important contribution to our understanding of the Islamic religious culture of Nigeria. The most important religious development is the rise of the Sufi brotherhoods, associations of discipleship which developed their own organizations, and hence, their own patterns of authority and community. "Reformed Tijaniyya," which became the dominant brotherhood in Kano, developed a transethnic authority structure and community membership, leading to a merging of ethnic identities and the reinforcement of a broader identity based on religious affiliation. It provided an experience with and a model of community integration of three sorts, elite-mass integration, ethnic group integration, and territorial integration, to an extent that was substantially different from other brotherhood groups and from the traditional political culture which had emphasized kinship ties and ethnic identity as the basis for political roles and social status.

In the several chapters treating the patterns of political authority and community, Paden examines the basic historical development of the Kano emirate and the Kano State, including the doctrinal basis for authority in both the fifteenth- and nineteenth-century religious sources, the structure of authority of the emirate as it has developed from the period of the Jihad down to the period of independence, and the recent transformation under civilian and military rule. Over this period, the Kano emirate developed with a strong separate identity, largely independent from Sokoto and with political loyalties focused on the Kano community. Yet by the early 1950s, there were already doctrinal challenges to the power of the Emir coming from the younger *mallams*, particularly Aminu Kano, and Paden provides a substantial selection from hitherto unavailable material expressing these political reformist views cast in the framework of Islamic doctrine. Also, political challenges came from the development of regional power in Kaduna, military power in Lagos, and most recently, the emergence of Kano State.

In relating the religious to the political developments in Kano, Paden makes his problem manageable by restricting his attention largely to the formal notions of authority and community, the rules and patterns of succession, the criteria for eligibility for office, the conceptions of law, and the interaction between ethnic, religious, and territorial conceptions of community.

This serves to keep in focus those aspects of the political culture which facilitate the drawing of parallels to the religious ideas and organizational developments. It does, however, make the elements of political culture rather formal and abstract, and it bypasses the working character of political life, the way power was exercised, and the processes by which political life was actually controlled and political conflicts were actually carried on or resolved. Much of this type of information has already been provided by other writers on Northern Nigeria, such as C. S. Whitaker, Jr. and B. J. Dudley, and Paden's work is thus complementary to theirs in that it neither repeats them nor supersedes them, but rather, opens and explores a dimension which they largely ignore.

Paden's treatment of the problem of community identity and the integration and differentiation of ethnic groups is also a valuable complement to the earlier work of K. Little on voluntary associations in West Africa and A. Cohen's study of the Hausa in Ibadan, both of which show how groups based on ethnic and kinship ties provide important support to the "strangers" settled in the urban areas, thus mediating the adjustment of migrants to the urban community by providing a differentiating support unit, rather than mitigating the differences and providing cross-ethnic integration in the urban areas. What Paden's work shows is the possibility that the religious brotherhoods may be vehicles for interethnic integration and rural-urban integration. They cut across ethnic and rural-urban lines in ways that other voluntary associations did not. The integration which the religious brotherhoods and leaders facilitate, however, is primarily among northern Muslims who have been traditionally divided by ethnic, class, or territorial lines. As Paden points out, it may promote differentiation as well, by separating the Muslims from the non-Muslim members of a national society.

Paden does not attempt an overall assessment of the broad processes of integration and differentiation in Kano society, or the relative weight of the religious factors as compared to other factors, economic, political, or social, in this process. He does, however, see the religious developments as preceding those in the political culture and makes a *prima facie* case for a causal relationship here. He also makes the point that important economic changes precede both the religious and political cultural developments, but he does not explore the significance to be attached to this fact.

This is a work of careful and informed scholarship, carried out within an explicitly formulated conceptual and theoretical framework, and is worth careful reading by those concerned with Nigeria, with the adaptation of Islamic organizations and thought to modern political life, or with the processes of change in the value orientations of a political culture.

CHARLES R. NIXON

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Structure and Conflict in Nigeria, 1960–1966. By Kenneth Post and Michael Vickers. (Madison: University of Wisconsin Press, 1973. Pp. 248. $15.00.)

This is a tragic story told in mock-heroic prose. Within five years of independence the Nigerian polity had been so bloodied by pervasive internal conflict that the military coup of January, 1966, was hailed by most Nigerians as a relief from the disgusting display. During that period political institutions were emasculated, official corruption swelled, and the countryside was bilked of the products of its labor but otherwise abandoned. In a series of court actions and fraudulent elections, the governments in power in the Regions and the Federal capital jailed opponents, smashed electoral alternatives by thuggery and acts of naked official power, wreaked havoc in the infant bureaucracy, and narcissistically deluded themselves that the system they were destroying would somehow succor them perpetually.

In this book Professors Post and Vickers attempt to explain the disintegration of the political structures extant at the time of independence. By specifically excluding the political behavior of the Nigerian masses, the analysis of social class, and the significance of external economic factors (p. 3), the authors are thrown back upon an analysis of the political elites and the legal structure as both phenomenon and cause. Methods for the study, however, seem (if the footnotes tell all—there is no statement on methods) to have relied heavily on newspaper accounts, political ephemera, and secondary sources, so we rarely get behind events very deeply. (The contrast is regrettably strong with the prodigious field work for Post's earlier book, *The Nigerian Federal Election of 1959* [London: Oxford University Press, 1963].)

As the authors would have it, then, Nigeria was a Conglomerate Society (their version of pluralism à la M. G. Smith and their capitalization), with a Structural Frame run by Structural Elites (read politico-legal system and bureaucracy), a Political Hierarchy (read mobilized population and participants in politics), and a System of Rewards. These nearly-German nouns are elaborated as "models" in the first three chapters.

In the remaining chapters, on politics from 1960 through 1965, the authors use the models to "explain" events. The focus is especially upon the Federal general election of December, 1964, with chapters on the issues, the campaign, nominations and the boycott, and the election aftermath—a shaky pact among party leaders too bound to the state to have it blow away.

The explanation is a common one for Nigeria—an unworkable federation, too many political demands, a bureaucracy that held up as long as it could, ethnic sections willing to forego the war to win the battle, inexorable pressures and polarization. In lieu of named culprits the models themselves are often reified as causes or goals: the "Structural Frame . . . had operated, in conjunction with the System of Rewards, to polarize the conflict. . ." (p. 185); the Action Group could try to "capture . . . and . . . modify, if not totally destroy, the System of Rewards" (p. 76). The human actors—the Ikoyi clique (p. 51), the Action Group turncoats who joined the NNDP (p. 101), the "National Reconstruction Group" (p. 75), the "new men" of the North (p. 209)—all these critical interest group members are unnamed and unexamined. That other African state systems with different primary factors have suffered the same fate, or that Nigeria did not finally "disintegrate" at all (even if the political structures did), and has instead gone on to become the eighth largest oil exporting country, and thereby to wax capable of satisfying vast new internal political demands, goes unreflected in the case study. The Capitalized Concepts march back and forth instead.

The result is sterile analysis choking up a reasonably deft recapitulation of the published details of the 1964 election. Other books on this period will offer more revealing interpretations: indeed, the senior author, who states in a footnote (p. 10) that this study is "epiphenomenal" and that only his newfound ("since the completion of this book") Marxism will achieve the explanatory goal, might yet give us one of these.

DAN R. ARONSON

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Reform and Rebellion in Afghanistan, 1919–1929: King Amanullah's Failure to Modernize a Tribal Society. By Leon B. Poullada. (Ithaca, N.Y.: Cornell University Press, 1973. Pp. 318. $14.50.)

This outstanding book deserves a much wider readership than that of scholars primarily interested in the history of South Central Asia. This is so because Professor Poullada, while concentrating on Afghanistan, in effect deals with much broader problems endemic through much of the developing world.

Poullada, who knows his country and his sources well, has written a careful and well-documented yet exceedingly readable book. It is neither a biography of the luckless King Amanullah, like Rhea Talley Stewart's *Fire in Afghanistan 1914–1929* (New York: Doubleday, 1973), nor a diplomatic history like Ludwig Adamec's excellent *Afghanistan 1900–1923* (Berkeley, Los Angeles: University of California Press, 1967). Poullada rather sees his task in testing—and disproving—the widely held thesis that Amanullah simply assaulted too many religious and tribal dogmas with his reforms and thus failed. Poullada's thesis, on the other hand, is of wider applicability. He shows that in developing countries, social and political-constitutional reforms simply have no chance until much greater national unification has been achieved than has been the case in Afghanistan. Moreover, Afghanistan's problem was compounded not only by conflicts between the ruling Pushtoons and the others (Tajiks, Uzbegs, Hazaras, etc.) but even more by the unending tribal conflicts among the Pushtoons themselves. Although the case of Afghanistan may be unusually complex for the above reasons, the lesson that constitutional government cannot precede national unification has wide applicability and ought to deter those who demand that developing countries give instant priority to political-constitutional (democratic) reform.

Poullada makes a good case for the thesis that reforms were possible but only through such infinite cunning, constant deception, and staggering cruelty as was practiced by Amanullah's grandfather Abdur Rahman. By contrast, Amanullah combined constitutional reform (which weakened his power) with social reforms (that aroused suspicion of the traditionalists). As a result he achieved neither but created uneven, unsettled conditions satisfying nobody.

As if this were not enough, Amanullah, partly at least under the influence of his strongly anticolonial advisor and father-in-law Mahmud Tarzi, never really made peace with his powerful British (India) neighbor nor established much lasting relationship with the Soviet Union. Perhaps if Amanullah's relation had been directly with London, a greater understanding of his considerable qualities might have evolved. But London insisted on going through Delhi, where the fierce tribal pride of the Afghans met little understanding. The Viceroy's government had become too used to the Indians who were at that time more obsequious than the independent-minded Pushtoons. Thus nearly insuperable domestic problems combined with unsatisfactory foreign relations to seal Amanullah's doom.

Several related generalizations can be drawn from Poullada's thoughtful work—generalizations which I found confirmed by a personal experience covering a much later period (1973) in which yet another king was overthrown. The book suggests that constitutional reform must follow, not precede, national unification in a tribal society—secondly that tribalism and constitutionalism are largely incompatible, and finally that social reform cannot succeed without a strong sense of national purpose and identity.

ROBERT G. NEUMANN

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A History of the Bemba: Political Growth and Change in North-eastern Zambia before 1900. By Andrew D. Roberts. (Madison: University of Wisconsin Press, 1973. Pp. 420. $15.00.)

Because this book is so obviously a major contribution to African studies it seems pretentious to burden Professor Roberts's *A History of the Bemba* with such a label. It serves many purposes and disciplines, however. Africanists concerned with politics in Zambia and with Central Africa will find a wealth of carefully integrated historical evidence interpreting Bemba political behavior. Political scientists seeking additional illustrations of patterns of political development can regard this book as an excellent case study of political change in precolonial Africa.

As a dissertation completed in 1966, it profits from long seasoning and represents an early example of scholarship drawn from sources based on local African influences and institutions rather than political history viewed at a distance through the telescope of Victorian imperialists or colonial office despatches. Analyses of this *genre* require a broader base of evidence than previous explanations of African political behavior, and the author's chapter on sources is unusually instructive. The recorded evidence which he uses consists of a wide range of the conventional sources: manuscripts and diaries of missionaries; company records (British South African Company); published and unpublished government documents; French, Portuguese, and German primary sources recorded before 1900, and the bulk of the social science analytical commentary on the Bemba and related groups. More important, this evidence is leavened, and often tested, with materials from the extensive oral tradition gathered and recorded since the time of early missionary activity, as well as during the author's field research in 1964–1965. He also notes other forms of evidence, such as personal relics of chiefs (which are inaccessible to outsiders), the sites of their villages, and the rituals of chieftainship. The author's scrupulous description of his sources is matched with careful acknowledgements of conjecture where the evidence seems insufficient for definitive conclusions, but more than adequate for informed speculation.

Although the author concentrates on the latter part of the nineteenth century, he traces Bemba development from the early eighteenth century until the initial European occupation in the late 1890s. The history concentrates primarily on the political development of chieftainships, focusing on the politics of succession, and the evolution of trading and relationship patterns with other tribes such as the Ngoni and the Lungu. One sees the gradual unfolding of the institutional structures and networks of political authority of various chiefs, as well as the gradual territorial expansion of the Bemba.

Noting—and certainly demonstrating—that "pre-colonial Bemba politics were largely a matter of dynastic conflicts" (p. xvii), Professor Roberts concludes that these conflicts produced a pattern of nonrepetitive development rather than cyclical, repetitive behavior. Reactions to internal and external variables produced cumulative political effects resulting in dynamic, rather than stagnating structures and policies. That the European intrusion coincided with a cyclical decline in the Bemba's territorial unity and political authority accounts in large measure for the relative ease with which the British occupied northeastern Rhodesia. The author argues that probably the Bemba deliberately chose not to engage in a test of strength at a time when their capacity to do so was doubtful. This is considerably different from other interpretations of colonial occupation and conquest which suggest that Africans chose between total opposition and total surrender. That British control was acquired gradually and comparatively peacefully, rather than through direct confrontation (as with the Ndebele in the mid-1890s), was of considerable importance in the future attitudes that the Bemba and British could have toward each other, as well as toward themselves. In the end. this led to structural continuity for the Bemba, which might well ultimately justify the claim of some African scholars that colonialism was only an interruption of African political development.

MARION E. DORO

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World Communism: A Handbook, 1918–1965. Edited by Witold S. Sworakowski. (Stanford: Hoover Institution Press, 1973. Pp. 576. $25.00.)

Annually since 1967, the Hoover Institution has been publishing the *Yearbook on International Communist Affairs* which surveys and summarizes the year's developments in world communist affairs. The volume under review is meant to complement these *Yearbooks* by filling in the background for the period 1918–1965.

This *Handbook* contains approximately 120 separate entries. Most of these fall into four broad categories: first, the entries listed by country which trace the history and evolution of the Communist movement in a particular nation; second, the entries devoted to the Communist parties of particular republics of the Soviet Union; third, those that deal with specific international Communist organizations (e.g., the Warsaw Treaty Organization, Comecon, the Comintern, etc.); fourth, those that examine various Communist controlled or dominated "front" organizations (such as the World Council of Peace). The individual sections are generally of a very high quality since they are written by acknowledged experts and well-informed specialists (e.g., Theodore Draper on the American and Cuban Communist Parties, Robert A. Scalapino on Japan, John S. Reshetar, Jr. on the Soviet Union, Stephen Fischer-Galati on Rumania, etc.).

This *Handbook* will prove valuable both to professional researchers in international communism and to university students who are interested in a quick overview of particular Communist parties. The entries are concise and to the point, and they provided valuable information organized under such convenient headings as: History, Organization and Membership, Relation to the Soviet Leadership, Party Press Organs and Records, Chronology of Party Congresses, and Future Prospects. Each entry ends with a brief bibliography which quickly leads the student into the mainstream of the relevant literature. This volume is well conceived and well executed, and the editor is to be commended for having assembled so much useful information in such a readily accessible form.

PAUL MARANTZ

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Voting Behaviour in a Changing Society: A Case Study of the Fourth General Election in Rajasthan. By S. P. Varma and Iqbal Narain. (Delhi: National Publishing House, 1973. Pp. 385. Rs. 45.00.)

Voting studies have long comprised a kind of centerpiece for students of politics in democratic political systems. Such studies are often seen not only as revealing the basis for voter preference but also as providing a measure of the very development and character of the political system. Individuals' belief that they have influence in the political system, for example, is deemed important because, as Almond and Verba have suggested, people who believe in their political efficacy are more likely to participate politically; and the more people who seek to participate, the more responsive a system will be.

The Varma study is in keeping with this general frame of reference. By examining Indian voting behavior—more particularly voting behavior in Rajasthan in the 1967 general elections—S. P. Varma, Iqbal Narain, and their associates at the University of Rajasthan seek not only to understand what motivates participation but also to generalize from the observed patterns of participation to the current state of Indian democracy.

Within narrow limits, the authors succeed in accomplishing their purposes. Their analysis of demographic characteristics, socioeconomic status, political information, and media of communication as determinants of participation is highly informative, if only to show the limitations of these variables in actually explaining participation. Unfortunately, the Varma study echoes all too many voting studies in its most general conclusion: that the principal motivation for an individual to participate politically is his sense of political efficacy. The conclusion is "unfortunate" because it really says very little. There is already ample evidence to support the conclusion reached here, but all of this evidence merely tells us that those who believe voting to be important and effective are more likely to vote than those who do not so believe. A more basic question needs to be asked: How is it that some people come to believe in their own political efficacy and others do not? On this, the Varma study offers little help. None of the variables considered offers conclusive explanation. There appears to be, for example, surprisingly substantial participation among less literate, low SES categories and, at the same time, substantial nonparticipation among the most urbanized and literate section of those surveyed. Neither of these phenomena, incidentally, are sufficiently scrutinized.

Against the background of these data it is really very difficult to say anything at all about the "state of Indian democracy" except that it exhibits a general pattern of limited participation. The authors note that "we have a long way to go" before a democratic polity can be achieved. Having explained the limited participation that exists primarily in terms of political efficacy, however, they provide us with no understanding about how that path toward a democratic polity is to be negotiated.

GERALD A. HEEGER

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Chartism. By J. T. Ward. (New York: Barnes & Noble, 1973. Pp. 286. $12.00.)

Chartist studies in Britain are in danger of losing all sense of direction: for interest is now chiefly concentrated on the careers of particular individuals and on regional variations within the movement. There is a clear need for new themes and general ideas to replace the largely left-wing assumptions which once held sway in discussions of "the first working-class political party" (p. 7). Dr. Ward of Strathclyde University fails to seize this opportunity. He prefers grinding factual detail to bold speculative comment. His book provides à densely packed synthesis of his own and other people's work (based mainly on the newspapers of the time), but lacks an intellectual framework into which the synthesis can be fitted. In his view indeed it is not possible to construct such a framework. Some places went Chartist because of the well-known horrors of the Industrial Revolution: by contrast other Chartist strongholds in the provincial backwaters (like Jane Austen's Bath) bitterly resented the fact that the Industrial Revolution had passed them by. Both the backwaters and the centers of industry were left to go their separate ways by the leaders of the movement, who were far more interested in their own private quarrels than in devising a formula which would unite their diverse followers. According to Dr. Ward the most that a book on Chartism can do is to record these perplexing cross-currents. He offers cold comfort to all those who wish to arrange political events in firm structures.

If this book has a message, it is that radical agitation in early nineteenth-century Britain posed no great threat to the traditional governing classes: but this emerges mainly through purely incidental comment, as for instance in a short passage at the beginning where Dr. Ward disposes briskly of the popular belief that the 1832 Reform bill narrowly averted an epic social struggle which the aristocracy must have lost. However, on the larger question of how narrowly an inconsequential Chartist uprising was averted in 1839, Dr. Ward is unable to provide a verdict. All that can be said is that the forces of law and order at this period "were supported by a considerable body of loyalists, largely 'Church and King' Tories, whose activities have never been recorded" (p. 42), simply because the working-class opponents of radicalism have never been regarded as interesting or significant. Yet there were so many of them that the special elections organized by the Chartists to illustrate the superiority of universal suffrage became memorable chiefly for their hopelessly undemocratic character. In plain terms, early Victorian England preferred the politics of consensus to class conflict. For their part the governing classes, remembering Peterloo, showed great wisdom in avoiding provocative gestures which alone could have turned the fragmented elements that kept Chartism weak and divided into a determined radical force. As a rule, prominent figures in the movement were arrested only after they had brought grave discredit on themselves. At the height of the "crisis" the army was employed extensively in the north of England, but care was taken to find a commander noted for his liberality. Though never developed to the full, such points do help to modify somewhat the stiff tone of this unremittingly informative work.

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The Human Face of Socialism: The Political Economy of Change in Czechoslovakia. By George S. Wheeler. (New York: Lawrence Hill and Co., 1973. Pp. 174. $7.50.)

The Czechoslovak Reform Movement, 1963–1968: A Study in the Theory of Socialism. By Benjamin B. Page. (Amsterdam: B. R. Bruner B. V., 1973. Pp. 127. $8.00.)

These two small books—both written by American Marxists—present a sharp contrast in interpretation of the efforts to reform communism in Czechoslovakia in 1968. George Wheeler, who emigrated to that country in 1952 and became a member of the Economics Institute and later a corresponding member of the Czechoslovak Academy of Sciences, offers a relatively positive analysis of the achievements of the centralized management system but follows this with a sharp criticism of its defects and a somewhat superficial and uncritical portrayal of the reform efforts in 1968. Benjamin Page, whose background and occupation are not revealed, expresses some sympathy for the reform but makes a severe and fundamental criticism of it which is marred by lack of conceptual clarity and a onesided and sometimes misinformed interpretation.

Wheeler condemns the Soviet intervention, but Page refrains from expressing either approval or condemnation. Each book offers certain information about 1968 and is interesting for the distinctive viewpoint of the author, but neither contributes to a deep and solid understanding of the reform movement.

Both authors are ardent believers in socialism and both recognize the need for reform of the model introduced in the 'fifties, but their interpretation of socialism and of its reform differ vastly. Wheeler is a staunch defender of the "socialism" established in Czechoslovakia after February 1948, and even praises the initial use of dictatorial methods, and the introduction of the Soviet model, including the centralized planning system. Using official statistics, he sets forth the successes achieved by the latter, especially as regards economic growth. He admits the ultimate breakdown of this system, culminating in the failure of the Five-year Plan adopted in 1962, and recognizes other sources of discontent, such as censorship, lack of initiative and the scientific spirit, and the dictatorship established under Novotny. He does not regard these as representing a failure of socialism as such, and he approves the efforts to develop a socialist market, combined with planning, as advocated by Ota Šik. Surprisingly, and contrary to the facts, he argues that these changes were not unlike the reform trends in the USSR and the other socialist countries. His analysis of Czechoslovak economic development and the weaknesses of the economy is useful, but his treatment of 1968 is limited to a summary of the Action Program (with a bare mention of the enterprise councils) and is thin and unoriginal. He criticizes the Two Thousand Words declaration and notes dangers to socialism but denies a counterrevolutionary situation or the threat of a return to capitalism. The main cause of the Soviet intervention, he believes, was not the economic reform, but the proposals to democratize the political system and to permit competition for the Communist party.

Page is a critic of socialism as it exists in all socialist countries, which he finds wanting in terms of an abstract model derived from Marx. His critique of the Czechoslovak reform movement rests largely on his interpretation of the Action Program as "a managerial and technocratic manifesto" which would establish not a socialist system, but a form of managerial capitalism in which the working class would not rule. He offers a detailed summary of the pre-1968 reform ideas expressed in the writings of Šik, Radovan Richta, and Zdeněk Mlynář (whose work he does not consider Marxist). He rightly distinguishes the official viewpoint of reform expressed in these writings and the Action Program from the wide-ranging post-January ferment of reform ideas, but fails to recognize the rich diversity even among the official reformers, their shifting views after January, the interpenetration of official and unofficial viewpoints, and the changing balance of forces which was likely to produce a more radical reform program. His criticism of the pre-1968 and official approaches therefore distorts the full meaning and implications of the post-January development.

Page's specific criticisms, although not always without point, betray intellectual confusion and a lack of understanding of urgent practical problems facing Czechoslovak society. Thus he criticizes the centralized system of management as applied in Czechoslovakia, but argues that the cause of the crisis lay not in the system itself but in international factors. He does not accept the necessity, recognized by almost all Czech and Slovak economists, for a greater play of market forces and for greater managerial independence as means to improve the efficiency of the economy. Nor does he show any awareness of the need for curbing the dominance of the party apparatus in the economy, rewarding the educated and the skilled, and modernizing the economy—all of which were driving motives for the reform and key objectives. Although he criticizes the introduction of the Soviet model, Page is ambiguous about the "national path to socialism," and condemns the 1968 reform as being merely a return to this path as introduced in the period 1945–1958—an extraordinary distortion of the efforts of the reformers to develop a new model of socialism differing substantially from the postwar pattern.

The main thrust of his argument is that the reforms would lead to rule by the experts, but he uses this term sometimes to refer to managers and technicians, sometimes to scientists, and sometimes to undefined "bureaucrats." Although he explicitly declares that this kind of rule would not mean a return to capitalism, he argues repeatedly that Sik's reform plan would produce all the essentially negative features of capitalism. He contends that this would lead toward a "consumerist society," thus seeming to be unaware of the urgency of improving consumer-goods production and raising the standard of living, and argues at the same time that consumers would have no protection against enterprise domination of the market. While lamenting that the working class would not rule in the reformed society, Page does not mention the extraordinary revival of the trade unions and the official acceptance of the right to strike, and he minimizes the significance of the projected working people's councils. Yet all of these developments would have represented serious checks on enterprise arbitrariness and would have constituted elements of direct workers' democracy, which Page favors. He berates the bureaucracy of the old system, but minimizes the efforts of the reformers to subject bureaucracy to control, not only in the factories through the councils, but also in the political system as a whole through the separation of powers, an elected parliament, and an independent judiciary.

In summary, then, although he gives some faint praise for the Czechoslovak reforms and does not approve the Soviet occupation, Page presents a severe but confused indictment of the reform movement which, while differing essentially from the Soviets' condemnation, sometimes resembles their specific criticisms.

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Immigrants and Politics. By Paul R. Wilson. (Canberra: Australian National University Press, 1973. Pp. 175. $10.50.)

Australia is a relative newcomer to the countries of large-scale immigrant absorption. Prior to 1945 its intake was small, and the criteria restrictive—based on a desire for the continued dominance of a "British" Australia, where all newcomers, however few, would be subject to the pressures for Anglo conformity. Between 1947 and 1970 over 2½ million migrants arrived in Australia, of whom only about 1 million were British; the rest were Italian, Greek, East European, Dutch, Yugoslav, German, etc. Thus in a population of approximately 11½ million in 1966, about one in five was either a postwar immigrant or the child of one. By 1972 it was estimated that about 18.5 per cent of the voting population was foreign-born.

This short volume seeks to chart the entry of these immigrants into Australian political life, and in particular to examine their pattern of political participation. It is based upon a survey conducted among 456 Australian-born, 223 British immigrants and 225 Italian immigrants in the city of Brisbane in 1967 and 1968. Its main focus is upon the "passive" and "active" participation of these various groups in Australian politics, the sociological and demographic factors associated with the participation of the two immigrant groups, comparisons with their patterns of political participation prior to migration, and a brief analysis of partisanship.

The book skillfully contrasts the differences between the two groups and relates these to their previous political behavior, the motivations for migration and the current concerns and preoccupations of the immigrants in Australia. At all times the author informs his work with the relevant academic literature.

The author adds significantly to our knowledge of immigrant political participation in Australia and not only because there is so little published in this field. The importance of his contribution lies mainly in his careful analysis of the factors which promote and retard political participation among migrants, and his development of a model that seeks to account for all the relevant influences. He is at his best in analyzing the reasons for immigrants becoming more highly politically involved in Australia than in their homelands.

It is for this reason that it is a pity that his analysis is restricted to two groups in one city. Only when the analysis is carried out nationally covering all the important immigrant groups will we be able to fully assess the reasons why immigrants have had so little impact on Australian politics, and have blended imperceptibly into an immigrant version of the prevailing low-keyed, almost apathetic, participation of "old" Australians.

The book before us has cut away much of the undergrowth, asked many of the right questions, and answered them in a satisfying manner. It is to be hoped that the present author can be encouraged to pursue his work further, or at the very least, that others will be encouraged to follow on and widen the path he has so ably demarcated.

PETER Y. MEDDING

Monash University

Die Grosse Krise in America: Vergleichende Studeien zur Politischen Sozialgeschichte 1929–1939. By Heinrich August Winkler. (Gottingen: Vandenhoeck & Ruprecht, 1973. Pp. 243. Dm 34.)

The authors of the seven articles of the sixth volume of the series *Kritische Studien Zur Geschichts-Wissenschaft* are specialists in U.S. history, but their essays are intended for Germans. Their value for the American reader will be found in developing a better understanding of historic and economic differences between the two countries by viewing the New Deal through German eyes.

In the introductory essay, the only one written by an American, Ellis W. Hawley explains why the growing managerial revolution encountered little opposition in America. The absence of any military caste with obsolete pre-industrial values, as much as the failure of the petit-bourgeoisie to oppose the rise of the managers to their new economic power, contributed to a basic acceptance of industrial progress and big business. Thus, while in Europe the strength of nationalism directed the rising discontent caused by the Depression, Americans attempted to correct the decline in prosperity through greater economic cooperation. Not political but economic problems were to be cured by the guidance the New Deal provided. The New Deal became the synthesis, which led the managerial revolution into a "higher form of a mixed economy and of democratic pluralism" (p. 32).

The fact that Jurgen Kocka considers the salaried white collar workers separately from the other unions during the New Deal, treated separately by Peter Losche, must be explained by the pronounced distinction Germans make between salaried and hourly workers. In the U.S., where most of the class and of the pay differential between *Angestellte* and *Arbeiter* have disappeared, such differentiation is senseless. Indeed, Kocka correctly states that the integration of the white-collar salaried employees and even of professional engineers into the American system of unions took place easily during the Depression because there had been no limiting class-consciousness in either group (p. 61).

The theme of Losche's article is the integration of labor unions into "organized capitalism." With the help of the Wagner Act and NLRB, unions not only prospered, they became an integral part of the system. Unlike their German counterparts, the American unions had never opposed the state, and despite the great militancy with which they often conducted strikes, American unions always worked within the political framework. During the New Deal, cooperation between state, industry and workers improved, and employers were forced to accept the unions as partners (p. 97).

Professor Hans-Jurgen Puhle discusses the New Deal as part of the progressive struggle in America, relating it to crisis and populism. He traces the great faith in capitalism which characterizes the American farmers to their origin as adventurous settlers in an unexplored wilderness. Hellmut Wollman views the housing policies of the New Deal as part of the general struggle of American cities against an increasing deterioration into urban slums caused by overcrowding and shabby building of large tenement districts. Professor Willie P. Adams's discussion of the New Deal in the courts is least interesting to American readers. It reviews the story of the judicial struggle which the New Deal legislation underwent in the U.S. Supreme Court and explains to German readers the American system of Constitutional review.

The final essay by Professor Winkler examines the "Anti-New Deal Movement." It notes that there never was the basic opposition to democracy among American employers which existed in Germany when the Depression hit (p. 218). The abortive efforts of Father Coughlin, the half-baked economic ideas of Townsend's Union Party, and the clerical Fascism of Gerald L. K. Smith could succeed as little as the German Bund and the Silver Shirts because there was no power-vacuum in America like the one that then existed in Germany. The failure of its adversaries proved the strength of the political system which had spawned the New Deal, Professor Winkler concludes; but he has "some doubt whether today's moral crisis is met with a determination equal to the one with which America faced the Depression" (p. 231).

GERD W. EHRLICH

Towson State College

Twenty Years Indonesian Foreign Policy, 1945–1965. By Ide Anak Agung Gde Agung. (The Hague: Mouton, 1973. Pp. 640. Guilders 69.)

Apart from his service as Foreign Minister in 1955–56, career diplomat and Balinese prince Anak Agung was neither a prime architect nor a major implementer of Indonesian foreign policy during the troubled first generation of the Republic from 1945 to 1965. Nonetheless, students of both Southeast Asian and Third-World foreign policy cannot neglect this uneven and bulky chronicle. Not only is Agung's book one of the very few attempts by an Indonesian political or diplomatic figure to write his memoirs, but it is largely representative of what Agung calls the "center-right" segment of the pre-1965 Indonesian political elite. Concentrated in the Indonesian Socialist Party and the modernist Islamic Masjumi Party, Agung's political grouping also displayed the legal-administrative political skills characteristic of Herbert Feith's "administrator" type–in contrast to the Sukarno "solidarity-maker" type. Throughout his political-diplomatic career as head of the Dutch puppet state of East Indonesia in 1948–1949, as Ambassador in Western Europe from 1950–1955, as Foreign Minister, and then from 1957 as a highly vocal critic of Sukarno's leftist policies until his imprisonment in 1962, Agung espoused the typical center-right administrator's foreign policy orientation. Fundamental was the pronounced Western bias placed on Indonesia's "active and independent" nonalignment policy. Tacitly looking to the American Seventh Fleet for military protection, they also invited dependency on the capitalist states for economic, technical, and cultural assistance as the necessary price for sustaining both economic development and their own political power. Understandably Washington viewed the administrators as "the responsible moderates" throughout the pre-1965 years.

Given Agung's political orientation, the purposes of this book are predictable. The first half of its nearly six hundred pages seeks to demonstrate how during the pre–Guided Democracy years Dutch and American intransigence on the West Irian issue undermined the best efforts of administrator cabinets to solve the issue and thus prevent the rise to power of

Sukarno and his Communist backers (PKI). A subordinate theme recounts how the arrogance of Indian leaders reinforced the radicalizing consequences of the festering Irian question. Wounded by Nehru's patronizing rebuff to his bid for an Afro-Asian leadership role, Sukarno responded increasingly to the charm and skill of Chou En-lai's diplomacy first displayed at the April 1955 Bandung Conference.

The second half of the book concentrates on detailing Sukarno's betrayal of nonalignment principles for a radical nationalist confrontation foreign policy. Most conspicuous in the "crush Malaysia" policy and the attendant estrangement from the United States, Sukarno's embrace of confrontation concurrently meant, as Agung stresses, the cementing of relations with both the PKI at home and Peking abroad. In a brief epilogue, Agung applauds Suharto's restoration of Indonesian foreign policy to its traditional and proper principles of an "active and independent" nonalignment with, he might have added, the familiar Western tilt of the administrators of the 1950s.

As reflected in this brief recital of the book's contents, Agung has attempted much more than the conventional personal memoir. Driven by his evident personal and ideological bitterness toward Sukarno—as well as, perhaps, by a sales-minded publisher—Agung has sought to produce a general interpretive history of Indonesian foreign policy from 1945 to 1965 in order to highlight his indictment of Sukarno's Guided Democracy (1957–1965). Politically isolated or in prison through most of that period, Agung relies almost exclusively on Western secondary sources to patch together rather repetitive and polemical chapters on subjects he knows largely second-hand. For example, his personal outrage at American Ambassador Jones's "supine" diplomacy toward Sukarno leads him into an unbalanced portrayal of both Jones's role and the objectives of Washington's policy.

Happily, the first part of the book tends to compensate for some of the inadequacies of the second part. Here, at least intermittently, Agung relies on personal experience to offer not only a fresh Indonesian vantage point on controversial moments in Indonesian foreign relations, but also some insight into Agung's own attitudes and behavior. Especially illuminating is Agung's chapter on his seven-month tenure as Foreign Minister in 1955–56. Drawing on some hitherto unpublished personal papers and recollections, Agung provides an informed and spirited defense of his handling of the dominant foreign policy preoccupation of the Harahap cabinet—the arranging and conduct of what proved to be the last bilateral Dutch-Indonesian negotiations on the already embittered controversy over West Irian. Convinced that progress on the West Irian question could have deprived Sukarno and the PKI of the inflammatory nationalist issue on which they would soon ride to political power, Agung condemns both Secretary Dulles and Dutch Foreign Minister Luns for frustrating his initiatives. Among the domestic political forces involved, Agung charges the Sukarno-led forces with blatant opportunism, but regrettably omits all mention of the role of the increasingly politically assertive army leadership. More serious is Agung's conceptual failure to recognize the probability that even the resolution of the West Irian issue in 1956 would not have prevented the rise of Sukarno, the army and the PKI. As Herbert Feith, Frank Weinstein and others have contended, is it not more consistent with the character of Indonesia's political environment in the 1950s to assume that functional equivalents to the West Irian issue would have arisen as channels for the deep frustrations of a society stirred by nationalist and economic aspirations yet trapped in the syndrome of underdevelopment?

Agung's inattention to this possibility indicates more fundamental assumptions about the class and cultural roots of Indonesian politics. His is in the end a very narrow western elitist view of Indonesian politics. Not only is there little sense in his book of the class element in the policy of the administrators, there is also no evidence of a personal capacity to empathize with the poverty of the peasant masses. They remain a dim abstraction introduced only as an addendum to stereotyped anti-Communist allusions to the PKI and its mass support.

If the class dimension is absent from Agung's analysis, so is the cultural perspective. Hazardous as a cultural roots approach is to any politics, to ignore it is to flaw understanding. In Agung's preoccupation with charting the surface movements of international politics, individual actors seldom appear more complex than their political positions. Agung's research into Western sources clearly left him indifferent to the seminal work of scholars such as the anthropologist Clifford Geertz and the late historian Harry Benda.

Still it is to Agung's credit that he has offered Western readers an Indonesian perspective on several international events hitherto seen too exclusively through Western eyes. One hopes that his example will not only spur more Indonesians to produce memoirs, but also stimulate the Suharto government to permit even political prisoners to write freely.

FREDERICK BUNNELL

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The Treaty Maker's Handbook. Edited by Hans Blix and Jirina H. Emerson. (Dobbs Ferry, N.Y.: Oceana Publications, Inc.; Stockholm: Almqvist and Wiksell, 1973. Pp. 355. Price not indicated.)

This reference book is basically what the title indicates—a handbook for those legal specialists, government workers, and policy makers directly involved in the business of writing and dealing with treaties, conventions, declarations, pacts and other forms of international agreements. It contains constitutional provisions governing treaty-making powers of a large number of states and organizations, an extensive sampling of clauses covering a wide range of treaty questions, texts of types of treaties and similar instruments, and the full text of the Vienna Convention on the Law of Treaties.

The *Handbook* grew out of two seminars held in Sweden in 1966 and 1967 by the Dag Hammarskjold Foundation, under whose sponsorship this publication appears. It is an expanded and systematized version of the materials prepared for the seminars which were under the co-direction of the book's co-editors: Hans Blix, Legal Advisor to the Swedish Ministry of Foreign Affairs, and Jirina H. Emerson, who has been engaged in research on the law of treaties at Columbia University.

Although there are highly specialized books covering clauses and treaty provisions, this is the first to provide a selective, but still comprehensive, and reasonably current source. The authors have combed through constitutions and the vast treaty collections, primarily as found in the *United Nations Treaty Series,* and abstracted sample clauses from both bilateral and multilateral treaties—broadly representing nations in all areas of the world and a wide variety of international organizations. In essence, the volume is addressed to the multitude of questions which arise in the writing and study of treaties—what is the meaning of reservations, when do treaties come into force, when are they suspended, etc.

The first and last sections are distinct. The first contains clauses from constitutional provisions on the conclusion and application of treaties. The sampling, primarily from Amos J. Peaslee's *Constitutions of Nations* (rev., 3d ed.; 1965–1970), reflects both the basic documents of international organizations as well as national constitutions. The citations in this, as well as other sections, are heavily weighted toward instruments coming into force in the 1950s and 1960s, thus giving the collection a current flavor. The final section reproduces texts (with selected articles deleted in some cases) of types of agreements including treaties, protocols, and agreements as well as individual samplings of a pact, convention, memorandum, *modus vivendi,* declaration, final act, etc.

More than two-thirds of the volume (sections 2-20) is devoted to sample clauses grouped within 19 categories. Within this portion of the book are different kinds of formal treaty provisions or methods of expression such as consent to be bound, entry into force, duration, termination, withdrawal and denunciation; revision, enforcement, breach, suspension and expulsion; internal and external application; and interpretation, application and settlement of disputes. Also included are sample clauses of various instruments used in connection with treaties such as full powers, ratification, reservations and amendments as well as special topics such as depository functions, registration, state succession, and preambles.

The main value of this reference book is that the user is able to find sample treaty clauses quickly, either for illustrative purposes or to answer perplexing questions. The 21 major sections are well defined as noted above but, as a further aid, the authors have broken these down into subareas. For example, the section on state succession includes clauses grouped under automatic succession to a treaty, optional succession, continued application until withdrawal or accession, non-succession, and non-succession implied. A similar pattern is followed for the other major sections.

As noted in the preamble, selection of areas, choice and number of examples, or arrangement of materials can be questioned. But these challenges are more likely to come from treaty specialists than from the student of international law. For them, the lack of accessibility of representative treaty clauses and provisions has been a not inconsequential frustration. They should join the lawyers and policy makers in welcoming the new *Handbook.*

CLIFTON E. WILSON

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The Selection of National Party Leaders in Canada. By John C. Courtney. (Toronto: Macmillan of Canada, and Hamden, Conn.: The Shoe String Press, 1973. Pp. xiv, 278. $12.50.)

This is the first book-length treatment of leadership conventions in Canada to appear, and it is a competent, interesting, well-written, and straightforward study. It shows how leadership selection parallels so many other Canadian phenomena by first copying British patterns, and then, in a peculiarly restrained and serious fashion, emulating American ones. The Liberals in 1919 were the first major party to hold a leadership convention as a means of uniting a divided party, most of whose M.P.'s came from the French Canadian province of Quebec. The operation was a great success for the party, which became the ruling party of the country thereafter (with a couple of interruptions). The Conservatives and others followed for similar reasons, and the system was firmly instituted.

The heart of Professor Courtney's study is an analysis of the Conservative and Liberal conventions of 1967 and 1968, relying heavily upon data obtained through mail questionnaires sent to the delegates. The rationale for the convention is its "democratic" and "open" characteristics. It appears to take the power of decision out of the parliamentary caucus and the small elite around it, and give to it the representatives of the party across the country—a substitution of one elite for another, as this author shows. The openness comes by permitting virtually anybody to contest the leadership—although of course there are always only a few serious candidates, determined by associations, experience, ability and money. The nuisance candidate serves a useful role in demonstrating the openness of the convention, and thereby masking the elitist nature of its operation. Most of the delegates come from the individual constituencies where they are supposedly chosen by democratic methods. About a third of the total are delegates-at-large (Conservative) or *ex officio* delegates (Liberal). These, of course, tend to be insiders.

Courtney points out that Canadian conventions represent young people and women better than American ones, and tend to over-represent the areas where the party is strong. This is true, and it ought to be so, since the convention is representing the party and not the electorate. Indeed in the 1976 Conservative convention, the large representation from Quebec, where the party scarcely elects any M.P.'s, nearly foisted a French Canadian leader on the most Anglo-Protestant of parties. So, contrary to Courtney's position, the parties may not draw enough delegates from their strongholds! The author shows the delegates, unlike the American ones, to be free agents, voting in secret; approximately a third of them are undecided before their arrival about whom to support. As a group, they are prosperous, middle-class people.

The study opens a door on the party system by showing the Liberals as a normal governing party, eschewing factional conflict to preserve unity and effectiveness. They co-opt notables, often with little parliamentary experience, who will have popular appeal and have qualities suiting them to deal with the current major problem facing the country (King to cope with labor unrest in 1919, Trudeau to handle English-French relations in 1968). They tend to come from central Canada and from the walks of life where co-opting is possible (the civil service and the business corporations). The Conservatives, normally the opposition, tend to choose provincial leaders, often from the outlying areas, who are English-speaking Protestant lawyers, because "the caucus bears the marks of electoral defeat." The author applies interaction theory to a choice of four leaders, to show how the parties have chosen men who would attract support because of their orientation to a pressing national problem.

Courtney shows how choosing leaders by convention benefits the parties by attracting popular interest, especially before elections, and by integrating parties that are prone to divisions because they are composed of representatives of vastly differing communities in a large and diverse country. While the candidatures of party dissidents have served to focus attention on important problems, the conventions have served to develop and enunciate policy. There have been problems, however. Conventions tend to focus on qualities other than those of a good parliamentary leader, often leaving the party stuck with a man who could not perform his role, yet who could not readily be deposed because of the legitimacy conferred by the convention process.

The convention process itself has its weaknesses. The delegates are too isolated and deprived of information while the voting is going on. So instead of party bosses telling their men what to do, one finds delegates seeking access to television sets to learn the positions of leading figures, which are better known to the public watching the screen than to the participants. Therefore strategic moves like forming coalitions, and retiring aspirants throwing their support to others, are difficult to accomplish. The author soundly concludes that this is no great loss. He argues, however, that the advent of the convention has produced a plebiscitary mentality that accords ill with the parliamentary tradition, and exalts the leader, especially if he is Prime Minister. Moreover the emphasis on advertising slick manipulative techniques in the preconvention goings-on downgrades the solid values of parliamentary experience and long-time service.

This book is a firmly based analysis and description of the history and practices of conventions in Canada that choose party leaders. There is a chapter on the New Democratic Party, but the empirical research is mainly concentrated on the two old parties. The significance and implications of the practices are amply explored.

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Le Conflit de Chypre, 1946–1959, 2 volumes. By François Crouzet. (Brussels: Établissements Émile Bruylant, 1973. Pp. 1187. 3.040 Francs Belges.)

Cyprus 1958–1967: International Crises and the Role of Law. By Thomas Ehrlich. (New York: Oxford University Press, 1974. Pp. 164. $1.95, paper.)

Cyprus: Reluctant Republic. By Stephen C. Xydis. (The Hague: Mouton, 1973. Pp. 553. Guilders 86.)

The developments of 1974 in and around Cyprus have increased the significance of the works reviewed in this essay in three ways: first, because of the background information to the Cyprus Question included in them; second, because of the issues and questions that the three works raise and attempt to analyze; and third, because of the implications that the attitudes, actions, negotiating tactics and positions of the main protagonists in the earlier phases of the conflict may have on the future of this conflict.

The book by Ehrlich is part of a series examining the role of law and legal institutions at times of international conflict. Without excluding factors other than law in crisis decision making, Ehrlich attempts to discover how law operates to define and limit the objectives of nations and the optimal means of realizing them. Four critical decisions from the long history of the Cyprus Question are analyzed in *Cyprus 1958–1967:* (a) Britain's 1958 decision to relinquish its sovereignty over Cyprus; (b) the 1963 Cypriot decision to propose revisions to the Zurich-London agreements; (c) the Turkish decision to bomb Cyprus in 1964; and (d) the Greek government's 1967 decision to withdraw its troops from Cyprus.

In his brief yet coherent analysis of these complex decisions, Ehrlich defines the role of law in terms of its probable perception by the major participants in each decision. The author acknowledges that he has discussed these decisions with representatives of the governments involved and that they also had a chance to make comments on his analysis of the events. The discussion of why and how these governments acted often is speculative, reflecting the problem of assessing recent decisions of foreign governments with no privileged access to their archives. Ehrlich concludes that law does operate to structure national decision making in the international arena, by formulating issues for decision; channeling the authorization and exercise of power; as a tool of advocacy of policy makers; and through substantial involvement of international organizations. Other common links in these cases, according to the author, are the emphasis by all governments on treaty obligations and rights; the lack of an ordered system of treaty revision and of procedures for the participation of groups other than national governments in most international organizations.

Cyprus 1958–1967 is a useful book for area specialists as well as for those interested in international law. The reader will find of interest the critical comments supplementing the Ehrlich essay by Louis Henkin, Edwin C. Hoyt and Hans Linde. Ehrlich's footnote citations are also useful, but are based on predominantly English language sources, thus excluding a wealth of materials in Greek and Turkish on the Cyprus Question. Further, the case studies of the 1964 and 1967 crises over Cyprus provide relevant comparisons to the events that transpired on Cyprus in the summer of 1974. Why were certain legal and political norms operative in 1964 and 1967 but not in 1974? A future revised edition of this book will have to confront these questions in view of the conclusions derived from these case studies. Ehrlich, in his analysis of the 1964 Turkish bombings of Cyprus, fails to note that three days earlier the U.S. had retaliated for the incidents in the Gulf of Tonkin by bombing North Viet Nam. Did this action by the leader of NATO affect the decisions of its small ally over Cyprus? Would the protagonists in these four cases have acted differently, and how, if the law had been totally disregarded? Finally, the author ignores the crucial covert policies beyond the scope of law that have been simultaneously pursued by influential actors such as the United States in order to achieve a politically optimal solution of the conflict. Having served under George W. Ball in 1963–1964, Professor Ehrlich should be quite aware of this issue, given the covert diplomatic initiatives undertaken by George W. Ball and Dean Acheson in that phase of the Cyprus Question.

Stephen G. Xydis's *Cyprus: Reluctant Republic* is the latest and smallest of his works and is a sequel to his earlier book on Cyprus.

There is a certain amount of complementary overlap between his two works, since the latest begins with the period following the consideration of the Cyprus Question by the 12th General Assembly, and concentrates on the resolutionary process rather than the parliamentary diplomacy of the Cyprus Question. Actually some familiarity with Xydis's earlier work provides a better understanding of the 1958–1960 period. As the author shows, the birth of the independent Republic of Cyprus was the outcome of conventional as well as parliamentary diplomacy, the use of violence, and the evolution of the strategic interests, the domestic politics and the goals sought by the protagonists.

Xydis opens his discussion with an analysis of the micropolitical process through which Cyprus gained its independence—a process that he feels provides a better understanding of the proliferation of states in the international system. Cyprus' emergence into statehood was the result of conflict resolution by pre-existing states, even though at the time neither of the two Cypriot communities desired separate statehood. This introductory chapter is replete with other cases of state-building by pre-existing states whether because of or in order to settle or avert unresolved conflicts. The implication therefore is that neither the state-building nor the restrictive constitution of Cyprus were unique in international politics. The rest of the discussion takes the reader through NATO's abortive conciliation effort, the secret Greco-Turkish negotiations, the Zürich-London accords, the writing of the Cypriot constitution, and the granting of independence to Cyprus.

Xydis's latest work is a valuable addition to the literature of the Cyprus Question, but at times it is a tediously minute account of the 1958–1960 diplomatic process. It is mainly a descriptive work that fails to provide any critical commentary on the tactics, goals, and achievements of the Greek negotiators. Nevertheless, the book is especially valuable because of the author's privileged access to the personal archives of Karamanlis and Averoff-Tossizza. The author did not have access, however, to the archives of the other major participants, and especially of Turkey, where the unavailability of official sources is complicated by the deaths of Menderes and Zorlu. Thus Xydis's work reflects more the official Greek perspective of the state-building process, making difficult an evaluation of the objectivity of his analysis. By concentrating on these sources the author also neglects the significance of the views of the Greek opposition, and indirectly shows how Karamanlis's government proceeded alone to liquidate the Cyprus problem. Finally, in terms of secondary sources, the controversial work by Terlexis is not included in the bibliography.

Because of the descriptive nature of Xydis's work, several questions remain unanswered. Certainly, the Cypriot constitutional structure is not unprecedented. But what of its nature and content in contemporary international law and politics? The reader is left with the impression that the introductory chapter under the guise of theory attempts to rationalize the system that was imposed on Cyprus in London. Then there are the tantalizing details of the Zürich negotiations for which no record was ever kept and no official Greek post-conference report ever prepared. There has not yet been any satisfactory explanation of how well Makarios had been informed about the state-building process. The reader is also aware of the American factor, the pressures of the outcome of the 1958 elections on Karamanlis, the Greek commitment to NATO, and the ideological perspectives of the Greek decision makers. But what was the impact of these factors on the negotiating options of Greece? From the background information included in Xydis's work the reader is once more tempted to speculate about the future of Cyprus, given the largely unexplored insights offered into Turkish negotiating behavior, the prejudices of British and American diplomacy in the Cyprus Question, and the presence of Karamanlis, Averoff-Tossizza, et al. in the present Greek government.

Although the massive work by Crouzet provides no new information to the area specialist, it remains an important account of the 1946–1959 period. With the aid of several research assistants the author has overcome the linguistic barrier and has utilized a wealth of sources in English, Greek, and Turkish. Crouzet unduly emphasizes the economic origins of Greek Cypriot nationalism. He shows clearly the impact of AKEL on the internationalization of the Cyprus Question and the crucial 1950 plebiscite. He also shows the influence exerted by the Cypriot nationalist leaders on the evolution of the Greek position on the Cyprus Question. Crouzet meticulously documents the misperceptions of the protagonists toward each other; outlines the domestic and foreign policy dilemmas created for Greece by the Cypriot nationalists; and shows how the present deadlock on Cyprus is the outcome of misperceptions, lost settlement opportunities, and bad judgment on all sides.

The author rejects the suggestion that perfidious Albion stimulated the Turkish claims in the Cyprus Question. He places considerable blame, though, on subsequent British policy and the Greek miscalculation of the Turkish factor for contribution to the intransigence of Turkey in the conflict. Crouzet provides useful insights into the origins and evolution of the partition idea and into the effect that the cold war and strategic considerations of the protagonists had on settlement options. The book is generally well documented and includes an extensive bibliography of both primary and secondary sources on Cyprus, although the important works by Christides on Greco-Turkish relations and by Terlexis are not included in the bibliography. The analysis of the 1958–1959 period, in contrast to Xydis, is weak and often speculative, given the unavailability to Crouzet of the archival sources utilized by Xydis. Yet the author in his concluding chapter provides an important critical summary outlining the evolution of the Cyprus conflict as well as a critique of the independence agreements rather than treating them, as Xydis does, as another example in the micropolitics of state-building. The author does not adequately deal with the role of the Council of Europe in the conflict and tends to rationalize the documented mistreatment of political prisoners by the British in Cyprus. Nor has he utilized available British archival materials in his analysis of the pre–World War II period. Finally, there are some minor errors in dates: the Spaak negotiating initiatives (p. 955) commenced in June, 1957, not in 1956; and the consideration of partition as an alternative by Macmillan (p. 960) dates to the spring of 1957, not the summer of 1956.

The different approaches to the study of the Cyprus Question offered by these books represent the addition of a few more pebbles in the mosaic that when completed will provide a complete picture of the "toil and moil" in the island of Aphrodite.

VAN COUFOUDAKIS

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The Nuremberg Fallacy: War and War Crimes Since World War II. Why the U.N. and the Nuremberg Doctrines Do Not Keep the Peace. By Eugene Davidson. (New York: The Macmillan Company, 1973. Pp. 331. $9.95.)

In this uncommonly well-written study, Eugene Davidson casts a skeptical and rather journalistic eye over the efficacy of the doctrines proclaimed at Nuremberg in respect of war crimes and crimes against humanity. What he sees is hardly reassuring. The past three decades have been riddled with armed strife on virtually all levels except that of world war, and, he points out, all parties to conflicts have without exception (though not necessarily to the same degree) been guilty of violating in one way or another the canons of the laws of war or the international legal principles proscribing the use of force.

But the burden of Mr. Davidson's inquiry is not merely to catalogue the horrors of war, though he does so in vivid, terse prose; rather it is to suggest that the roots of intra- and interstate conflicts and the manner in which they are fought are so immensely complex that they do not lend themselves to policing by the Nuremberg approach. Indeed, had the Nuremberg principles been strictly adhered to during the postwar era, the author believes that they in themselves could have contributed to a world conflagration: "No statesman can possibly act intelligently if he relies on the presumed validity of the findings of Nuremberg, on the concept of one world, on the promise of a universal collective security" (p. 296). The ingredients for preserving peace, for the United States at least, come down in the author's view to ". . . military parity (as a minimum) with the Soviet Union, firmness of resolve in maintaining strategic positions essential to defense, even at high risk, the willingness to compromise in less important matters, the setting of achievable goals" (p. 298).

If practiced by all states, these techniques for preserving peace might well in themselves increase the likelihood of conflict, though the author manifestly would not concur. Be that as it may, Mr. Davidson extracts his maxims from a brief review of the principles and assumptions underlying the postwar international order, a precis of endeavors to define aggression, and an assessment of five major conflicts: the Arab confrontation with the great powers; the Algerian struggle for independence; the Arab-Israeli conflicts; the wars in Indo-China; and Soviet policy in Eastern Europe. The impression that emerges is one of immense diversity: conflicts that arise because of the inability of major powers to enforce their will as in the past; conflicts intensely ideological and emotional, whose fury transcends legal and moral considerations; conflicts engendered by irreconcilable objectives; conflicts waged to preserve imperial hegemony.

And yet despite a considerable flair for incisive description and pithy insights, the study as a whole is most dissatisfying. It is not only that the author seems inclined to dismiss the role of international law in laying down norms of human behavior in warfare which, notwithstanding breaches, are often, perhaps commonly observed by combatants, or the development of rules which offer a reference point for evaluating, criticizing, and sometimes constraining the use of force. It is also the failure to consider the nature of the Second World War itself, whence came the Nuremberg principles. Was that a conflict *sui generis* that produced a body of law inappropriate to the realities of the postwar era, or if the Nuremberg doctrines had existed prior to 1939, would they have been viewed as wholly suited to the situation? Although exasperating at times for these reasons and others, the study nevertheless succeeds as a whole in obliging one to rethink, at a layman's level, the premises underlying the law of armed conflict, and that is no mean accomplishment.

WILLIAM E. BUTLER

University of London

System 37 Viggen: Arms, Technology and the Domestication of Glory. By Ingemar Dörfer. (Oslo: Universitetsforlaget, 1973. Pp. 258. Price unknown.)

To use Dr. Dorfer's own words, *System 37 Viggen* deals "with the processes that go into the practical business of supplying national security, maintaining the technostructure, and fighting bureaucratic battles" (p. 199). In his conclusion he adds that these processes, as far as the procurement of the Viggen military combat aircraft was concerned, ignored both the foreign policy elite and all questions of the international goals of the nation; he also contends that the case study of the Viggen is the best example of the ability of the technostructure to resist the impact of science and technology on high politics. By implication he suggests that Viggen might be the last example, though regrettably, he does not tell us why.

As a case study, for all its attention to detail and tight, compact presentation, the book fails, except only superficially, to balance or integrate what are implicitly two principal objectives: to present a comprehensive case history of the procurement of the Viggen military combat aircraft; and to use it as a means to test general theoretical propositions, imported wholesale from study of the United States, about the "scientific estate" and the "technostructure." The result is a juxtaposition of general propositions on one level and the detailed specifics of the case study on the other. The gap between them is too wide and never successfully bridged. Though the chronological structure of the work provides the basis of some organization, the overall effect is of a lack of direction. The necessity of a concluding chapter suggests that the author himself feels that there is too much implicit and too little explicit in the body of his work.

His study might have been more successful had he, first, concentrated principally on the exceptional material at his disposal, and then formulated theoretical propositions, rather than the reverse. Moreover his theoretical propositions, drawn from the works of Galbraith, Price, Kaysen, and Peck and Scherer, among others, are not only applicable at differing levels of specificity and generality, but are also questionable as the most appropriate for a study of "bureaucratic battles," "supplying national security," or even "maintaining the technostructure." Theoretical studies aimed at a more specific level of analysis, such as those of Lindblom, Cyert and March, Banfield, Wildavsky, or Simon might not only have bridged the gap between theory and data but also have provided more suitable frameworks for the book as a whole.

Without making them always explicit in his text, Dr. Dorfer, who is now serving as a Special Assistant in the Swedish Ministry of Defence, alludes to many theoretical propositions about decision making processes, administrative behavior, and policy formulation. This makes his study, written initially as a thesis under the auspices of the Center for International Affairs at Harvard, all the more frustrating, for the wealth of material which he has used is tailor-made for students of public policy, public administration, and national security decision making. There is no opportunity to illustrate the point here, except to suggest that anyone reading the book might consider doing so with Simon's concept of "satisficing" or Wildavsky's notions of "Community and Policy" in mind; the experience should be rewarding.

One feature of the study which warrants mention is the importance the author attaches to individuals and personalities, particularly when they changed role or function as the Viggen program progressed. This emphasis is a refreshing and more than welcome departure from the recent pattern of studies on public policy and administration in which insufficient significance is attributed to the *sine qua non* of individual and group behavior. To his credit, Dr. Dorfer has clearly brought out the influence a number of individuals had, at particular points in time, not only in determining the size, scope, and direction of the Viggen program but also in arguing against a variety of forces opposing its continuation. Personalities are important; and in a case study such as this it is far from sufficient for theoretical purposes merely to lump them together as part of the "technostructure" or simply to encapsulate them collectively within the closed political world or the "scientific estate."

Unfortunately, Dr. Dorfer has failed to resist the temptation to contrast the Swedish experience in weapons procurement with that of Britain, France, and the United States; as a consequence he stumbles into making a number of elementary mistakes of fact and interpretation and embarking on a number of irrelevancies (for example, it was not the Glibb but the Gibb-Zuckerman Report, p. 65). In one respect, however, a contrast would have been worth developing: this is the *flygmaterielberedningen*, a body unique to Sweden, which started as an *ad hoc* study group to monitor (from an early stage) the progress of the project and developed to one which became institutionalized and made all the major decisions. The development of the group warrants separate study in itself; its significance in the book and for students and practitioners of public policy and administration lies in its role and function as an intermediary body separate from, and wedged between, the traditional institutions associated with national security and defense procurement. The success of the *flygmaterielberedningen* was to drive the project toward completion by dint of its composition, expertise, separateness and ability, sometimes aggressively, to operate outside the traditionally accepted rules. The scope for individual initiative and determination amid the treacle of bureaucratic procedure is evidently not totally lost. Nevertheless, somewhat puzzlingly Dr. Dorfer asserts that there is little the rest of the technically advanced world can learn from Swedish experience with the Viggen (p. 198).

This is an important book, more for what it represents and reveals about a major technological program than for what it attempts by way of underpinning theoretical propositions about advanced technological states and the closed nature of "high politics." What the book well represents, as its author rightly claims, is the value of open research and the benefits that can be derived from it. For this reason, governments and bureaucrats elsewhere might well accept Dr. Dorfer's study as an exemplary precedent and follow Sweden's enlightened initiative.

MARTIN EDMONDS

University of Lancaster, England

The Capabilities of Nation States: International Conflict and War. By Wayne H. Ferris. (Lexington, Mass.: Lexington Books, 1973. Pp. 191. $12.50.)

Is there some relationship between the power capabilities of states and their propensity for involvement in war? If so, are disparities in the power capabilities of two states relevant to the probability of their becoming involved in military hostilities with each other? Are changes in the capacity of states to wage war related to the incidence of war? These are some of the basic questions to which Dr. Wayne H. Ferris sought empirical answers while he was associated with the Foreign Policy Research Institute at the University of Pennsylvania. In his elaborate quantitative analysis of armed conflicts from 1850–1965, he particularly hoped to shed light on two questions about which earlier scholars had differed: the causation of wars and whether preponderance of power or equilibrium was more conducive to their outbreak.

To conduct his rigorously reasoned study the author felt obliged to fill in gaps in existing data relating to the power capabilities of the whole world of recognized sovereign states. Crucial among the careful definitions of the concepts he employed, "power capabilities" was used to mean "those factors that enable one entity to threaten sanctions vis-à-vis another" (p. 6). He focused on six factors, later expanded to eight, which seemed important in measuring power capabilities and on which data were available or could be generated: armed forces personnel, defense expenditures, population (reduced to logarithms of country totals), government revenue, trade value, area, government revenue per capita, and defense expenditures per capita. These were then combined into a single index for each state. Just as he derived his definitions from previous analytical works by recognized scholars, he utilized several other classified lists of armed conflicts, most particularly those produced by Quincy Wright, Kal Holsti, and J. David Singer and Melvin Small.

With the data so gathered or combined, he constructed a table of the ten highest- and ten lowest-ranking states for selected years and for different time intervals, which he found generally to accord with historical knowledge. Through several operations he then constructed tables of rates of change for different time intervals to show trends over time. After isolating 41 wars from earlier lists he tested fourteen hypotheses, which fell into sets. One set centered on relationships between power capabilities and war outcome, i.e., the victor, duration, and amount of devastation, measured in terms of battle-connected fatalities. Another set focused on disparities and changes in disparities. Four hypotheses dealt with conflicts escalating to the level of military hostilities. Others concentrated on the occurrence of international conflict in general and its relationship to disparities in power capabilities.

Most of the hypotheses turned out to be

unsupported by the operations conducted, or were only supported when there were further refinements in the operations which produced results apparently closer to the assumptions. Of the latter outcomes, if one state had twice the power capabilities of the other, the hypothesis that "most wars are wars in which a high level of disparity characterizes the power capabilities relationship prevailing between the belligerents at the initiation of hostilities" was unsupported, but if the level of disparity was set at 1.45 times (rather than 2.00) it was confirmed. Confirmed also was the hypothesis that "given the occurrence of war, the side possessing the greater power capabilities at the time of the initiation of hostilities will be victorious more often than would be expected by chance" (p. 115). Yet Ferris's research left unplumbed the intriguing question of why great powers so often desist from allowing war to occur when dealing with a stubborn small power. The author did find some correlation between widening disparity in power capabilities and occurrence of intense conflict which also appeared to increase as the change between a state *and others with which it interacted* increased.

Both the findings and the policy implications were modestly and carefully presented as tentative. The ingenuity in the manipulation of available data is impressive. Nevertheless, the nonquantitative scholar may continue to have misgivings. Dr. Ferris refers to some of the unanswered questions in his suggestions for future quantitative research, but the difficult ones, including those relating to the rationality of decision makers and to perception and motivations would seem to call for further historical and psychological case studies to "test" his findings rather than for more sophisticated statistical techniques and operations. He admits at the start that he has left out "the more abstruse indicators" because of the impossibility of gathering such data for all the countries for all the 116 years he has studied, but key periods and actors might be singled out. He also deliberately omitted from his "pooled list" of 41 wars those which were civil wars, revolutionary wars, and wars between sovereign and dependent states. Having recently lived through the Vietnam War one wonders how to classify that conflict, which raises the difficulty continually nagging in the non-quantitative researcher; proper classification. This study, though multi-dimensional, does not tell us the extent to which power potential is shaped so as to promote particular objectives and what determines the amount of that power that is to be invested in a particular confrontation. Is differential willingness to sacrifice for one policy objective rather than another to be excluded altogether because it cannot be combined in one grand capability score? How is alliance potential to be weighed in the relationship?

Despite such causes for doubt about the assumptions, methods, and findings, the reader should find this book stimulating as well as provocative.

ANNETTE BAKER FOX

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British Foreign Policy, 1945–1973. By Joseph Frankel. (New York: Oxford University Press, 1975. Pp. 356. $22.50.)

Of all the profound alterations in the structure of the postwar international order, the rise of the Soviet Union to the status of a global power; the recovery of Japan and Germany; the emergence of OPEC or the group of 77 at the UN, few phenomena have been as striking as the change in Britain's fortunes. In 1939 she was second only to the United States in GNP per head and was the stronger military power of the two. Though profoundly weakened and impoverished by the Second World War, in which she was the only power on either side to be a belligerent from the first day to the last or for nearly six years, she was still the center in 1947 of a Commonwealth and Empire that embraced five hundred million people in five continents. Thirty years later she is simply one of a group of European middle powers and by no means the strongest, either in terms of economic and military strength or even of domestic coherence.

There are two standpoints from which one can regard this process of transformation and change in British perspectives that have accompanied it. On the one hand, one can argue, as many American analysts have done, that Britain has steadily lost power and influence as a consequence of official complacency, bad planning, and the prevalence of delusive myths on the part of her elite about her "special relationship" with the United States, the vitality of the Commonwealth or her qualifications for a continuing world role. On the other, one can argue that Whitehall saw the inexorable change in the structure of relative power as early as the beginning of World War Two, and that the subsequent years have witnessed—apart from aberrations like Suez—a gradual and deliberate transference of responsibility to others, without trying to sustain an untenable position as France did in Indochina, and a skillful adjustment to a world order in which

Britain could no longer play a dominant role.

The great merit of Professor Frankel's sober and informed study is that he adopts neither the accusative nor the congratulatory attitude but a judicious mixture of the two, as the evidence warrants.

The first half of the book is concerned with the modern British political system and its effect on perceptions of British interests; with the confidence that a long tradition of successful political leadership of a homogeneous society signified 'that Britain was endowed with unique powers of adaptability' (p. 67); and with the subsequent delusion that her understanding of world politics could keep her in the forefront of the nations, or that the international role of sterling would sustain her world role, despite the growing limitations on the exercise of British power outside Europe. There is a particularly good chapter "The National Style" which emphasizes and explains the pragmatism of British decision making, how this served the country well in the immediate postwar years and badly when the problem of relations with the EEC had to be confronted. It deals also with the equivocal nature of morality in the making of foreign policy.

> Britain's international reputation for hypocrisy, epitomized by the phrase "perfidious Albion," may be attributed not to national insincerity but rather to a national distaste for logic and planning. As the British prefer to deal with situations after they have arisen rather than to prepare for them, they show a pronounced tendency to violent fluctuations between idealism and realism, between humanitarianism and self-interest (p. 140).

Professor Frankel also rightly emphasizes the extent to which British policy has not simply been a reflection of American: he traces, for instance, not only the familiar arguments over the recognition of China in the 1950s or in dealings with the Arab world, but also the extent to which Anthony Eden and the Foreign Office in the 1950s had a coherent theory of detente with the Soviet Union at a time when Washington could envisage no alternative between indefinite confrontation and Soviet accession to American views and principles.

The second part of the book deals with the history of British foreign policy since World War Two. It naturally includes particular concentration on British relations with the superpowers and also on her other major relationships, Western Europe and the Commonwealth. The section on the latter is particularly worth reading for Americans, few of whom have attempted to understand what the modern conception of Commonwealth was and indeed still is.

The particular quality of Professor Frankel's study is that it adheres closely to the facts, using examples from earlier as well as contemporary British diplomatic history to validate his points. But at the same time he deals also with more intangible questions such as the loss of British will power in asserting her influence or interests, and thereby illuminates this process.

ALASTAIR BUCHAN

Late of Oxford University

Socialism and the Great War: The Collapse of the Second International. By Georges Haupt. (London: Oxford University Press, 1972. Pp. 270. $17.00.)

The author of this book is probably the most knowledgeable historian of the Second International in the Western world, and this reshaped and enlarged English version of his previous volumes on the same subject in French and German can only enhance his reputation. Students of general European pre-1914 political and social history will find in this compact book all the relevant data concerning the manifold socialist theories and policies devised first to understand and interpret the phenomenon of war and then (unsuccessfully) to oppose it. Professor Haupt, a French scholar of Romanian origin, was fortunate enough to become closely associated with the late secretary of the Second International, Camille Huysmans, who opened to him the heretofore inaccessible archives of the International Socialist Bureau (ISB). New documents surfaced as a result, especially the printed but never circulated materials prepared for the Tenth Congress of the International, which should have convened in Vienna in August 1914 but was never held because of the outbreak of the war. The blending of this material with a rich assortment of primary and secondary sources—some well known, others quite new—permitted the author to approach his subject with unparalleled documentary richness. A particularly successful aspect of the book is the use of footnotes whose selection and illustrative vividness are simply admirable.

From the abundance of documentary evidence there emerges a devastating picture of the intrinsic weaknesses of the Second International. With its "reformist practice screened behind verbal radicalism" (p. 221), with the national socialist parties at odds with each other while at the same time lacking internal coherence, with its most prominent theoreticians expounding in the name of the same doctrine mutually exclusive interpretations (for example on the issue of imperialism, pp. 147–60), with the widely fluctuating mood of the workers' masses—the socialist "sub-culture" was too deeply integrated into the "global European society" to be able to tame or thwart its most dynamic and destructive forces and impulses. A particularly interesting and paradoxical aspect of the International's failure was a growing confidence of leading socialists during the first half of 1914 that Europe was entering upon an era of lasting detente. This confidence was based on their belief that pacifist tendencies were increasing within modern capitalism—a belief that replaced the earlier view of capitalism as a conflict-breeding economic system (pp. 107, 123, 149–50).

The portrait of the International's failures is indeed so vivid that upon reading Mr. Haupt's study one is still at a loss to comprehend fully the deeper causes and meaning of the débâcle. The excellence of the documentary and descriptive sides of this work overshadow its analytical and interpretative aspects. One may accept the author's claim that he has covered what actually happened, but the chronicling of what transpired on the European diplomatic and socialist scenes during the years immediately preceding the First World War is much superior to his attempts to shed new light on socialist behavior before the guns of August. In this sense, the author's explanations and hypotheses do not go beyond theories and affirmations of other scholars who had tackled the same problem. Quantitatively, Professor Haupt knows more than they; qualitatively, he does not add to the sum of other writings he has carefully expounded. Fairly enough, in the concluding chapter, which is occasionally lacking in clarity, he points out the direction in which further research should be pursued.

More specifically, on the critical side, while the author rightly charges that the Second International lacked a coherent theory of war, he neglects to discuss the contribution of the original Marxian thought to the ambiguities, vacillations, and indecisions of post-Marxian Marxists. One is also tempted to question the author's judgment about the intrinsic political qualities of some of the Second International's leaders. For example, Mr. Haupt's appraisal of Jean Jaurès as "statesman" of the Second International (p. 14) not only seems questionable when confronted with Karl Kautsky's opinion (much closer to the mark) that Jaurès was a "rhetorical genius" but is contradicted by Haupt's own description of Jaurès's zigzagging and politicking within his own party in July 1914 (pp. 171–180). The book is also marred by some confusing passages. Mr. Haupt does not clearly summarize and explain, for example, the description of the Balkan situation given by Dušan Popović, the secretary of the Serbian Socialist Party, in a confidential note sent to the ISB on August 1, 1912 (pp. 72–73). Likewise, the reader wonders which of the prominent Socialist leaders had prepared reports on imperialism for the aborted Tenth Congress of the International: at one place, four names are given: Jaurès, Haase, Vliegen, and Hardie (pp. 139–40); and at another only one: Otto Bauer (p. 156).

With these reservations in mind, one cannot but be impressed by the performance of an industrious scholar who has plowed deeper than his predecessors into the raw materials of socialism during a momentous era of world history.

MILORAD M. DRACHKOVITCH

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The Security Council: A Study in Adolescence. By Richard Hiscocks. (New York: The Free Press, 1973. Pp. 371. $8.95.)

A book entitled *The Security Council* will stir different expectations in different readers. One may wish, as I did, to follow the interplay of Great Powers (the Council's permanent members) and lesser or bit-part actors within an international arena that exposes their changing relations. Another reader may be following a hunch that the Council must exhibit some process suitable for investigation by the social scientist: What is the pattern of its decision making? Of its institutionalization? Yet another may long to tut-tut a global thingumajig that has failed to respond properly to international crises. Of course, the reader may simply hope to learn a little more about this principal UN organ, its participants, and their tribulations and modest achievements since the last big war.

Richard Hiscocks's study rewards the latter expectations; it does not help much to satisfy the former ones. Other recent studies have dealt with the history of thinking about how to prevent war or establish peace; with Security Council votes; with the role of personalities in a very select club; and with the dynamics of negotiation behind the screens, or off-camera. *The Security Council* manages to touch on all of these matters. At somewhat greater length the book depicts the Council as "a forum and a rendezvous," reviews some problems of peacekeeping, describes relations between the Council and the General Assembly, and peers cautiously ahead. A central chapter occupying more than one-third of the volume recollects cases of conflict brought before the Council and those that somehow stayed beyond the reach of its members. This chapter summarizes the Council's spasmodic operations over a human generation. Hiscocks's discussion is judicious and his coverage remarkably full. Here we have a competent work.

The book neither applies nor works out an illuminating thesis. It does not offer an explanation of Council ineffectiveness much beyond that implied by the subtitle. Some observers may see a satisfactory interpretation of Council impotence today in Hiscocks's treatment of certain Council procedures that were acquiesced in at the outset by the UN's founding fathers. To reach this conclusion would be to misconstrue why the Security Council works as it does. Moreover, it would misread Hiscocks. For although the author gives extended treatment to voting strictures and other Council rules, he punctuates his study with judgment calls upon the Council's participants. Thus the reader is invited to ponder whether the adolescence to which Hiscocks alludes refers more accurately to a stage in the life of a political institution, or a stage in the lives of Council members who have not yet learned that they must act in concert or continue to act ineffectually.

If *The Security Council*'s shrewdest insights are borrowed from other studies, the author at least chooses wisely which ones to embrace. And he has, by way of an admirable index and a splendid bibliography, provided any reader with means to use this volume as a reference work and to pursue special questions elsewhere.

JAMES P. SEWELL

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Prospects for Partnership: Industrialization and Trade Policies in the 1970s. Edited by Helen Hughes. (Baltimore and London: The Johns Hopkins University Press, 1973. Pp. xx, 289. $10.00, cloth; $3.50, paper.)

Collections of conference papers frequently fail to provide much sense of the confrontation of issues and the interchange of ideas which took place at the meeting. Unless they are substantially revised afterwards, the conference papers usually indicate only the starting points of the discussions—a list of characters without a script—leaving the reader to wonder how it all worked out. This volume of papers from a seminar on industrialization and trade policy which was sponsored by the World Bank in October 1972 suffers from these defects. In addition, the papers for the most part are quite brief—essays rather than extensive scholarly explorations—and the introductory and concluding chapters do not give the reader a very adequate summary of the issues which were actually discussed at the seminar or of the conclusions reached.

The seminar focused on changing patterns of trade in industrial goods between the developed and developing nations. It looked in particular at two dimensions of this trade: at the role of demand in the developed countries for the manufactured product exports of the developing nations, and at the adjustment problems in the developed nations to which an increasing volume of manufactured product imports from the developing nations would give rise. The participants at the seminar were less concerned with the ability of the developing nations to increase the supply of manufactured products for export or to expand the range and improve the quality of these goods than with the possibility—likelihood—that the developed nations would impose limits on their imports of manufactured goods from the developing nations. The volume includes papers on the role of multinational corporations, on problems of employment in the developed nations which might result from or be intensified by an increase of manufactured imports, and on the different approaches to adjustments to expanding imports of these goods that have been undertaken in Japan, the E.E.C., and the United States. Also included in the volume are several comments on each paper, some of which are particularly useful.

Although the issues involved are extremely interesting and important to students of development, international political economy, and the economic policies of advanced industrial nations, and the papers, while brief, are generally well constructed and illustrate the views of leading economists in universities and international financial institutions, the volume itself is a disappointment, even for a collection of conference papers. The papers, largely because they are meant to be initial statements of position, are developed at very high levels of generality. They deal by and large with implicit and rather simple economic models of international trade, and say little about the forces in the real world which influence the making of trade and economic policies. The papers suffer particularly from the authors' tendency to exclude political considerations. Yet the central issues of the seminar—how governments in the developed world will respond in the making of domestic macro- and micro-economic policies to an increasing flow of manufactured goods from the developing world—are much more likely to be influenced by political than by purely economic factors. But, aside from the blunt comment by Nat Goldfinger of the

AFL-CIO, politics is largely ignored. Although there are some more recent signs of change (for example, on work on prices and incomes policy), it remains remarkable how economists continue to feel that they alone have something useful to say on matters such as these.

A second major reason for dissatisfaction with this volume is in no way the fault of the organizers of the seminar or the editor of the book. It is simply that enormous changes have taken place in the world trade situation since the seminar took place. In particular, very great increases in the prices of many primary commodities and the worsened economic situation in the developed world have substantially influenced patterns of trade between the developed and developing nations. Given these changes, the basic assumptions made by almost all of the contributors—that the primary emphasis in international trade by the developing nations will (and should) be to expand and diversify manufactured exports to the developed nations and that an increasing volume of such exports can be integrated into the economies of the developed nations without giving rise to serious economic dislocation—must, at least for the foreseeable future, be re-examined.

Finally, although the book is a useful introduction to some of the major issues at stake in a vital policy area (despite the reservations I have noted), it would have been much improved by the inclusion of a short bibliographic essay.

STEPHEN BLANK

The Conference Board

The Bases of International Order: Essays in Honor of C. A. W. Manning. Edited by Alan James. (New York: Oxford University Press, 1973. Pp. 218. $11.25.)

Many *festschrifts* display more reverence than rigor, but this one turns out better than most, not only because of the long and close association among the authors but because of their concentration on the unifying theme expressed in the title: "the bases of international order." The man to whom the book is dedicated, Charles A. W. Manning, was Professor of International Relations at the London School of Economics from 1930 to 1962. He is honored not only for his substantive contributions to international studies but for his influential teaching and institutional innovation in prodding his own university and others into giving more adequate attention to international relations.

The eight authors, mostly former students and colleagues of Professor Manning, chose their theme because the problem of developing more orderly and peaceful relations among nations was a central concern of Manning and because it is a major continuing issue in the study of international affairs. The eight essays were conceived of not only as a gloss on Manning's work but as an independent contribution in their own right.

The essence of Manning's thought which emerges from the volume is a strong interest in the many aspects of international life which are pushing states toward more pacific and organized management of their relations. While Manning gave much emphasis to law, he was a pioneer in his broadly interdisciplinary approach. Although he did not develop a coherent "systems" methodology, his thinking was harmonious with that concept. He was not a salesman of any ambitious blueprints. Rather he emphasized trends which he thought would require greater awareness and understanding of the growing interlacing of national societies. In thinking about how to deal with these trends, he was realistic, pragmatic, constructive, and evolutionary.

While the authors are friendly toward the man they honor, there is some criticism, both in the lines and between them. Yet many of the problems mentioned are endemic in the formidable subject matter Manning was wrestling with and are shared by other scholars who have tackled the same adversary. Probably the greatest difficulty that Manning faced was his effort to strain the ocean of international complexity through the sieve of his inadequate methodology. His principal findings, summarized mainly in his book, *The Nature of International Society* (Bell, 1962), lacked something in clarity and sophistication. He was right, however, in stressing the need for macro as well as micro approaches, and he pointed the way for others to explore further.

Did his interest in order incline him to see more than there was, or could be? Did he see trends which he was all too eager to extrapolate into deterministic portents of future progress? In his defense, it can be said that the science of measuring and predicting such progress is still in its infancy. If he erred on the side of optimism, he did so with far more careful, comprehensive, and realistic study than many others. And he stood with the realists in warning the prophets of international integration that reports of the demise of the nation-state were premature. This was a useful antidote for excessive wishful thinking, but, in the process, Manning tended to underplay the role of nongovernmental actors and relations which have become an increasingly important ingredient in what are currently called transnational relations.

While this is the general profile of Manning's thought which emerges from the volume, each chapter focuses on some special feature through the eyes of its author. In the first essay, F. S. Northredge, Professor of International Relations at the London School of Economics, reminds us that the quest for order cannot, and should not, halt change. Yet he concludes on a rather conservative and fatalistic note:

> The most that men seem able to do is, not to change the world or to resist the tide of change which originates outside the will of any determinate person or group, but to ensure that change takes place with the slightest possible risk to the contingent order of the international society. In the final analysis it may be order, not change, that we should concern ourselves with. Change will look after itself (pp. 22–23).

The successive waves of new states since the late eighteenth century and major aspects of their international involvement—military, diplomatic, legal, economic, and moral—are the subjects of the second essay. The author, Peter Lyon, Secretary and Senior Lecturer at the Institute of Commonwealth Studies, University of London, provides a comprehensive discussion of the subject. But surely he underestimates the economic role of the less developed countries when he says that "... it has, until recently, been negligible, and even now their impact is marginal and mostly verbal" (p. 56). He also tends to neglect the intellectual (including ideological) and political aspects of the impact of the developing countries on the world scene.

The editor of the volume, Alan James, Reader in International Relations at LSE, contributes a thoughtful essay on international law, but he claims too much for law when he says, "... international society, like domestic, relies primarily on law for its framework of ideas about orthodox behaviour" (p. 67). Again, "... if it is sought to provide long-term protection for human rights, the obvious way forward is through the creation of legal obligations" (p. 72). In contrast to law, the balance-of-power process is emphasized by Martin Wight, who was Professor of History at the University of Sussex until he died in July 1972. He saw the power-balancing tendency, with all its limitations, as the most effective means of maintaining peace: ". . . the balance of power lasts only so long as someone is ready to take risks to maintain it, and that international order will in the end be brought about only by those who are prepared to make sacrifices to construct and enforce it" (p. 115). He is on weak ground, however, when he argues that "the failure first of the League of Nations, and then of the permanent members of the Security Council to achieve unanimity, disabused men of the idea of international order as a work of political construction" (p. 113). The peace maintenance process is not an automatic "invisible hand"; it is quite dependent on wise and skillful "political construction," whether through multilateral or other channels.

The chapter on "war" by Hedley Bull, Professor of International Relations, Australian National University, reminds us that, although nuclear weapons have greatly restricted the use of violence as an instrument of policy, military might—especially conventional weapons—is still a major factor in international relations. Oddly enough, however, the author gives no significant attention to the quest for international arms regulation. In a chapter on "morality," Geoffrey Stern, Lecturer in International Relations at LSE, observes that concepts of morality are so varied and parochially interpreted that they offer little firm foundation for international order. His value prescriptions for the future are tolerance and more equitable economic and social relations, but he doesn't explain how these humane principles are to be injected into the policies of nations.

The chapter by Geoffrey Goodwin, Professor of International Relations at LSE, is a carefully balanced analysis of international organizations, eschewing extreme optimism or pessimism. It is unfortunate, however, that at the end he finds it disturbing that "some rationalists have joined hands with military revolutionists in putting 'justice' before either peace or order" (p. 186). Surely the lack of reasonable justice is a major cause of disorder, and greater justice would contribute to greater order. In the last chapter, Michael Banks, Lecturer in International Relations at LSE, summarizes Manning's principal concepts: world society as a coherent system because of certain basic deterministic trends, creating increasing interdependence and contact which reinforce order and "myths," thereby facilitating international intercourse.

My final impression of this volume and the man it honors is that they are admirable in attempting a broad interdisciplinary approach to understand the extraordinary complexities of international relations but that they are regrettably conservative in their emphasis on order above the quest for social justice.

H. FIELD HAVILAND

Tufts University

The United Nations and Economic Sanctions Against Rhodesia. By Leonard Kapungu. (Lexington, Mass.: Lexington Books, 1973. Pp. 155. $12.50.)

This study has several real strengths. Dr.

Kapungu writes dispassionately and with sober and balanced judgment, and his work is based on careful research. The result is a valuable addition to the already extensive literature on the various dimensions of the struggle of the large African majority of Zimbabwe to establish control of its political destiny.

The most interesting and original sections of Dr. Kapungu's study are the sections on the tardy build-up of international sanctions after the white minority regime had unilaterally declared its independence of Britain, his material on sanction-breaking, and his discussion of the machinery which has developed to administer and enforce the sanctions. His conclusions on how U.N. sanctions might be made effective are applicable beyond merely the particular case he is studying. Dr. Kapungu makes the perhaps obvious and basic point that if sanctions are to succeed, there must be very widespread agreement on their purpose and their introduction amongst the states which must enforce them. In addition, he argues that sanctions will be more likely to succeed if they are accompanied by strong support for the sanctions within the state concerned. He suggests that sanctions should be rapidly introduced once they have been decided upon, that they should be comprehensive and mandatory, and that they be enforced by an international agency with the means and the authority to identify sanction-breaking quickly. Finally, he argues that publicity must be given as early as possible to all suspected cases of illegal trading in defiance of the sanctions.

Dr. Kapungu would agree, I believe, that if it was known that these criteria would characterize any effort to introduce U.N. sanctions, his first and basic condition of their widespread acceptance would be much harder to achieve. However, he would, I think, argue that when approval for sanctions can be obtained only by omitting essential enforcement features, they are found to fail and ought not to be introduced.

I suspect that the more original sections of this short volume could have been presented in a substantial article. Instead they are accompanied by less original and less necessary discussions of the British position and the stand taken at the United Nations on the sanctions by the major powers and the main groups of nations, as well as by a rather labored discussion of the types of sanctions available to the United Nations.

Dr. Kapungu's brief discussion of the international political aspects of the introduction of sanctions, makes the interesting point that African states never felt it important that the sanctions be vigorously enforced. These states viewed the sanctions as an effort by Britain to avoid responsibility for that more direct intervention which these states were pressing upon Britain. No doubt Portuguese and South African opposition to the sanctions would in any case have caused their failure. The fact that the African states did not advocate them vigorously, however, made it easier than it need have been for such other states as Japan and Germany to ignore the sanction-breaking engaged in by some of their nationals.

CRANFORD PRATT

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Mao & China: From Revolution to Revolution. By Stanley Karnow with an introduction by John K. Fairbank. (New York: The Viking Press, 1972. Pp. 592. $15.00.)

This is one of the most thorough, detailed and vivid accounts of the Chinese Great Proletarian Cultural Revolution. It combines the readable style of Stanley Karnow the journalist and experienced China Watcher with the careful analysis of the less well known, but complementary, Stanley Karnow the academic researcher. The book excels particularly in its graphic descriptions of blow by blow accounts of the development of the Cultural Revolution from a "China Watching" point of view, which has been tempered by reflection. The author takes us through the main stages of the Revolution from the vantage point as seen by him of the chief protagonists in Peking. At the same time the author is careful to show the emergence of different interests as well as the conflicts between the cliques and factions. This is not therefore simply an exercise in the analysis of conflict for power.

While one cannot but admire Karnow's style and enterprise, in my view his book is badly flawed in its central theme and in the assumptions underlying his approach to the subject. The main theme is of a Mao obsessed by revolutionary values who continually and disastrously intervenes in the otherwise smooth process of a rationally and bureaucratically modernizing state. "This is a book about a man in collision with a country. This is Mao Tse-tung. The country is China." Thus Karnow begins his lengthy study and that is how he proceeds. Mao is seen as a great heroic and romantic figure whose interventions in the economy and polity are seen as basically harmful in their effects and as ultimately foredoomed to failure.

The author's forthright attitudes regarding Mao are spelt out early on: "Pervading Mao's concepts and conduct, then, was his reluctance to concede to the reality that modern states, which are ideally guided by lofty philosophies, actually operate on the basis of administrative routine and technical specialization rather than through exhortations, emulation movements and mass enthusiasm" (p. 5). A little later he elaborates further, "Mao focussed on transforming the Chinese 'soul' as prerequisite to modernization. Hence he aligned himself less with Marx than with the romantic utopians who had interpreted history as a moral drama in which virtue rather than material progress was to be promoted" (p. 7).

This view of Mao is at best a partial and distorted one which overemphasizes one particular aspect of his many sided personality and political philosophy. At the same time Karnow claims to know that the operations of modern states are based upon rational routinized bureaucratic lines—a claim that even the most ardent behaviorist could not accept without severe qualification. Indeed such a concept of the modern state can only apply to idealized Weberian models rather than to actual ongoing states with their peculiar mixtures of the "traditional" and the "modern". Mao's writings, speeches, conversations and commentaries which became available in unofficial publications during the Cultural Revolution with their careful and detailed critique of the Soviet economic model and their idiosyncratic but profound comments on Chinese politics and economics (including remarkable self-criticisms) should all have cautioned Karnow against drawing so facile a picture of Mao—however poetically and dramatically conceived. At the same time reflection upon the convulsions and dysfunctions of the American state particularly with the failure of leadership and the bureaucratized warfare in Indo-China should have made Karnow a little more modest and it should have cautioned him against the easy presumption of understanding the operations of modern states. Would he have Mao follow the Soviet pattern? Is that such a successful economic and political system so manifestly superior to that of China?

These are vital considerations which pervade the book. For example, in Part I Karnow sets the background to the Cultural Revolution in a historical survey of the People's Republic from 1949 to 1965. Throughout Karnow's view of Mao is imposed upon the reader. The analysis of the Great Leap Forward of 1958 is typical. Under the heading of "The Leap That Failed," it is described as one mad disaster after another. There is little or no analysis of the economic changes and indeed the changed domestic and international situation leading up to the Leap. Mao is thoroughly blamed for the whole enterprise. The fact that the other Party leaders went along with the new scheme is evidently an embarrassment for the Karnow thesis and he explains that away by first suggesting that they had no alternative solution and second by suggesting that they sought to enhance their power: "the Party machine may also have come to believe that it could fortify itself through the Great Leap Forward by exercising control over the 'people's communes' " (p. 95). At all times we are presented with a picture of Mao at odds with his colleagues and in battle with the Chinese people to change their nature.

Briefly, Karnow's view of the Cultural Revolution is of Mao unleashing a children's crusade of the Red Guards and activating Chinese radicals for the tactical purpose of exposing and undermining his adversaries in the leadership and moreover for the strategic reason of changing China's political culture. But the radical forces thus released soon surged out of control; fissions in Chinese society opened up and order had to be restored through the largely conservative armed forces. This in turn led to the struggle to diminish the exalted role of the military and once again to subordinate them to civilian control. The whole account vividly brings to life the passions and the drama of the time. It has bite and pace. It is also thoroughly documented. The verve and panache of the writing occasionally leads to hyperbole but that is a small price to pay. Karnow leaves us finally, with a vision of a Mao who had led his people to rediscover their "grandeur" and who had inculcated in them "a spirit of national identity, a sense of purpose and a dedication to self reliance." We are also left, however, with a view of Mao in whose very success "lay the seeds of his chief failing. Unwilling to adjust to the prose of stable administration, he sought to sustain the poetry of revolution" (p. 516).

Karnow's view of the Cultural Revolution is one held by many, but it is a "China-Watching" point of view. The large changes in Chinese political culture may yet show that it was Mao's understanding of the workings of a modern state and now Karnow's which has been the more solidly based in reality. There are in fact other approaches which yield different results and perspectives as is indeed implied in Professor Fairbank's introduction. But as Fairbank also rightly points out it is unlikely that anyone will write of the momentous events of the Cultural Revolution with greater vitality.

MICHAEL YAHUDA

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INTELSAT: Policy-Maker's Dilemma. By Judith Tegger Kildow. (Lexington, Mass.: D.C. Heath, 1973. Pp. 118. $12.50.)

This book examines one case in the ongoing process of American foreign policy making. Its principal focus is on the development of the International Telecommunications Satellite System (INTELSAT) but the real value of the book is in what it tells us about U.S. foreign policy toward Europe and about the problems of combining U.S. commercial and political interests in policy decisions.

The author holds that the making of INTELSAT policy reflected an uneasy marriage of private business and public interests. The impact of the commercial interests apparently prevailed in the early stages of developing the satellite system and furnished the foundation and rationale for U.S. policy in this area. This policy came into conflict with the European approach of government ownership for telecommunication enterprises. Additionally, the author says that much U.S. policy was oriented to protect what economists like to call the patent—technology and skills associated with the creating and sustaining of the industry in question. Europeans also objected to American INTELSAT policy because they too wanted to generate the products of telecommunications technology and because they came to believe that American commercial domination in the construction and use of INTELSAT amounted to American political domination as well.

The description of U.S.-European conflict over the development of INTELSAT provides a very useful case study in the problems of U.S. policy making from a foreign point of view. Professor Kildow does not write from this point of view but she wisely gives the reader valuable insight on the European perspective. While not surprising to the specialist in U.S.-European affairs, the information contained in this book, along with the sense of perspective conveyed, would be instructive for the informed laymen. Clearly, American communications satellite policy angered many influential Europeans and probably sowed seeds of future mistrust among allies. The documentation demonstrating the dominance of U.S. industrial interests and philosophy over certain aspects of American foreign policy is, likewise, very instructive. In this latter area the author clearly demonstrates how the American Communications Satellite company (COMSAT) required an "other end of the line" to justify its growing operations and how COMSAT thus sought to dominate the development of INTELSAT on the basis of its self-defined corporate needs.

The book tells an intriguing tale, but the information is revealed in a round-about fashion, and organization within chapters is rough. A more serious problem for those who would apply policy science to foreign affairs is the lack of some model or intellectual framework into which this valuable information can be placed. This is particularly important because there are some pressing questions to be answered.

This is a time when students and policy makers need to know more about the relationship between private money and public government in international policy making Multinational corporations have been identified as private decision makers affecting public policy, and much is being learned about their impact on international politics. The dimension of the public/private corporation should be an important addition to this knowledge. Many questions come to mind about the ongoing relationship of the United States and its European allies who got "the business" from COMSAT. One wonders whether or how this affects American efforts to obtain consensus on common energy policies, IMF agreements, trade arrangements and currency adjustments. Such questions become more important as the strategic reasons for Atlantic unity diminish and the economic demands of the poor nations mount.

Professor Kildow cannot be expected to answer such questions; only to discuss the particular issue so that comparison and application to other areas of policy making can be made more smoothly. This is a responsibility more of us should undertake. No grand theory is required here; just some assistance so others can make better use of the information.

DONALD E. MILSTEN

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World Poverty and Development: A Survey of American Opinion. By Paul A. Laudicina. (Washington, D.C.: Overseas Development Council, 1973. Pp. 126. $21.30, paper.)

Considerable discussion has taken place in recent years concerning the subjective character of social inquiry. Regardless of our attempts to generate uncontaminated knowledge, ideology and inquiry seem inevitably linked. Investigators have thus been challenged to elucidate their biases at the outset, allowing the audience to judge the validity of their arguments or observations in light of this knowledge. Laudicina's analysis of American opinion on world poverty is salutory in this respect. The author himself is an Associate Fellow of the Overseas Development Council, and the research on which the monograph is based was largely conceived by the United States Coalition for Development. Vested interests in the reduction of world poverty and in the contribution of the United States to this end are clearly manifest. The nationwide survey providing the data base for the present analysis was designed to assess the public's reaction to poverty and its feelings about our nation's involvement in poverty relief. Further, in the hopes that future policy makers might utilize the findings to generate support for the aid programs, special inquiry was made into what segments of the populace were especially sympathetic to world poverty problems, how much information people had available on the relevant issues, and what media might be used to influence future opinion.

From the author's perspective, the findings provide strong popular grounds for reversing contemporary isolationist trends. As Laudicina maintains, public support for assistance programs to underdeveloped countries is at an historic high of 68 per cent. Furthermore, one of every three Americans is basically sympathetic to the concerns of poor countries, while only one in four is negatively disposed toward their needs. Public support is especially strong for programs alleviating problems of hunger, malnutrition, disease and illiteracy. The major source of this desire to aid poor nations appears to be the prevailing moral sentiments. The "cold war" rationale for helping the needy appears to have lost its appeal. Consistent with such humanitarian interests, the majority of Americans favor cutting the foreign military assistance budget. Strongest support for developmental aid is found in the younger, better educated, wealthier and more politically liberal segments of the population. A future emphasis on education seems particularly fruitful, as it is found that most Americans are generally "very ignorant" regarding such issues. Television gets the nod as the most effective source for further informing the public about world problems.

While such findings would seem to give a striking mandate to our federal government, the wise politician would do well to scrutinize the research very carefully. The patina of humanitarian concern seems to conceal a strong core of niggardliness. Almost half the respondents felt the U.S. is already doing more than it should to fight world poverty and doing less than it should to offset similar problems at home. When respondents were asked about their preferences for future budget allocations, the only items for which budget increases were desired by the majority were those concerned with domestic betterment; a decrease in economic assistance to foreign countries was preferred. Other responses reveal substantial antipathy to providing outright financial grants to the starving nations and prevalent agreement that if all foreign aid were to cease, the poor nations would learn to rely on themselves. Such facts are little stressed in the present work, and one begins to regret the passing of naive objectivity in social reporting. Surely there is an important distinction to be made in social inquiry between confirmation of ideology and a humble fumbling toward objectivity.

KENNETH J. GERGEN

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Cross and Sword: The Political Role of Christian Missions in the Belgian Congo, 1908–1960. By Marvin D. Markowitz. (Stanford, Calif.: Hoover Institution Press, 1973. Pp. 223. $12.00.)

Zaire is an unnatural state whose origins go back to the Congo Free State, where the Protestants gained an entry through Leopold II's intervention and became as zealous as the Catholics in emphasizing subservience and loyalty to *Bula Matari* (the state). The large companies involved in the exploitation of the central African area, notably the Société Générale de Belgique, the Union Minière du Haut Katanga, and Unilever favored the Catholics and placed restrictions on Protestant evangelization. Complaints to the Commission for the Protection of the Natives were generally useless because both Protestants and native Congolese were without any influence on this appeal board.

The Catholic missions were further advantaged politically by the backing of the Parti Social Chrétien in Belgium. Furthermore, the Congolese population often tended to see the Catholic missionaries as representatives of the colonizing power. These missionaries, many of them Flemish, tended to turn a blind eye to the exploitation of the local population, until in 1954 Minister of Colonies Auguste Buisseret began curtailing their excessive influence and forced the missionaries to turn to their Congolese converts for support against state "encroachments."

A frantic attempt was made to integrate the *évolués,* or assimilated élite, into the institutional framework. Professor Markowitz argues that the missions were at once innovative and conservative: innovative in having greatly accelerated social change in the Congo; conservative for desiring to preserve the foreign superiority over native cultures.

The study is based on thorough research in Protestant archives—notably the New York archives of the National Council of Churches, the Brussels archives of the Congo Protestant Council, the Kinshasa archives of the Congo Protestant Council—and in the Kinshasa archives of the Bureau l'Enseignement Catholique and of the Agence de Documentation et Information Africaine. The Vatican archives and the archives of individual religious institutes have not been consulted. This study is therefore much stronger in its consideration of the Protestant role than in its presentation of the Catholic positions. Compensating somewhat for this deficiency is a lengthy bibliography of newspapers, periodicals, pamphlets, and official bulletins. (It is very annoying to find the footnotes, which have been kept to the absolute minimum, are not always very informative, grouped at the end of the book, whereas explanatory notes have been situated at the foot of the pages.)

The author explains the limitations that were placed on his study by political developments in the Congo in 1964, i.e., the Kwilu rebellion, but he might have considered changing the title of his study, which has all the strengths and weaknesses of a traditional thesis, to "The Political Role of Christian Missions in the Lower Congo."

Perhaps the Christian concepts the missions tried to inculcate in the Congolese were better assimilated than the author concludes. One is rather heartened by the fact that African priests refused to apply for their "civilization cards," the *carte d'immatriculation,* contending that their educational and spiritual attainments spoke for themselves.

CORNELIUS J. JAENEN

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Soviet Naval Developments: Capability and Context. Edited by Michael MccGwire. (New York: Praeger Publications, 1973. Pp. 576. $23.50.)

Politics is concerned with power, and among the kinds of power, military force plays a major role. Because they can project power at a distance, and because they are traditionally used in diplomatic and influence wielding roles, naval forces would be expected to occupy a prominent place in the study of international politics. Yet this is not so. The scholarly literature on many intrinsically less important political subjects is more impressive in both quantity and quality than is the literature on naval affairs. Despite scattered efforts, such as those by Laurence Martin, Vince Davis, Robert Herrick, James Cable, Antony Preston and John Major, the scholarly inventory about naval affairs is depressingly limited.

This makes Michael MccGwire's *Soviet Naval Developments* especially welcome. Although the book is in the form of a collection of papers (from a seminar held at Dalhousie University, Halifax, in October 1972) it has more coherent form than is usual for an edited assemblage of papers. This is so partly because MccGwire, a retired Royal Navy Commander now teaching at Dalhousie, contributed more than one-third of the book himself, and partly because most of the contributors seem to share a similar frame of reference.

The focus of inquiry which provides continuity among the several authors is a search for the motivations that underlie the dramatic increase in Soviet naval power. Most of the contributors reject the classical thesis which links navalism with aggressive and expansionist goals. MccGwire's central thesis, which is shared by most of his authors, is that the Soviet Navy has developed primarily in response to the perceived nuclear strategic threat as embodied in the aircraft carrier and the Polaris submarine. This thesis explains the Soviet shift from a coastal defense strategy to a blue-water strategy as a response to the increasing range from which opposing navies—primarily the U.S. Navy—are able to attack the Soviet homeland. Not all of the contributors share this thesis, however. Robert Herrick, whose name has been almost synonomous with a "defensive mentality" explanation of Soviet naval strategy, has in his contribution here moved somewhat away from that interpretation. Herrick, in an analysis of the recent series of articles "Navies in War and Peace" by Soviet Navy Commander-in-Chief Sergey Gorshkov, places much greater emphasis upon an offensively minded interpretation of Soviet naval policy than he has done in the past.

Noteworthy in the book, particularly in MccGwire's own contributions, is a sensitivity to the problems of evidence and inference. In his search for the roots of Soviet naval policy, MccGwire succeeds in getting beyond the journalistic speculation which too often passes for analysis in the field of military affairs. Chapter 16, "The Turning Points in Soviet Naval Policy," is particularly well done. One relatively minor shortcoming is the failure to define some of the specialist jargon involved. Most readers may know that SAMs are surface-to-air missiles, but at least some are unlikely to recognize that CODAG refers to a main propulsion plant that combines diesel engines for cruising with gas turbines for high speed operations.

In summary, *Soviet Naval Developments* brings a higher standard of scholarship and analysis to the subject of naval affairs than has heretofore been the case. Given the importance of the subject, it is hoped that its appearance is indicative of a trend.

JAMES ALDEN BARBER, JR.

Department of the Navy

Soviet Politics and Society in the 1970s. Edited by Henry W. Morton and Rudolf L. Tokes. (New York: The Free Press, 1974. Pp. 401. Price unknown.)

Central direction, single-party rule, and an official class that stresses hierarchy, order, discipline, and deference—these are the features that characterize the Soviet political system today. Can this system continue to provide rising levels of material and physical security to its citizenry? Can it adapt to the demands of an increasingly complex economy and an increasingly differentiated and sophisticated populace, even as it ignores demands (from dissenters) for full democratization? *Soviet Politics and Society in the 1970s,* compiled in tribute to Professor John Hazard of Columbia University, touches upon these questions in ways that differ from the standard fare of readers in the field. As the editors point out in their introduction, "We have deliberately refrained from inviting contributors on such subjects as foreign affairs, military, cultural, and educational policies, Marxist-Leninist ideology, party propaganda, and the like" (xxiii). Instead, we are offered informative and, in some cases, highly stimulating, essays on the relationship between politics, economics, and social structure in welfare-oriented policy realms.

The volume contains articles on Soviet women (by Barbara Jancar), housing construction (by Henry Morton), crime and criminology (by Peter Juviler), consumer welfare planning (by David Cattell), and a unique case study of many aspects of local government in the Georgian city of Kutaisi (by Theodore Friedgut). Not all the chapters deal with questions of local government or social welfare, however. Rudolf Tokes provides a penetrating analysis of divergent trends within the Soviet dissident movement, their emerging ideologies, and the evolution of their relationship with the regime. David Albright documents the declining influence of the Soviet model of power seizure and social transformation on Third World non-ruling Communist parties. Paul Shoup deals with the similarities and differences among East European and Soviet paths of modernization, while William Taubman wraps things up with a critique of Western images and concepts about the future development and stability of the Soviet system.

One chapter deserves special mention as a masterful piece of analysis: Grey Hodnett's contribution on the relationship between technology, social change, and politics in Central Asia. Hodnett leads the reader through a complex maze of policy options and environmental constraints that Soviet leaders face in further developing the rural economy of Central Asia. It is a model of what can be done by the skilled analyst who examines policy and its implementation at the republic level and below.

The editors live up to their promise to avoid "semantic, culture-bound neologisms and intellectually barren model-building exercises" in favor of detailed exploration of "the live stuff of verifiable data on Soviet society and politics" (xxiii). The contributors have in most cases collected large quantities of evidence and displayed them in highly readable form. Most of the authors take pains to present a balanced view of Soviet accomplishments and shortcomings in the issue-areas under consideration. Advances in consumer welfare, housing construction, opportunities for social mobility, and in "modernizing" portions of the traditional non-Russian peasantry are duly recorded, and some contributors point to lessons that the West might learn from selectively emulating Soviet accomplishments.

At the same time, the reader of this volume has a difficult time swallowing one editor's introductory remarks about "the Soviet system's continued capacity for evolutionary change," and his belief that changes of the past decades demonstrate "the system's inherent flexibility and responsiveness to challenges posed by social and political change" (xxiv). Tokes may be correct in making these assertions, but the evidence displayed, and conclusions reached, by the contributors to the volume suggest that the future may be far more open-ended. To be sure, Tokes himself argues that the regime and the dissenters are likely to reach an accommodation (p. 41f), and Friedgut postulates that "collective Soviet values may well spread from the leadership into broader circles of the citizenry, providing a better-socialized and more-responding matrix within which to carry on the mobilization of citizen participation" (pp. 290-291). At the same time, though, several authors highlight some ominous problems for the not-so-distant future. Hodnett notes the possible fusion of economic nationalism and ethnicity in Central Asia, leading perhaps "to a more broadly based, self-conscious, and articulate nationalism than has existed in the past" (p. 107). Cattell believes that urban decay may overwhelm Soviet planners within a decade (p. 257); Morton notes that the Soviet housing program has been very hard-pressed to keep up with the rising expectations of the population (p. 186).

None of this is meant to support a "gravediggers of communism" image of the Soviet future. Clearly, each of these issues is likely to have a different type of impact on the legitimacy of the regime or its leaders. Predicting the future of any system is a notoriously difficult task, and it requires subtle notions about the components of stability, effectiveness, or efficiency, as well as an appreciation of the state of public opinion and the relationship between attitudes and behavior. What this volume demonstrates is that, despite their lack of fundamental political innovation, Soviet leaders of the past decade have adapted to a changing and more complex environment in order to attack certain technical issues in a more coordinated and self-conscious fashion. To demonstrate adaptation, however, tells us nothing about whether that adaptation has been sufficient to deal with emerging problems at a certain level of efficiency. Nor does it alert us to the impact of a given level of *in*efficiency on the legitimacy of the regime. (One could argue, after all, that most Soviet citizens will accept an authoritarian regime that provides them with the most incremental advances in welfare.) These are, of course, complex questions, but they are inescapable once one sets out to forecast the future stability of the Soviet system. Perhaps, at our current level of knowledge, more cautious conclusions are in order. Perhaps we should evaluate the proven capability of the Soviet system for dealing with problems of the past, but, for the present, set our sights on examining the changing nature of social, administrative, and political problems facing the regime. We will then be in a position to wait and see how well the regime adapts past policies and institutions to the new demands raised by its environments.

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Economic Analysis of Law. By Richard A. Posner. (Boston: Little, Brown and Company, 1973. Pp. 415. $16.00.)

Within the past few years a new school of jurisprudence has emerged, and since it uses the tools of modern economics it can appropriately be termed economic jurisprudence. Richard Posner's book, *Economic Analysis of Law,* provides a useful introduction to this rapidly growing field, surveying a great deal of the literature and integrating it into a unified framework. It should greatly interest political scientists and public law scholars in particular.

In an opening chapter Professor Posner holds that economics is the science of rational human *choice* and is not confined to the study of monetary matters. Law, too, involves rational human choice, and he enthusiastically sets about applying the theoretical perspectives of economics to the traditional doctrines and concerns of law. His purpose is both normative and positive. It is normative because he proposes to evaluate legal decisions, rules, and institutions in terms of their social *efficiency,* and to point out other more efficient alternatives. It is positive because he also seeks to explain how and why legal rules develop as they do. In both cases the underlying principle is social efficiency.

In undertaking this task, Posner reviews all the major fields of law. Like the legal realists he sets out to dip beneath the rhetoric of the legal opinion and come up with the "real" explanation for the legal *decision.* Chapters include discussions of torts, criminal law, contracts, property, regulation, taxation, business organization, problems of constitutional law, federalism, and a concluding section examines the logic of the legal process and ends with a note on jurisprudence.

In chapter 23, the heart of his book, Posner presents the argument that because the common law process operates in much the same way as the market, it is an efficient device for social choice. He argues that the common law, with its independent and disinterested judiciary, its fragmentation of decisions, and its ability to respond to slight changes in preferences, is a finely tuned mechanism that can carefully express social preferences and maximize social utility. In contrast, the legislative and political processes are much more susceptible to socially inefficient preferences.

Two problems emerge from this analysis: First, others have argued that the judicial process is singularly ill equipped to register and consider all the costs and benefits that might flow from a particular decision—indeed, that it is impossible for a passive judiciary to identify all the external effects of its actions. Therefore, the argument continues, the courts should limit their activities and defer to the better equipped political processes. Posner's contention that judges act as *agents* for society to maximize net social utility is undeveloped and as presented is too facile, and in certain aspects it challenges the basic assumption of self-interest. While a tradition of disinterestedness, competition, decentralization, and incrementalism may help Posner explain why judges are so efficient, so too might the looseness of his analysis. Despite his seemingly impressive framework, much of his analysis comes down to *post hoc* explanation. There are no testable propositions or carefully drawn theories. What "explanatory" statements are offered are usually true by definition, not deduction. The author's explanation of the development of the various fields of the common law, therefore, provides no more convincing an account or complete an understanding than do the sociological jurists' discussions of balancing interests. And, like the sociological jurists, his work suffers from the same confusing mixture of positive and normative analysis.

Second, Posner tends to examine each rule of law in isolation to see if it is socially efficient. This type of analysis might explain why he seems to have more respect for the judicial process than the legislative process. The legislative process is probably best understood and evaluated by examining *bundles* of actions, not each provision independently. Some "unanimous consent" models of the legislative process, for instance, hold that while individual legislative actions may be suboptimal, taken together over the long run and with reliance on log-rolling, side-payments, and vote trading, a set of decisions by an electorate or representative body may be socially efficient. Posner's rule-by-rule analysis leads him to ignore such arguments in his discussion of the legislative and political process, perhaps explaining his preference for the judicial process.

One of the primary attractions of an economic approach is that theory is built within a closed and carefully defined system. Posner's discussions, however, frequently do not proceed in this manner. Much of the analysis is too casual and openended, and consequently it loses much of the persuasiveness of the very approach it is promoting. All in all, however, the book is an important and welcome addition to this growing field, provides a useful introduction to some of the field's more technical discussions, and can profitably be read by students of public law, jurisprudence, and political theory. While they will find much to quarrel with, they will also find much to build on.

MALCOLM FEELEY

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A New Era in Competition. Edited by T. M. Rybczynski. (New York: Barnes and Noble Books, 1973. Pp. 105. $5.00, paper.)

The eight lectures in this slim collection, delivered at the Society of Business Economists' Conference in Cambridge, England, April, 1972, examine international and domestic responses to a new era in competition. T. M. Rybczynski suggests the ramifications of this theme: "There is no doubt that the new competitive pressures have been all-embracing and all-pervasive. They have become very marked internationally and domestically; they have affected all types of activities, industry, commerce and finance; and they have covered all types of enterprises, be they large, medium-sized or small" (p. vii). The essays attempt to evaluate the dimensions and impact of this trend toward competition. Yet since there is no agreement at the outset on common terms or scope, the theme of competition remains ambiguous and obscure. The reader must search for the unifying issues from individually informative, but disparate, essays.

The first four essays focus on competitive developments at the international level. Two of these essays analyze the role of international institutions in stimulating or moderating competitive pressures. Herbert Giersch of the Kiel Institute of World Economics speculates on changes in competitive conditions coming from Britain's entry into the European Economic Community. New competitive pressures will provide a positive stimulus to economic growth. The enlarged EEC will have to make structural adjustments, develop a common regional policy, and support policies favorable to developing nations. This argument, advanced in 1972, assumed a smooth British entry into the EEC and an economic upswing. Giersch's optimism has proved ill-founded. C. H. Sharpston of the United Nations Industrial Development Organization explores the possibility of moderating competition for the benefit of developing nations. With rules of the competitive game hampering industrialization, developing countries are relying on more cooperative policies to temper international competition.

Two essays discuss competition from the perspective of the business community. G. Manca reports on the efforts of Pirelli to develop long-range planning and forecasting techniques to identify societal trends. The firm found tensions arising from incomplete socialization of youth, environmental deterioration, and poor working conditions. Such factors will determine the development of industry in the next years. Ronald E. Geesey of the First National City Bank examines the response of international banking services to the competitive pressures of the market. These case studies have limited utility since other institutions have offered sophisticated societal projections, and all viable enterprises have adjusted to changing patterns of competition.

Four essays focus on aspects of competition in the United Kingdom. The most informative is Kenneth D. George's contribution on the changing structure of industry in the U.K. He carefully traces increasing industrial concentration. By comparing the output and productivity growth rates for the U.K. and West Germany, he concludes that the low rate of growth has been a weakness in maintaining the competitiveness of British industries. Several recommendations are proposed to improve the British position.

The last three essays assess particular characteristics of competition in British industry. Harry Townsend of the University of Lancaster investigates the lack of internal competition in British industry and the predicted external competition stimulated by the EEC. Likewise, D. C. Hague of the Manchester Business School evaluates the state of competition and finds oligopoly instead of competition, satisficing instead of optimization, and organizational slack instead of efficiency. He suggests new institutional forms to provide alternatives to competition: quasi-governmental and quasi-nongovernmental organizations new amalgams of the public and private sectors. Finally, L. V. D. Tindale traces the financial history of the City of London taking into account a variety of institutions, merchant banks, finance houses, Building Societies, insurance companies, and investment trusts. These interpretive essays are primarily of interest to those specializing in business economics.

This volume might have been strengthened by a concluding chapter which integrated the domestic and international, the economic and political aspects of competition. How does domestic economic competition impede or facilitate international competition? What is the impact of international competition on domestic politics? How are economic factors of competition affected by political variables? How do political manifestations of competition affect economic relationships? These questions are of critical importance to the political scientist and the political economist, yet they remain unasked and unexplored in this unfocused, often disjointed, volume.

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Principles of International Law Concerning Friendly Relations and Cooperation. Edited by Milan Sahovic. (Dobbs Ferry, N.Y.: Oceana Publications, Inc., 1973. Pp. 450. $12.00.)

The aim of this collection of eight essays is to present an in-depth exposition of Resolution 2625 (XXV) of the General Assembly of the United Nations which marked the first major step in the progressive development and codification of the seven cardinal principles governing friendly relations and cooperation among nations. These seven principles were identified in 1962 in General Assembly Resolution 1815 (XVII) as the legal basis of coexistence and assigned to a Special Committee for further study with a view to their progressive development and codification. That work culminated in 1970 in the "Declaration on the Principles of International Law Concerning Friendly Relations and Cooperation among States in Accordance with the Charter of the United Nations," adopted by means of Resolution 2625 (XXV).

Each of these seven principles is the subject of a separate essay by an individual author. The eighth essay is a study by the editor on the genesis and legal significance of the Declaration of 1970. As the seven principles and their official definition in Resolution 2625 (XXV) vary widely in scope and impact, the different essays had to cover, and cope with, distinct types of subject matter. Yet, the structure and approach of the collection is amazingly uniform and harmonious. The actual elaboration of the content of the seven principles was in the nature of a series of political compromises. Hence a careful assessment of the forces and interests which resulted in the ultimate formulation of the various aspects of each principle is an essential element of a meaningful analysis. The authors of the eight essays were fully cognizant of this need and made excellent use of the records and of the reports of the Special Committee. All essays, in addition, strive to discuss the newly crystallized norms in the light of divergent approaches to international law, as developed by writers belonging to nations adhering to different political and economic philosophies. Generally the presentations are scholarly and nonacrimonious. Only once (in the essay on Sovereign Equality of States) is there a polemical reference to the "bourgeois political and legal system" as an object of solicitude of "traditional" international law. As an overall objective the authors seem to aim at a strengthening rather than weakening the viability of the Declaration. Certainly the work should be of interest to all who attach significance to the Charter of the United Nations and formal actions of that body designed to give that document life and content.

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The Cybernetic Theory of Decision. By John D. Steinbruner. (Princeton, New Jersey: Princeton University Press, 1974. Pp. xiii, 366. $14.95.)

The Sciences of the Artificial. By H. A. Simon. (Cambridge, Massachusetts: The MIT Press, 1969. Pp. xii, 123. $2.45.)

Systems theory arose in the Department of Defense as a fancy expression of that immemorial fiscal creed—buy cheap and sell dear. Now writers with an ineradicable ache for the most various of technical pursuits are regularly described as systems theorists; what unites them, aside from a plangent concern for the interconnectedness of things, is an inclination to reject as intolerably partial and dissective the methods and models of classical physics and a barely barbered enthusiasm for such ripely comic subjects as cybernetics or the theory of artificial intelligence.

In *The Sciences of the Artificial*, for example, H. A. Simon describes the "design of an intellectual structure aimed at accommodating those empirical phenomena that are 'artificial' rather than 'natural,' " (publisher's blurb); such efforts, at any rate, dominate three of the book's four chapters. The last essay, "The Architecture of Complexity," was not delivered as part of the Karl Taylor Compton Lectures at MIT and appears in this book as an editorial afterthought. (Published in 1962 as part of the *Proceedings of the American Philosophical Society* [106, 467–482], it offers rather a gelatinous discussion of such topics as the theory of complex and hierarchical systems and the Simon-Ando theorem on dynamical systems; readers looking for a trustworthy trot to the technical material would do better to consider Fisher and Ando's "Two Theorems on *Ceteris Paribus* in the Analysis of Dynamical Systems," *The American Political Science Review* [56, 108–113, 1962].) By systems that are artificial Simon means systems that are *goal directed* or *purposeful*. These are objects of considerable internal complexity, but, Simon argues, "the possibility of building a mathematical theory of [such systems] or simulating [them] does not depend on having an adequate micro-theory of the natural laws that govern the system's components" (p. 19).

Instead, the theoretician need attend chiefly to the *environment* in which the system is situated: the system's theory will hinge on only the most modest of physical assumptions about the system's structure.

There is no arguing with this in the abstract, of course, but the specialization of the thesis to human beings—the theme of chapter two—is quite unconvincing. There Simon asserts that "a man, viewed as a behaving system, is quite simple. The apparent complexity of his behavior over time is largely a reflection of the complexity of the environment in which he finds himself" (p. 25). This is a position that he reaches by meditating on insects. The course of his argument involves the following steps. To consider an ant simply as a "behaving system" is to consider it purged of internal structure—that is the province of biochemistry. Simulating its gross behavior, as it scurries along the surface of the earth, involves only the application of a few principles dealing with the calculation of local optima and the like. What holds for the ant holds for men, Simon believes, at least if the analogy is carried to human beings considered simply as cognitive devices. Nor is the point limited to human beings: *all* systems that think are quite simple; elementary information processing principles explain their behavior (as well as the behavior of such concocted items as the digital computer). This is not behaviorism, of course, since Simon recognizes that human beings process information by means of principles that are internally represented and in any case innate, but it is a view comfortably within the empiricist tradition, and Simon's chief claims would, I dare say, be accepted as a natural point of convergence by other researchers working on artificial intelligence and theories of information processing.

What of the evidence for these conclusions? For the most part, Simon draws on fairly well-known (and none-too-recent) work in the analysis of short term memory; there is some evidence too from work in the simulation of crypto-arithmetic abilities and the capacities involved in the memorization of nonsense syllables and the organization of visual memory. A taste for the friendship of his natural enemies also leads Simon to the utterly improbable conviction that his views are sustained by the work of transformational grammarians. Although "the picture will continue to be enlarged and clarified," he avows, "we should not expect it to become essentially more eomplex" (p. 53).

The skeptic, however, will observe that this quotation reflects a degree of intellectual optimism made possible only by its author's unquenchable enthusiasm for the inflation of trivialities. An achievement such as the creation of an information processing model (the so-called EPAM program) by which the memorization of nonsense syllables may be mimicked hardly suggests a perfect understanding of human intellectual capacities that are either characteristic or deep. The insights that Simon records about the organization of short term memory, while genuine, need to be set against the relatively bleak panorama provided by our ignorance of the principles that govern long term human memory. In this regard, one might consider Patrick Suppes's perceptive and pessimistic essay, "On the Theory of Cognitive Processes," in his *Studies in the Methodology and Foundations of Science* (Humanities Press: New York, 1969). Concerning the massive central capacities of the human brain—capacities involving the use of language, the construction of scientific theories, the ordering and manipulation of highly abstract objects and the discovery of significant relationships among the systems of natural numbers—almost everything is dark and the idea that computer programs exist that can in any reasonable sense simulate these powers is quite simply preposterous.

Thus when Simon asks, *even of the ant,* how different its behavior would be from that of an automaton with "the approximate dimensions of an ant . . . provided with a few adaptive capabilities," the obvious answer, considering what we know of the complexity of insect behavior, is "utterly." Even if we accept for purposes of idealization an ant of the most ruthless simplicity, simulating its behavior involves at least the following: that a program be designed capable of orienting an object along an arbitrary manifold while computing maximum or minimum values in indefinitely many dimensions.

Were the accomplishment of such a program trivial, tank warfare could be conducted without men. In fact, state-of-the-art techniques barely extend to systems of fully automated trains. And plainly, the human brain is not simply more complex than the system that powers the ant but of an entirely different order of complexity.

Simon goes wrong straight off in his notion of an ant taken simply as a *behaving system,* a characterization he introduces to separate the internal complexity of the ant's biochemical organization from what seems to be the external simplicity of its physical behavior. No doubt, one can come to the theory of the ant without knowing much by way of its biochemistry, the arrangements of macromolecules and the like. But what remains when the physical substructure is subtracted from the organism as a whole is hardly *just* a behaving system, if indeed that phrase has any sense at all. The engineer draws a threefold distinction among inputs, outputs, and the structures that connect them. A *system* he dubs the most general of devices taking inputs to outputs by means of a set of states. The organization of a system's states provides a sense of its structure. Now even if the biochemistry of the ant is hopelessly obscure, having a record of what the ant does we yet require some account of the organization that explains its performance. Were the ant simple as a rheostat or filter, this would take the form of a system of ordinary differential equations. Uncomplicated as the ant is, at least by comparison with the human being, nothing at all suggests that the structures mediating between inputs and outputs are intellectually accessible, pliantly simple or, in fact, any less complex than those structures described by purely biochemical or physical theories. Thus when Simon writes that an advantage to "dividing outer from inner environment . . . is that we can often predict behavior . . . with only minimal assumptions about the inner environment" (p. 8), his point sticks just so long as the phrase "inner environment" is taken to describe purely the physical or biochemical substructure of a complex system. If the inner environment of a system is understood simply as its structure, in the sense of system's theory, then there is no reason to believe that "minimal assumptions" will generate anything save trivial theories.

It was at Stanford, Professor Steinbruner recounts, that he was "sensitized to the empirical and epistemological problems encountered in analyzing the processes of perception" (p. xi); the result, issued some years later, has been his fattened doctoral dissertation, *The Cybernetic Theory of Decision.* It is a curious book, split almost perfectly into two halves. The first discusses the "Paradigms of the Decision Process," the second, "The Politics of Nuclear Sharing." As far as I can see, both halves function independently, like the valves of a bivalve. Part Two, I would guess, will appeal most strongly to political scientists for there, in workmanlike narrative fashion, Steinbruner describes the development of nuclear sharing proposals between the United States and its European allies during the years from 1956 to 1960 and the rise of the Multilateral Force in years just after. Part One treats of decision theory and occupies itself with what Steinbruner calls the *analytic, cybernetic,* and *cognitive* paradigms. By the analytic paradigm, Steinbruner means classical decision theory; the cybernetic paradigm is commended in its place because decision theory offers no plausible account of complex behavior. Consider, as an example, the ordinary tennis player *(homo blaatus):* "Given what we know about the pace of the game and the intellectual characteristics of tennis players, it seems intuitively implausible that the player makes his moves by estimating the speed and trajectory of the balls and by computing the probable intersection point given his own rates of movement" (p. 49). Yet the classical paradigm has the tennis player toting up preference rankings and estimating utilities even as he lobs: by Steinbruner's reckoning the most apposite stroke in a typical exchange lies lodged in a sample space of some 4200 possibilities, and calculating correctly enough to distinguish it from the senseless smash or the infuriating wood shot involves computational powers improbably attributed to most players. What is needed instead are mechanisms that "clearly solve problems of impressive difficulty . . . with apparently little burden on the decision maker" (p. 51).

It is the science of *cybernetics* that provides just the right item: "The simple mechanism, which the . . . tennis player . . . require[s], can readily be conceptualized in terms of a servomechanism, a central cybernetic idea" (p. 51). Concerning such servomechanisms, Steinbruner is none too clear. He mentions the Watts Governor and the ordinary thermostat as examples. These are servo-mechanisms in the sense that their output, thinking of them now as systems, is matched against set standards, with deviations triggering an error signal. Inputs are then chosen to minimize the difference between actual and intended outputs. This is control through feedback. The most common and best understood of engineering theories treat of feedback systems that are quite simple: first order, generally, and with but a single input, the governing equations homogeneous and constant. Linearity prevails throughout.

Such systems, of course, can do nothing by way of simulating complex behavior. A Watts Governor does not play tennis. Nor is this a point to which Steinbruner is especially sensitive. Take his discussion of the B-71 bomber, purchased by the Air Force in the early 1960s, over Department of Defense objections. Analytic planners, Steinbruner believes, resisted the pitch offered by the Air Force because the "marginal investment in weapons" might more profitably have gone elsewhere, to tanks, perhaps, or stupendous bacteria; indeed, planners under the influence of the analytic paradigm considered Air Force covetousness for the B-71 as evidence for deep running behavioral irregularities. The "cybernetic theorists," however, understands what was at issue: "In his terms, the Air Force officers [were] clearly focusing decisions on a few key variables for which there [was] information feedback (the concomitant characteristics of the enemies' forces)" (p. 85). Thus on the cybernetic view, the right explanation for the decision to purchase the B-71 bomber is largely mechanical: far from measuring the marginal utilities of various weapons systems, Air Force procurement officers arranged themselves as instruments for imitative purchasing.

But had the Russians *reduced* their Air Force expenditures in, say, 1963 or 1964, if American Air Force officers were behaving even as the Watts Governor, American expenditures ought also to have decreased. This is plainly absurd. However ill considered the actual plan to construct the B-71, it was not undertaken to track the Soviet system in any *simple* fashion: a Soviet Air Force decision to expend funds in a way which suggested the graph of the cosine function would have prompted American officials to observe simply that their counter-parts in Moscow had taken leave of their senses. This ability is quite beyond the capacity of elementary servomechanisms and affords some evidence that Air Force officers are not just "focusing decisions on a few key variables for which there is information feedback" (p. 85).

Unless, of course, one means something more sophisticated by the subject of servomechanisms. There are adaptive control systems and systems that optimize their behavior. Instead of attributing to the free-swinging tennis player or the B-71 besotted procurement officer a calculus of rational decision, one might perversely insist that they both embody adaptive systems of optimal control.

Now we can think of a problem in optimal control theory as a whole composed of at least the following parts. There is, first of all, a *dynamical system:* the equations that describe it organize the system's states. Then there is a *target* or *target set*, which corresponds to the states that the system is meant to achieve. *Admissible controls* are specified, along with a set of *initial states* or *events* and a *cost of control functional* or *measure.* The problem of optimal control is to determine for each initial state or event an admissible control that takes the system to a target set and in so doing *minimizes* the cost of control functional.

Plainly it might be possible to describe complex behavior as control theoretic in the sense that one attributes to an agent the structure of an abstract control system; instead of computing consciously those decisions that maximize utility (or expected utility) such agents, given some prior specification of appropriate target sets, carry on rather a mechanical hunt for optimal controls.

But *both* decision-theoretic and control-theoretic models attribute to agents or organisms exhibiting complex behavior vast powers of optimization or maximization. Executing such behavior will involve strenuous computations: the numerical methods involved in steepest descent or gradient methods are rebarbative. Systems that are adaptive as well as optimal must be arranged so that in effect they guess at the solutions to differential equations.

If one rejects the decision-theoretic model on the grounds that it attributes to ordinary agents calculating powers that they could not command, the same scruples should count against such exotica as optimal control theory.

Perhaps this is why Steinbruner actually gives the cybernetic paradigm fairly short shrift, dismissing it in favor of those other standbys, the analytic and cognitive paradigms. It is a move that I applaud, but having noted it, one wonders what is left of the book's central theme.

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The United Nations and the Population Question. By Richard Symonds and Michael Carder. (New York: McGraw-Hill Book Company, 1973. Pp. 224. $8.95.)

It could be said that against a background of elaborate preparation, high cost (the U.S. alone contributed an estimated $5 million), and high-powered advance publicity—all reflecting the hopes of its organizers for a meaningful commitment by developing countries to control their excessive population growth rates—the latest World Population Conference (Bucharest, August 1974) produced a mouse. A two-week-long meeting replicating the political dynamics associated with a U.N. General Assembly session yielded a World Plan of Action, which provided minimal commitment for action by anybody, but rather constituted a vehicle by which the particular ideological positions of everybody, reduced to the most consensual denominator, were expressed in a format acceptable to all.

A prior reading of the Symonds-Carder volume would have provided reliable grounds for forecasting the scenario, dynamics and outcome that evolved at Bucharest. The authors have produced an historical analysis, dating back to the League of Nations period, dealing with the legitimization of changing world population characteristics as a discrete item for the agenda of the international system, and recounting what happened to the item once it was there, and why. Although their treatment is largely descriptive, and (measured against the accepted canons of academic political analysis) atheoretical, Symonds and Carder have turned out a valuable work because their historical treatment is imbedded in a political framework, which deals with the role of political variables such as ideologies, national interests, elite roles, organizational conflict, and bureaucratic behavior, in determining the treatment of population-related issues by the organized international system.

The major themes that have accumulated in the contemporary discourse on population policy—issues such as birth control, maternal/child welfare, dependency, disposition of excess populations, adequacy of food supplies, and the relationship of development and population growth—are tracked across a half-century time span. Symonds and Carder relate these issues to the international forums and agencies in which they were handled, and describe the role of national political and ideological biases in determining the quality of handling, and the outcomes, in the international institutions.

The work would have been a more interesting and valuable contribution to policy analysis had it dealt more in analysis and less in description. The detailed accounts of debates in the various forums, including numerous direct quotes of participants (whatever their merit, remarks by Malian and Voltaic representatives are probably marginal to any outcome), and information on the duration of debates, become somewhat tedious. The authors observe that much desirable archival material still is not available to researchers, but it is doubtful that such data would have added more than marginal light to the analytic enterprise. Many of the participants in the events and decisions, however, are still alive (Glass, Hauser, McNamara, Sauvy, Sen, Henderson, Rockefeller), and interviews with them, focused on key decisions and actions, would have yielded more valuable data for policy analysis than does the written record. The acquisition and utilization of such data might also have contributed to the substantiation of occasional assertions about cause-and-effect relationships, e.g., that Sen's intervention tilted the U.S. government to population control strategies (p. 132); or that the World Bank became involved because of the breakthroughs in WHO and the U.N. (p. 158).

Finally, Symonds and Carder might have speculated on what the population experience reveals about the capabilities and comparative advantage of the U.N. system for managing development programs. Conventional wisdom has long held that development activities are more tolerable when channeled through international institutions, and this untested assumption is increasingly influencing the deployment of American foreign aid. But both in the areas of acceptability and performance, there is no definitive analysis of the comparative advantage of aid provided through multilateral channels. Symonds and Carder identify operational difficulties in the U.N. system (pp. 192–94), but they resist speculation on whether these constraints suggest decreasing, rather than increasing, reliance on the U.N. system.

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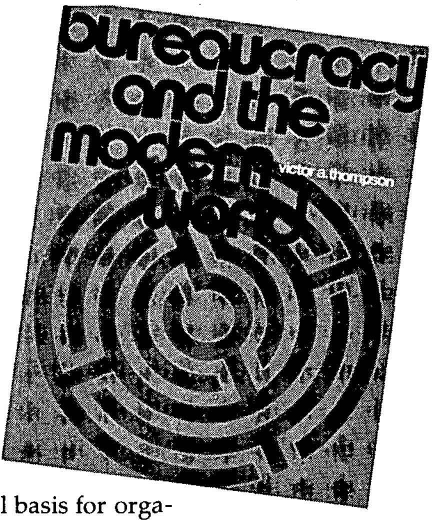
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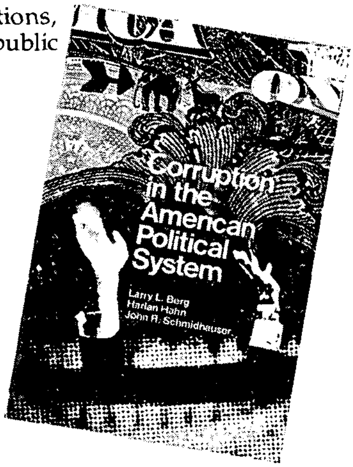
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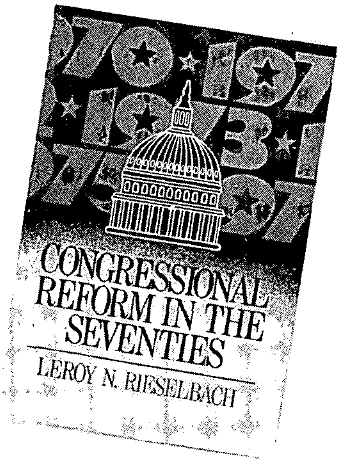
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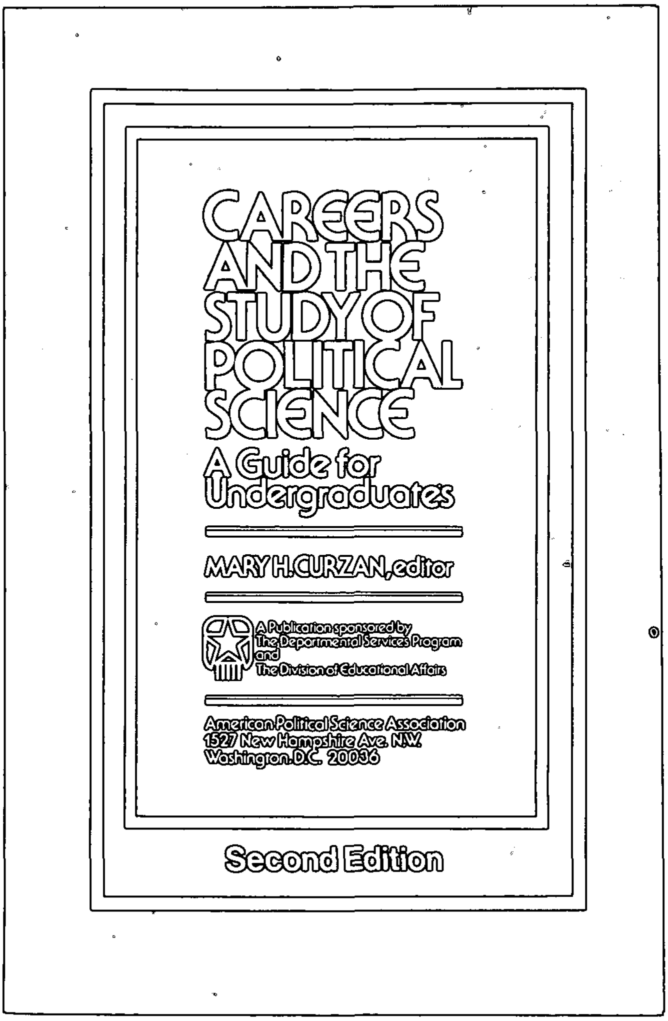

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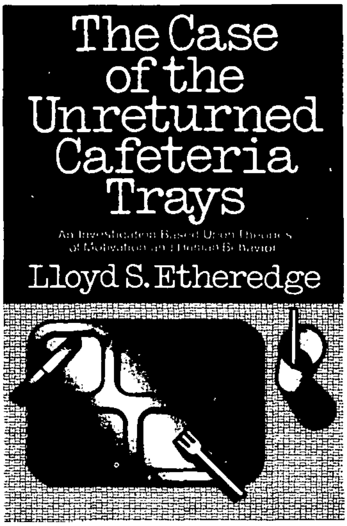
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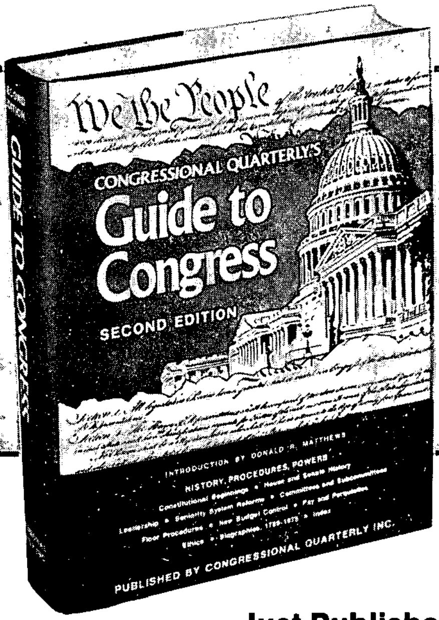
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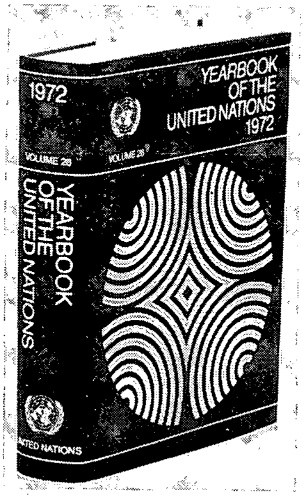
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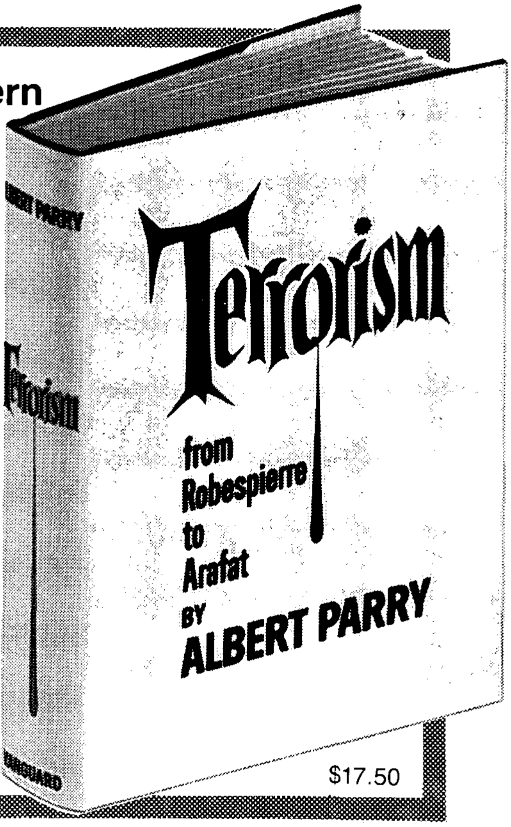

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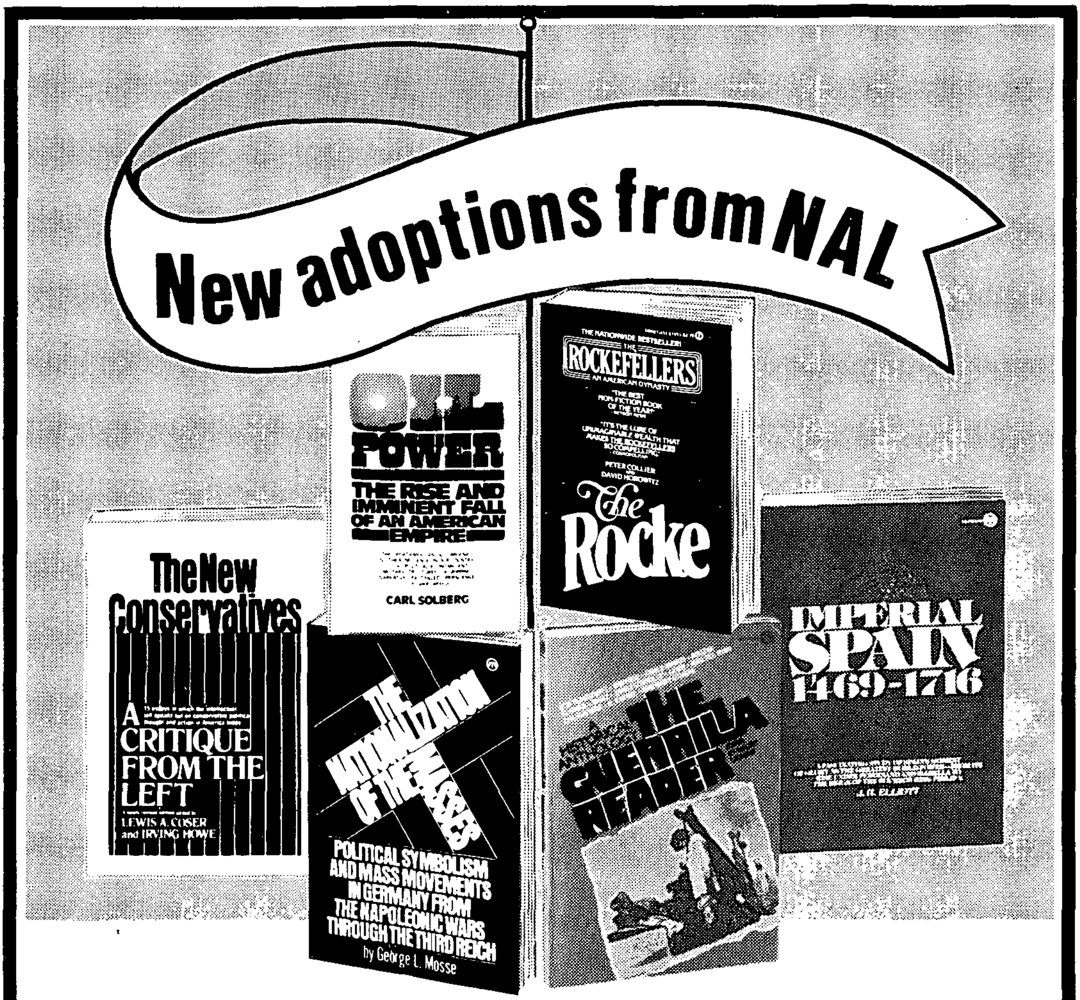
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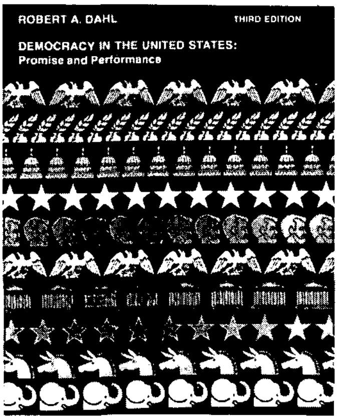
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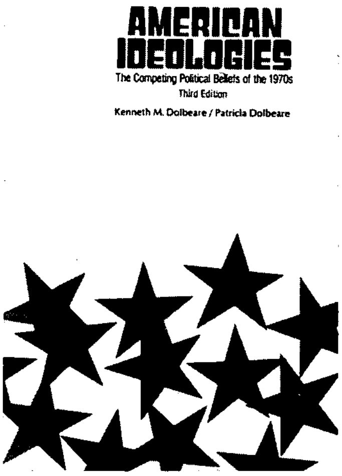
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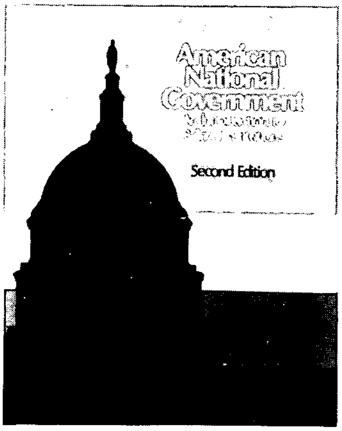
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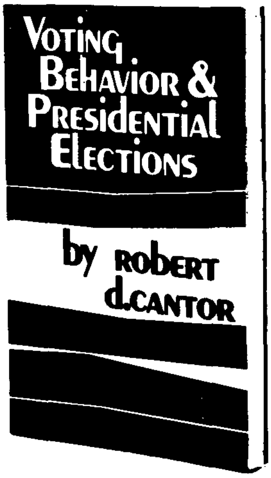

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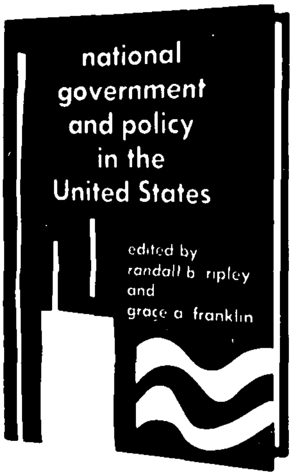

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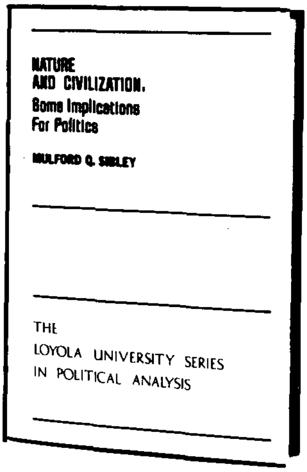

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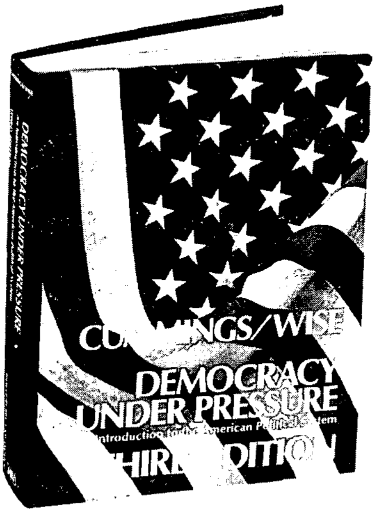

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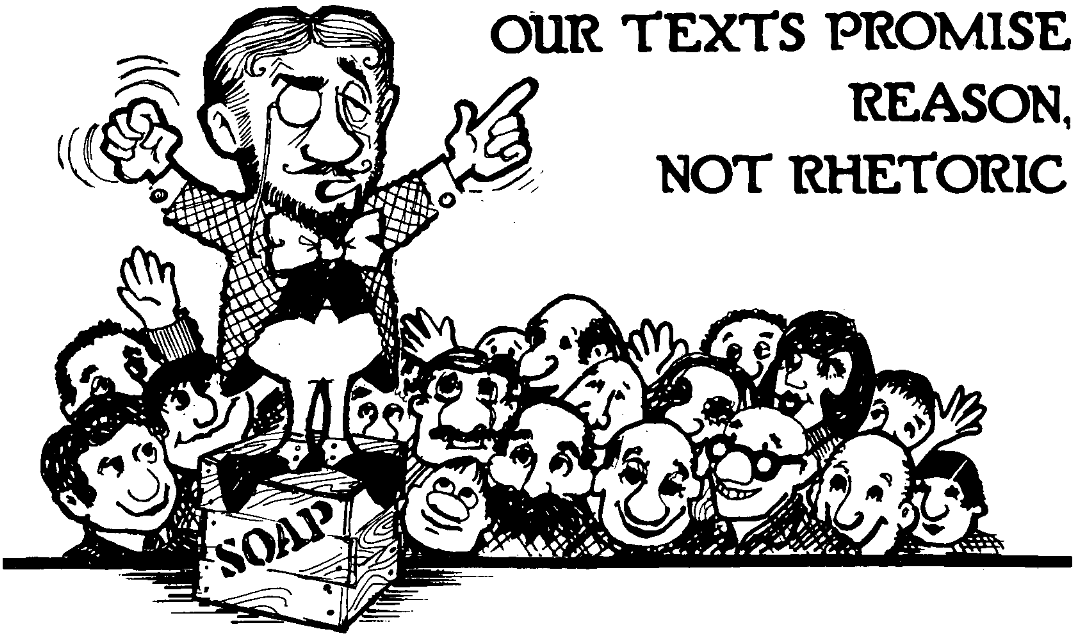

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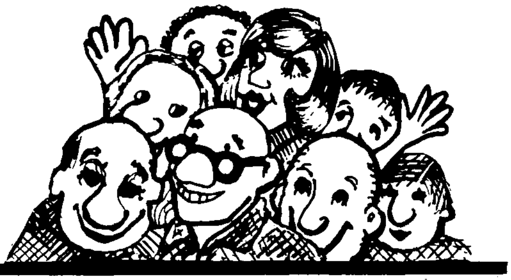

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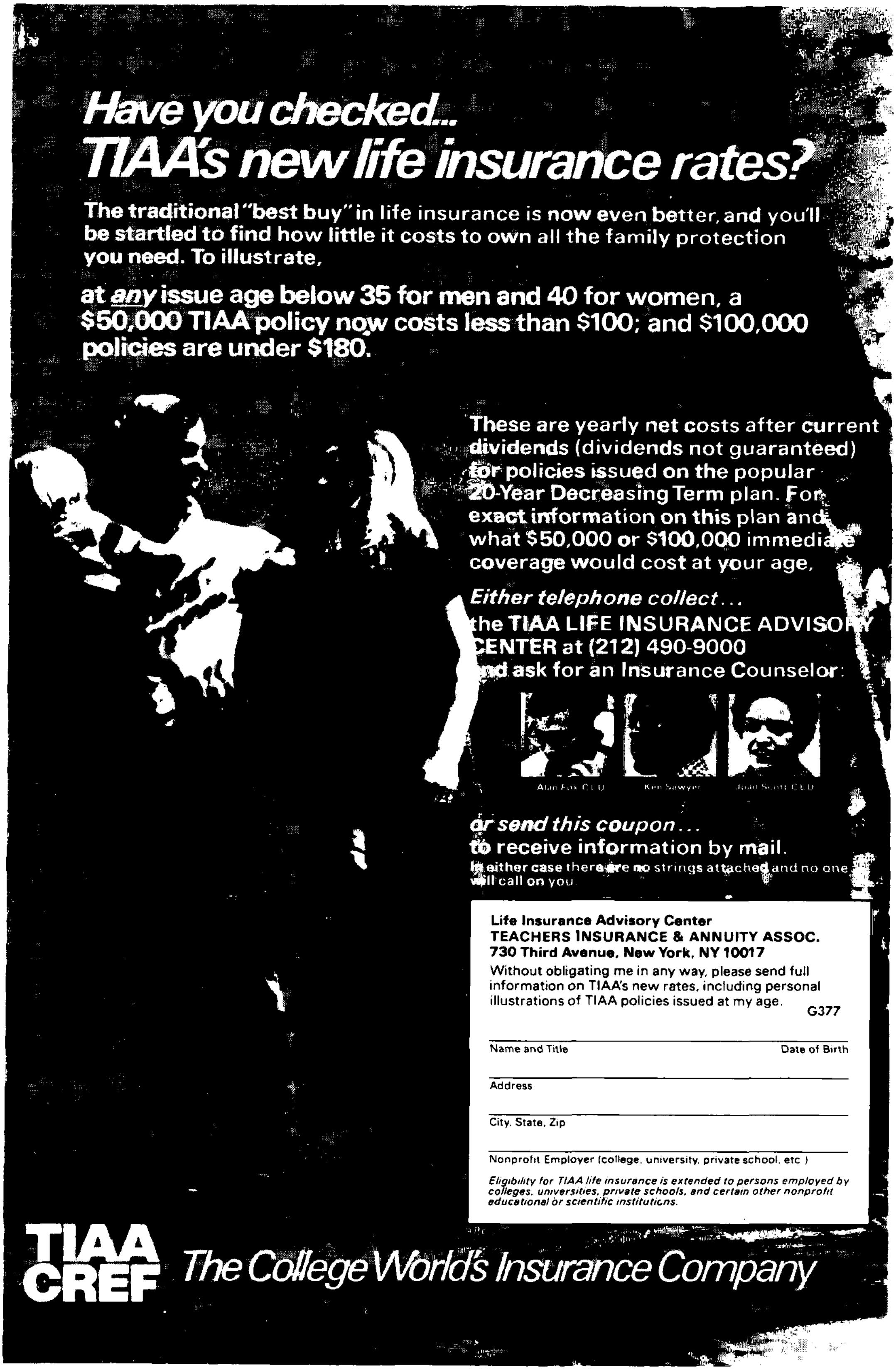

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